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THE PRINCIPLE OF INTEGRATION IN OBJECTIVE PSYCHOLOGY

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For the objective psychologist the problem of integration presents at once a peculiar difficulty and a peculiar cogency. If we accept Weiss' definition[1] that "Behaviorism is the science that studies the origin and development of those bodily movements of the organism which establish its status," it might appear that we have a range of inquiry which, whether feasible or not, is at least unmistakably delimited. Yet a cursory glance at some of the fundamentally divergent opinions possible within the compass of this definition convinces us that such is not the case. We have a group of workers who would explain behavior as due to paths of low resistance among the neurones, and another which would deal with it in terms of elaborations of unitary reflexes. A more obvious difference is that of Calkins, who would find the source and cause of the "origin and development" of the movements in question in an intrinsically active self, and who has recently termed her variant on the psychology of Bosanquet and Ward "truly psychological behaviorism."[2] Now it will be observed that these differences all gather about a central point, namely the problem of integration. In effect the question which is raised is this: What is the principle of synthesis that lies at the centre of organic behavior and gives it its distinctive character? It is our purpose in the following pages to present a critical review of opinions on this point, and to offer some constructive suggestions.

1. *Neurological Associationism*

According to this view the functioning of the nervous system is to be regarded as the essential integrating factor in organic

[1] A. P. Weiss, Behavior and the Central Nervous System, *Psych. Rev.*, 29, 1922, 330.

[2] M. W. Calkins, The Truly Psychological Behaviorism, *Psych. Rev.*, 28, 1921, 1 ff.

behavior. It is claimed that if we wish to understand the fundamental nature of instinct, habit, affection, emotion, and so forth, we must by various means analyse down to the neural patterns involved. Of course it is not denied that behavior involves receptor and effector structures, but it is urged that if we wish to find its determining characteristic we must look to the connective mechanism. Thus psychology becomes a study of the central nervous system. Warren[3] has even advanced the suggestion that here we have a means of reconciling the subjective and objective points of view, since the one deals largely in terms of receptor action, and the other largely in terms of effector action, while the nervous mechanism brings together both receptors and effectors and invests the organism with its powers of specific response.

Warren's exposition in the 'Human Psychology' is the most systematic presentation of this point of view before us. But the general tendency towards a neurological associationism is very widespread and influential. Apart from more extended treatments, there are many papers which exhibit neural constructions as a basis for psychological phenomena,[4] and very frequently monographs and special studies offer more or less tentative suggestions as to what may go on in the nervous system to account for the observed facts of mental life.[5] It is not at all important to canvass the whole of this scattered literature to estimate its general tendency. The point is that there seems to be a prejudice among psychologists in favor of neural constructions as solid and valuable explanations.

Such a prejudice has perhaps been rendered almost inevitable by two sets of influences. On the one hand we have a psychophysical parallelism which tends to treat consciousness as an epiphenomenon. And when consciousness itself is challenged—and such challenges have become fashionable in modern psychology—then at once explanations in terms of the physical series take on a special appearance of validity. In other words, neurological associationism is the logical descendant of the notion of a neural correlate. Then on the other hand we have

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[3]H. C. Warren, Psychology and the Central Nervous System, *Psych. Rev.*, 28, 1921, 249 ff.

[4]As instances consider the following: M. Meyer, The Nervous Correlate of Pleasantness and Unpleasantness, *Psychol. Rev.*, 15, 1908; The Nervous Correlate of Attention, *ibid.*, 16, 1909; L. T. Troland, A System for Explaining Affective Phenomena, *Journ. Abnorm. Psychol.*, 14, 1920; F. H. Allport, A Physiological Genetic Theory of Feeling and Emotion, *Psychol. Rev.*, 29, 1922.

[5]For instance, see the comments in J. E. De Camp's *A Study of Retroactive Inhibition*, and E. C. Tolman's *Retroactive Inhibition as Affected by the Conditions of Learning*, where the neurological suggestions are almost ludicrously inadequate to the complexity of the facts presented.

a very striking psychocephalic parallelism developed by the neurologists themselves, who usually seem innocent emough as to all the critical difficulties of such a position.[6] Now it will not be questioned that the nervous system plays an essential part in ordering and centralising all organic functions, and thus it is natural to think of such factors as reflexes, instincts, habits, emotions, and so forth, in terms of the functioning of the central nervous apparatus. But we question whether the fact validates the interpretation. There are serious difficulties in the way of treating the ability to form and preserve neural patterns as the essential integrating principle in behavior, and regarding objective psychology as a study, first of all, of the central nervous system.

In the first place, it should always be explicitly understood that any explanation of behavior in terms of the formation and functioning of neurograms is hypothetical. A typical and complex constructive effort in this direction[7] begins by urging that anatomical detail is not necessary for our comprehension of neural function as a ground for psychological explanations. The problem of the work is defined as follows: "What are the simplest assumptions necessary and sufficient to explain hypothetically the facts of human behavior as dependent on the functions of the nervous system?"[8] There is of course no objection to the legitimate use of hypothesis in science, but unfortunately the programme as thus defined leads us quickly out of touch with reality altogether. It is instructive to glance at a few of the constructions which have been involved. First we find several theories which begin to build up from the assumption of modal or qualitative differences among the energies of various nerves. Such assumptions are not merely without any foundation in ascertained fact, but are directly opposed to the findings of neurology as far as these go in this direction. Again in McDougall's view of inhibition by drainage[9] we have a piece of ingenuity which is to be criticised as entirely unverifiable. The concept of the synaptic planes as one-way valves is on a somewhat different footing, for there is some respectable factual

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[6]To select two out of a multitude of possible dicta consider the following. Bechterew (*La psychologie objective*, 6) explains that he uses the term "psychic" in speaking of neuropsychic processes with a view specifically to "les processus cérébraux qui en forment la base." This is the consistent point of view of the book. Again, Sherrington (Mechanism of the Mind: *London Times*, September 7, 1922) writes "The cortex of the forebrain is the seat of mind."

[7]M. Meyer, *The Fundamental Laws of Human Behavior*, 1911.

[8]*Op. cit.*, xv.

[9]W. McDougall, *Physiological Psychology*, 1905.

evidence in favor of it. But it is by no manner of means established, and to try to import it as one of the fundamentals of psychological explanation cannot be warranted.

But the mainstay of neurological associationism is its explanation of habit as due to the lowering of synaptic resistances. Even here, however, there is one important difficulty noted by Meyer,[10] in connection with the short-circuiting supposed to be set up by habit. If we assume that a direct path has been substituted for a devious one, it is obviously a question why the direct path was not used in the first instance. The explanation offered by Meyer is that new neurones actually come into existence, or at least ripen into capacity to function, under the tension of the system induced by constant repetition. It would be hard to find a better illustration of the entirely hypothetical character of these neurological explanations of behavior.

What is true in this special case holds also of the general account of habit in terms of lowered synaptic resistance. As Herrick points out, we have simply no knowledge whatever of the neurological meaning of habit.[11] The assumption that the formation of habit involves functional changes in the synapses is, however, peculiarly significant. The logic of the whole procedure seems most viciously *a priori*. Habit formation manifestly does not involve any immediate and obvious change in either the receptors or the effectors. Again, there are difficulties in believing that the change occurs in the sensory or motor fibres. And the known physiological modifications of the cell-bodies of the central neurones seem irrelevant as a possible basis for an account of hablt. There remain the synapses, and as nobody knows very much about them, nobody can contradict almost any hypothesis that it may serve our turn to frame regarding them. The whole account is not an explanation at all, but an appeal to ignorance.

If we approach this point of view from another angle, a further set of difficulties emerges. Not only are the various constructions and brain-schemes offered by neurological associationism entirely unwarranted by any direct knowledge we possess of nervous function, but they almost egregiously fail to account for the phenomena of conscious life for which they are supposed to provide a solid basis. The value of a good hypothesis is that it directs empirical investigation. But here the order is reversed. First we investigate and tabulate our facts, and then we indulge in speculation to account for them. No important psychological discovery has ever been made by drawing brain-schemes. We do not begin with a construct of nervous function,

[10]M. Meyer, *op. cit.*, 98-100.
[11]C. J. Herrick, *An Introduction to Neurology*, 1918, 328-330.

and then go on, in the manner of the mathematical physicist, to demonstrate such phenomena as retroactive inhibition, perseveration, the sequence of forgetting, the optimum distribution of practice periods, the relation of affect to memory and the like. Our procedure, in fact, is precisely the reverse of this. Our constructions are nothing but gratuitous additions to ascertained facts.

It is often urged as an apology for the treatment of the phenomena of learning in terms of brain-schemes that at least it has a certain pedagogical value. This, however, raises the question in its widest and most crucial form. It may undoubtedly be easy for an elementary class to grasp the plausible conception of the wearing down of cortical paths; and if this is no more than an innocent superfluity, used only for illustrative purposes, perhaps no damage has been done. But it is a question whether it does not involve an essentially misleading notion of the nature and purpose of psychological analysis. Haldane's views become relevant here. He argues that the biological categories are not adequate for psychology,[12] and that explanations of mental life in terms of nervous function are to be disallowed because they treat of the whole in terms of the part.[13] This point seems much more defensible than his contention that physiology cannot be reduced to terms of chemistry and physics, since the assumption of continuity which underlies biochemical research is one that bears empirical fruit. Neurophysiology is one of the necessary interests of the psychologist; but it has been pointed out that we cannot look to it for any factual explanation of psychological phenomena in general, and indeed not even of the detailed phenomena of learning. When an account in neural terms is forced, it becomes purely schematic and hypothetical, and in fact essentially outside the realm of scientific psychology.

A final and very important criticism is that presented by Weiss.[14] He contends that objective psychology must take into account the individual-social aspect both of stimulation and of response. "The writer believes that a very important part of the behaviorist's problem is that of establishing behavior categories which will displace the traditional methods of representing the social status of the individual." A behaviorism which confined itself to a study of the central nervous system, and rejected everything which could not be translated into terms of nervous function, would almost stultify itself by cutting itself off from all vital content. It may be said that we merely propose to employ

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[12] J. S. Haldane, Are Physical Biological and Psychological Categories Irreducible? *Proc. Arist. Soc.*, 1918, 419-478.

[13] J. S. Haldane, *The New Physiology and Other Addresses*, 1919.

[14] A. P. Weiss, *op. cit.*

the nervous system as the integrating agency, without limiting our science to any such narrow scope. But the difficulty remains; for many of the phenomena with which psychology has to do are very remotely related to nervous function, and are in no way illuminated or explained when represented by brain-schemes. Thus we are bound inevitably to seek a wider and more adequate as well as a more factual and solid principle of integration.

2. *Reflexology*[15]

Another and very different type of behaviorism regards the reflex arc as the fundamental element in behavior, and seeks to interpret all psychological phenomena as patterns of reflexes. It may be worth while briefly to note that it is essentially contrasted with neurological associationism, for objective psychology itself is so new that its subdivisions are not yet clearly recognised. The basic distinction is that here the reflex circuit is taken as the irreducible unit for analysis, and it is urged that the division of the nervous system into central and peripheral mechanisms, while it may have physiological and anatomical importance, has no meaning for psychology. Bechterew[16] offers little in the way of hypothetical construction to explain his instinctive, associated, imitative, symbolic and personal reflexes. He simply presents them as patterns discovered by a direct and factual analysis of behavior, without making efforts, in the manner of Meyer, to go "behind" them. And Watson[17] severely criticises the tendency to present brain-schemes in explaining psychological phenomena, even though admitting freely that the data of neurology may be of high importance. It is clear, then, that here we are in the presence of a tendency radically different from that which was first considered. Instead of dealing in terms of associations set up in the centres, we now look for our principle of integration in Sherringtonian groupings of reflexes.

The first thing that strikes us in criticising this account of objective psychology is the vagueness with which the term reflex is used. The "reflexology," in fact, is apt to become quite nominal, and this seems to undermine its validity as a system. This difficulty is perhaps most evident in the work of Bechterew. His systematic treatment of objective psychology, in spite of the great mass of interesting and significant empirical detail which is so freely introduced, amounts as a whole to a *petitio principii*. He lays down the following programme. "En

[15]This term is borrowed from Bechterew as the most apt designation of this tendency familiar to the writer. Calkins' "extreme behaviorism" obviously will not do, because it is intended to suggest a contrast with "modified behaviorism," which Warren has pointed out as quite illegitimate.

[16]W. Bechterew, *La psychologie objective*, tr. par N. Kostyleff, 1913.

[17]J. B. Watson, *Psychology*, 19.

fin de compte, tout acte neuro-psychique peut être réduit au schema d'un réflexe où l'excitation, atteignant l'écorce cérébrale, éveille les traces des réactions antérieures et trouve, dans celles-ci, le facteur qui détermine le processus de la décharge." But his method is merely to label every kind of neuropsychic process a reflex; and so far as one can see, the label is quite gratuitous and adds nothing. Let us consider as a single good illustration his treatment of language as a "symbolic reflex."[18] We are told that "la parole humaine est une modalité des réflexes associés."[19] In this development of language he distinguishes between the formation of concrete, generic and abstract complexes. But the amazing feature of this account is his claim that the development of language is closely analogous to the development of the conditioned plantar reflex. It is very difficult to deny that here (and this is a typical case) Bechterew is assuming what he has to prove, namely, that to subsume language behavior under the general head of reflex behavior is either possible or useful. He clearly seems to be twisting the facts to fit into the scheme of the reflexology.

Much the same criticism can be urged against the work of Watson on its systematic side. Watson gives definitions of instinct, habit, and emotion as specific reflex patterns. But he makes no attempt to show just what segmental reflexes go to make up these alleged patterns. The segmental reflex is an abstraction even for neurology, and one fails entirely to see the point of using it as the ultimate datum of psychological analysis If we wish to understand the nature of nervous function and the part played by the nervous system in the *milieu interne* we shall have to deal intensively with the compounding of simple reflexes.[20] But this by no means implies that we ought to regard every type of behavior as essentially organised out of simple segmental reflexes. Objective psychology cannot find its principle of integration here.

Further light on the claims of reflexology is derived from a study of its treatment of learning. Having repudiated brain-schemes, and (so far as Watson is concerned) retention-traces, it becomes a problem how to explain the modifiability of behavior through use. In general an account is provided by the work that has been done on the conditioned reflex. We have seen how Bechterew regards all the more complicated acquired

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[18] *Op. cit.*, ch. viii., pt. 2.

[19] *Ibid.*, 371.

[20] This is, of course, a main topic of Sherrington's *Integrative Action of the Nervous System.* There is no doubt that conceptions developed in this work have deeply influenced Watson; but in going over from Sherrington's positions directly to an account of psychological phenomena as reflex patterns he is guilty of a *non sequitur.*

types of behavior as essentially "associated" or conditioned reflexes. Watson writes: "Between the purely instinct-reflex level of activity seen at birth and the level represented by definite habits. . . . there is a stage of activity of the habit kind deserving more consideration than it has hitherto received. . . . The pattern of the activity is not complex and for this reason it is often spoken of as being the putting on of new instinctive part activities. Our own view is that they are of the conditioned reflex type and are therefore acquired."[21] The more elaborate acquisitions of later life would not necessarily involve an entirely different kind of learning, but merely an increased momentum and complexity in the whole mass of learning. Lashley also insists that the conditioned reflex is typical of learning,[22] and even goes so far as to suggest that a complete psychology of learning could be built up from this basis.

Unfortunately, however, we cannot admit that the work on the conditioned reflex provides us even with the elements of an account of learning. Pavlov finds that the study of the conditioned reflex casts much light on the functioning of the cerebral hemispheres, and with this, of course, we have nothing to do. But we strongly believe that the psychological significance of this work has been greatly exaggerated. The work of Doutchef-Dezeuse[23] and of Gley and Mendelssohn,[24] if measurably sustained, would completely undermine the explanation of learning in these terms. For, clearly, if a complex mass of psychic elements, such as memories, emotions, imagery, and the like, necessarily intervenes whenever a conditioned reflex is set up, the introduction of the concept will not simplify but very greatly complicate our psychology of learning. However, these elaborate psychical constructions seem gratuitous, and to that extent the protests of Pavlov[25] and Lashley[26] are justified. At the same time the critical point raised is quite serious and plausible enough to make us hesitate long before sweepingly accepting the notion of the conditioned reflex as an explaining concept. In particular, the experiments of Watson seem open to objection. Hamel[27] repeated his experiment on the finger reaction,[28] and by

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[21] J. B. Watson, *Psychology*, 269-270.

[22] K. S. Lashley, The Human Salivary Reflex, *Psych. Rev.*, 23, 1916.

[23] M. Doutchef-Dezeuse, *L'image et les réflexes conditionnels dans les travaux de Pavlov*, 1914.

[24] E. Gley and M. Mendelssohn, Quelques expériences sur le réflexe salivaire conditionnel chez l'homme, *Compt. rend. Soc. de biol.*, 1915.

[25] J. Pavlov, Huxley Lecture, *Lancet*, 1916.

[26] *Op. cit.*

[27] I. A. Hamel, A Study and Analysis of the Conditioned Reflex, *Psych. Monog.*, 1919.

[28] J. B. Watson, The Place of the Conditioned Reflex in Psychology, *Psychol. Rev.*, 23, 1916, 89 ff.

calling for introspections and observing the reaction time of the conditioned reflex, concluded that intervention of psychic elements is involved. Cason, however, obtained precisely contradictory results. Working with the palpebral reaction he found that the reaction-time of the conditioned reflex was very much faster than with the voluntary movement,[29] and he set up a conditioned reflex with the iris reaction[30] which is, of course, in effect involuntary. The conclusion seems to be that valid conditioned reflexes are possible in certain cases, but that in dealing with the more complex and voluntary type of reaction it is exceedingly probable that other elements are involved. Thus the conditioned reflex cannot be taken as the simplest complete type-case of learning. The work of Watson on emotional reactions confirms this view.[31] In conditioning the alleged fear-reaction to indifferent stimuli we have something far more complex than a study of the conditioned reflex itself. Rather it is a study of a highly complex piece of learning which itself calls for further analysis. The value of Watson's work in the field is that it shows the importance of what he elsewhere calls sensory habits,[32] that is, the tying-up of a single response to a number of different stimuli. The great importance of sensory habits in learning, to which far too little attention has been paid, is the more evident when we bear in mind the numerical excess of sensory over motor fibres.

We can now bring to a head our criticism of this whole tendency. The proposal to regard the segmental reflex as the ultimate unit of behavior seems definitely to break down. When we speak of an instinct, a habit, or an emotion, as a pattern of reflexes our description is purely nominal and ideal, and in no sense constitutes an explanation. The whole interest of psychology is focussed upon the pattern and not upon the reflexes. And also the notion of the conditioned reflex can give us no more than a nominal and very questionable basis for the treatment of learning. It cannot be demonstrated that even the formation of all sensory habits is in any significant sense a process of conditioning reflexes. Most of the striking cases of learning which have been studied with a view to reducing them to this category either do not involve conditioned reflexes at all, or involve other elements as their most important and prominent constituents. Thus evidently the attempt to find a principle of integration

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[29] H. Cason, The Conditioned Eyelid Reaction, *Journ. of Exp. Psychol.*, 5, 1922.

[30] H. Cason, The Conditioned Pupillary Reaction, *ibid.*, 5, 1922.

[31] J. B. Watson and R. Raynor, Conditioned Emotional Reactions, *ibid.*, 3, 1920.

[32] J. B. Watson, *Behavior*, 220 ff.

for objective psychology in a Sherringtonian compounding of reflexes, with the notion of the conditioned reflex superadded to account for learning, breaks down.

3. *Mnemic Causation*

It seems that objective psychology must seek a different and wider principle of integration to account for the facts of its field. There is abundant evidence to be found in the pages of the journals and in recent publications generally that psychologists are fully alive to this necessity; and it may help us to formulate our own positive suggestion if we briefly review three tendencies now before us that may be interpreted, at least in part, as protests against a narrow conception of the sphere of behavior and an inadequate formulation of the principle of integration.

The first of these is the point of view developed by Semon. Several points in regard to his conception of the engram are instructive here. It is not regarded as a specific pattern, and thus cannot be thought of as in any way equivalent to a neurogram. Semon himself characterises it as follows: ". . . nichts anders ist als eine veränderte Disposition der reizbaren Substanz in bezug auf die Wiederholung dieses Erregungszustandes."[33] Though this seems like a very indefinite description for an entity so fundamentally important, Semon tells us that engrams are found throughout the entire organic world, and that the activities of the individual very largely depend upon a store of engrams (Engramschatz)[34]. Furthermore, engraphic effects can carry over from one generation to another. The primary indifference condition occurs somewhere along the parental line, and the offspring directly inherit a secondary indifference condition. The conception of integration involved is perhaps most clearly seen in the two laws of engraphy and ekphory: "all simultaneous excitements in an organism form a connected simultaneous excitement-complex, which as such works engraphically", and "the partial return of the energetic situation which formerly worked engraphically operates ekphorically on a simultaneous engram-complex."[35]

Two general criticisms of this point of view seem to be relevant to the present discussion. In the first place, we cannot agree to Semon's contention that mnemic phenomena are exhibited by all organic beings from the protista upwards. To say that all learning, in the widest sense of the term, from that of unicellular organisms to man, is due to the formation of engrams is a thesis impossible of verification, and involves a *prima facie*

[33]R. Semon, *Die Mneme*, 1911, 38.
[34]*Ibid.*, ch. 2. Also cf. *Die mnemischen Empfindungen*, 1909.
[35]*Die mnemischen Empfindungen*, 146 and 173; *Die Mneme*, 200 f.

tendency to ignore essential differences. Then, secondly, the phenomena of learning, even in man, by no means necessarily imply the existence of such entities as engrams. Indeed the idea that each piece of learning depends on the existence of a unitary engram, distinct from all others, and capable of preserving its identity in a discrete aggregate or store of engrams, seems merely to add to, rather than to diminish, our difficulties.

The second of our three tendencies is that of *Gestalt-psychologie,* which expands a phenomenological account of perception into a method of general application throughout psychology. The justice of interpreting the phenomenological tendency as in part a protest against a narrow conception of psychological integration is evident from Koffka's criticism of associationism in general and of the behaviorism of Watson in particular.[36] The central thesis of the school is that mental life consists always of structures (*Gestalten*). We do not respond to discrete stimuli, but to a continuum. On the introspective side, the effort to analyse complex contents down into sets of discrete sensations is hopeless; for we begin with unified and articulated structures from the very first, and these cannot be further resolved. In a certain sense this implies that psychology need look for no principle of integration, as the primary real, the primary datum, is found to be structure already integrated. But here we come upon what is precisely the difficulty of the position. We may be forced to admit that we never encounter certain elements, e.g., sensations, in a "free" and discrete state, and that the attempt to build up the content of conscious life out of patterns of these elements is highly artificial. But it is just as artificial to regard mental life as consisting of unitary, individuated structures which preserve their independent identity. The picture which is presented of education as consisting in the progressive addition to a store of structures is open to precisely the same objection that we urged against Semon's *Engramschatz.* We cannot agree that the phenomenologists have solved the problem of integration by denying that any such problem exists.

The last of our three tendencies is what has been called "organismic" behaviorism. This is admirably presented, in a number of papers, by Kantor. The attempt is made here to present the nature of instinct, reflex, emotion, perception, and so forth, directly as stimulus-response complexes. One or two specimen points made by Kantor will suffice to make the programme clear. He regards emotions as "no-response" activities. "Unlike any other type of behavior the emotional reaction is not a positive response to a stimulus but rather a failure of a stimulus-response coordination to operate. Emotions are

[36] K. Koffka, *Die Grundlagen der psychischen Entwickelung,* 1911.

therefore essentially 'no-response' activities." And his comment on the work of Cannon, which is instructive for an understanding of his general view, is that it is really not an investigation of emotion at all.[37] Again, we find a characterisation of instinct derived from direct inspection of the stimulus-response elements involved, and bringing in nothing at all about preformed neural paths or groups of reflexes. "An instinct is a comparatively simple and direct response to a specific stimulating object or condition. It is in fact the functioning of a connate potential reaction system which is organised from simple psychophysiological dispositions or tendencies to respond to stimuli."[38] Again, in distinguishing between the anthropological and philological and the psychological interest in language he writes: "the psychologist must look upon language as a series of intimate actions of particular persons, speaking, reading, listening, gesturing and interjecting, in short, adaptive responses."[39] His general point of view is well expressed when he says: "when a physical object induces a reaction in the organism that object obviously becomes a datum for psychology and we call that object a stimulus."[40] And by way of elucidation: "by the term organismic we mean to point out the absolute inseparability of the factors in an emotion or any other psychical act. Emotions as acts of a unique individual cannot be thought of as composed of parallel or interacting paths."[41]

The strength of this position is that it implicitly and directly recognises the wide scope of objective psychology, but does not invoke noumenal entities such as engrams and *Gestalten*, physiological fictions like neurograms, or partial processes distended to an illicit generality like the conditioned reflex. But, it may be asked, is it not superficial, does it not completely fail to recognise the necessity for some principle of integration? The answer is that it is by no means necessarily inadequate in the sense that it ignores the problem of integration or makes a solution impossible. Direct inspection of functional adjustment, such as is advocated by organismic behaviorism, yields a feasible and adequate account of the principle of integration.

Suppose we raise the question why a specific response occurs: the cause will not have been exhaustively described merely by reference to the immediately preceding stimulation. The im-

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[37] J. R. Kantor, An Attempt toward a Naturalistic Description of Emotions, *Psychol. Rev.*, 28, 1921.

[38] A Functional Interpretation of Human Instincts, *ibid.*, 27, 1920.

[39] An Analysis of Psychological Language Data, *ibid.*, 29, 1922.

[40] Association as a Fundamental Process of Objective Psychology, *ibid.*, 28, 1921.

[41] An Attempt toward a Naturalistic Description of Emotions, *ibid.*, 28, 1921.

mediate and present sequence of stimulus and response is more or less mysterious without some reference to the individual's past. As we have shown at length, we cannot hope for an explanation in terms of neurological constructions or of the compounding and conditioning of reflexes, and the engram theory and the *Gestalt* theory are both unsatisfactory. Now the organismic point of view suggests an approach to the difficulty from another angle altogether. Why can we not say of a given response that it is either wholly the result of the preceding stimulus, when we should have a purely hereditary connection, such as is presumably found only with great rarity and under highly special conditions,—or that it is the result of the immediately preceding stimulating situation, together with a past sequence of stimulus-response complexes? In this case we have the result of training, which, be it noted, we have formulated in a distinctive manner. Our suggestion is that, in the case of any acquired reaction, the total proximate cause is not the immediate stimulus, but that it must be expanded to include an indefinitely large series of past stimulus-response complexes. If we ask how a telegrapher is able to use his key with such dexterity, we do not explain in terms of the visual-postural-kinaesthetic stimulating situation together with neural patterns, or sets of conditioned reflexes, or engrams. We explain in terms of the present stimulating situation together with past similar situations.

Now here we have precisely what Russell, borrowing a term from Semon, has called *mnemic causation.*[42] In ordinary physical causation, which is the basis for physical science, the proximate cause of an event is that which immediately precedes it in time. But in mental life this is not the case. It should be specially noted that our conception is not at all that any action is preceded by a long and determinate chain of causes and effects. This is a commonplace assertion that can be made about any event whatever. Our point is, once more, that the complete *proximate* cause of any acquired action is a whole string of past events in the history of he organism. The individual carries his past along with him in a unique manner. If we ask why the electric light has gone out, it is an adequate answer to say "because someone threw a switch and cut off the current." On the other hand, if we ask why a pianist plays a certain composition so well, it is obvious that to explain in terms of the immediate stimulating situation is absurd. We have to say that the reason is that he has practised it innumerable times, has been drilled and criticised in his interpretation of it, and has played it often enough in public to have full self-confidence. This is no *sequence* behind the behavior, but the actual immediate cause itself.

[42]B. Russell, *The Analysis of Mind*, 1921, 77-107.

It is suggested that in mnemic causation we have the principle of integration that is required by objective psychology. A number of points are to be urged in this connection.

(1) In the idea of mnemic causation we come upon the fact which the various theories of integration we have been discussing endeavor to formulate. In particular, of course, neurograms and engrams are hypothetical constructions designed to account for the unique manner in which a living individual carries forward his own past. But why are such constructions necessary at all? Why should we not merely be content with the facts as they stand? Without arguing at all as to whether Weiss' formulation of behaviorism already cited is the only or the best psychological programme, we may claim that it is at least a feasible programme. What it involves is simply a natural history of the individual's stimulus-response complexes. We return to Haldane's contention about the irreducibility of psychological categories when we insist that explanations in terms of neural constructions, or conditioned reflexes, are not valid, but that what we need is merely a direct recognition of the fact which these hypotheses approach, the fact of mnemic causation.

(2) Adopting the organismic point of view and recognising mnemic causation as our principle of integration, we immediately straighten out the relations of psychology to other sciences. Its relation to neurology and physiology is no longer that suggested by neurological associationism. We do not desire to know what happens in the synapses or the cortex because we think that here we shall find the ultimate explanation of our problems. Knowledge of obscure bodily changes is important because not until the organism has been made, so to speak, transparent to the eye of science can we be sure that we know precisely what is involved when a response takes place. Our interest in neurology and physiology, then, is merely the wish for a refined and precise description of responses. As to the relation of psychology to the social sciences, it is evident that we have here a sufficiently broad interpretation of the objective point of view to make possible an account of the individual-social aspects of behavior emphasised by Weiss. At the same time, the fact that we are always in effect dealing with events as mnemic phenomena makes it possible to preserve the distinctive psychological point of view everywhere.

(3) We see that the notion of mnemic causation is a fruitful and natural hypothesis for objective psychology. In the first place it enables us to give an account of heredity that is valid for psychology, as being constituted of stimulus-response complexes that occur without training. We do not refer to nerve-schemes to explain instinct and reflex, and in so far as we study the inherited structures at all it is merely in order to have a

better detailed picture of the functions. In the second place, every direct study that has been made by psychology of the effects of past experience can be nothing else than a study of the detail of mnemic causation. Objective psychology can validly raise two types of questions. First, it can raise the descriptive question what kind of functions instinct, reflex, emotion, will, language, perception, etc. may be. We then give an organismic account in terms of stimulus and response. Secondly, it can raise the causal question how such and such a behavior comes into existence, closely related to which is, of course, the question what types of behavior actually are in existence. We then deal in terms of mnemic causation.

THE ORIGIN AND PSYCHOLOGICAL FUNCTION OF RELIGION ACCORDING TO PIERRE JANET

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INTRODUCTION

For many years now Pierre Janet has been lecturing on psychology at the *Collège de France*, building up a formidable body of data and conclusions, and working out a method and an approach that are all his own. Each new topic which he selects for investigation is handled with a confidence which suggests not the speculative agility of the professional philosopher—though Janet's powers of synthesis are by no means contemptible—but the tested knowledge of the experimental scientist and the ripe experience of the clinical specialist; for his hypotheses in all branches of psychology and philosophy are suggested and tested by his observations of psychopathic patients

at the *Salpêtrière*—observations made with an exactitude and reported with a clarity and suggestiveness which we have often had cause to admire.

When such a man turns his attention for the first time[1] to the psychology of religion, it is time for all interested parties to take notice; for something fresh and stimulating is to be expected from him. Such at least seemed to be the feeling of the lecture-going public at Paris; for which Janet announced, for the winter of 1921-22, a course on the "Evolution of Moral and Religious Conduct", they crammed his dingy lecture-room to its capacity at the first session, and all the winter long they cheerfully endured the discomforts of backless benches and bad ventilation without once flagging in their interest. The popularity of the course was due in some measure, to be sure, to the scintillations of Janet's Voltairean wit—which no dull reproduction could ever hope to preserve undimmed—but chiefly, I believe, to the intrinsic importance of the subject and the originality of the views presented. I am confident that I was not the only foreign auditor who felt that these lectures alone had repaid him for his trip to France.

These are my reasons for hoping that an account of these lectures,[2] with a few notes and comments, will be welcome to American students of the psychology and philosophy of religion. It is to be hoped that the substance of the course will eventually

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[1]Directly, that is. The student of the psychology of religion will find studies of ecstatics, religious paranoiacs, etc., scattered all through Janet's writings. The following bibliography may be of use to those who wish to acquaint themselves with these studies:

(1) *L'Automatisme psychologique*, 1889; 7th ed., 1913.

(2) *Baco Verulamius alchemicis philosophis quid debuerit?* (Doctor's thesis at Sorbonne, 1889.)

(3) *État mental des hystériques: les stigmates mentaux; les accidents mentaux*, 3 vols., 1892-94; 2nd ed., 1 vol., 1911; Eng. version, 1901 ("The Mental State of Hystericals").

(4) *Névroses et idées fixes*, 2 vols., 1898 (in collaboration with F. Raymond).

(5) *Les obsessions et la psychasthénie*, 2 vols., 1903 (collab. Raymond).

(6) *The Major Symptoms of Hysteria* (lectures at Harvard Medical School), 1907; 2nd ed., 1920.

(7) *Les névroses* (popular exposition), 1910.

(8) *L'Alcoolisme et la dépression mentale*, 1915.

(9) *Les médications psychologiques*, 3 vols., 1919-20.

Numerous articles in the *Archives de Neurologie*, the *Revue philosophique*, the *British Journal of Psychology*, and especially the *Journal de psychologie normale et pathologique*, 1904 ff., ed. Janet, Dumas.

For reports of Janet's lecture courses, see the *Annuaire du Collège de France*.

[2]In fairness to Janet, it should be made clear that he neither guarantees the exactitude of my résumé, nor holds himself committed to views tentatively advanced in a series of popular lectures.

be published;[3] but meanwhile discussion cannot wait, for Janet has raised numerous issues of major importance with regard to the origin, function, and future of religion.

Janet began with a few comments on the title of the course.

(1) "CONDUCT" (literally *conduites*, in the plural. It would be convenient if we could also use the plural, and speak of "conducts" or "behaviors" as we speak of "actions"). The use of this word implies the "external" point of view in psychology, according to which psychology is "the science of the external reactions between the organism and the environment."[4]

Ever since Descartes set the internal phenomena of consciousness in the foreground of philosophy, psychology has—until very recently—followed in his train. If his subjectivistic lead be advantageous from the philosophical point of view—which may be doubted—it has at any rate proved "deplorable" from the scientific. To conceive that consciousness is the fundamental thing, and action the secondary, is totally to misconceive the situation. In order of time, action precedes thought, desire is only an abridged action, and external discussion between man and man is more primitive than that internal discussion which constitutes thought.

(2) "EVOLUTION". This is purely a methodological term. To speak of the "evolution" of moral and religious conduct, and to divide the process into higher and lower stages, is not to say that history presents any such continuous progression from lower to higher. We merely assert that, for purposes of study, the most useful order is that of increasing complexity and increasing perfection.

Nevertheless, if we analyze conduct into its simpler and its more complex forms, we shall discover that the simpler types are common to the infant, the savage, and the mental defective, while the more complex types are peculiar to civilized man in his normal mental state; so that our order of evolution is not merely arbitrary. For the reconstruction of the earlier levels of conduct we are not solely dependent on anthropology, for we can study those identical levels of conduct in the child and in the mentally subnormal individual. Mental diseases are nothing more nor less than the disappearance of certain higher, more complex levels of conduct, and the exaggeration of certain lower, simpler types. Anthropology, child psychology, and psychiatry confirm one another.

But what are these levels through which conduct must climb in the course of its evolution? Janet assumed that his auditors were familiar with his "hierarchy of mental tendencies," and so contented himself with outlining it upon the blackboard. I give herewith a somewhat fuller outline, based upon the important lectures which Janet had recently given at the University of London, entitled *La tension psychologique, ses degrés, ses oscillations*. These lectures, published in the *British Journal of Psychology, Medical Section*, 2, 1920-21, 1 ff., 144 ff. and 209 ff., form the best possible introduction to Janet.

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[3] Janet tells me that he may perhaps give a place to these lectures, in substance, in a book he hopes to write on the "Psychology of Behavior" (*Psychologie de la conduite*).

[4] Janet's *point de vue externe* seems to be identical with what we call the "behavioristic" point of view. American behaviorists apparently are not aware of the close parallel which exists between his form of anti-introspectionism and their own. In a recent lecture at Teachers College, Watson made the statement that the behavioristic method had never yet been applied in the field of abnormal psychology. Either I am wrong in classifying Janet as a behaviorist, or Watson was mistaken.

1. *Reflex level:* explosive reaction, in which discharge takes place all at once.

2. *Perceptive or suspensive level:* retarded discharge.

3. *Social or socio-personal level:* common to the higher animals, primitive man, and idiots;[5] conduct with reference to other members of the same species; distinction of Self from Other. "The individual no longer reacts merely to the stimuli which come from the external world, he reacts to his own reactions" (*Brit. Journ. Psych., loc. cit.*, 147).

4. *Level of elementary intelligence: imbeciles;* "combination of two perceptive *conduites* into a single synthetic act" due to the necessity of adding something to one's acts "to make them social, to render them intelligible to others" (*ibid.*, 149).

These socially intelligible acts are the beginning of language, and mark the transition to the human stage, for man is an *"animal bavard"*. All succeeding levels are characterized by "the establishment of more and more complicated relations between speech and language" (*ibid.*, 151).

5. *Assertive level (niveau assértive ou pithiatique):* level of *le débile mental;* roughly equivalent to Lévy-Bruhl's "pre-logical" stage. "The assertive tendencies", says Janet in an explanatory note to me, "are characterized by the application of language to conduct, by the active union between language and the movement of the body (*membres*) which constitutes the essential part of primitive will and belief." At this stage, desires are inseparable from beliefs; beliefs are not inhibited by the consciousness of inconsistency between speech and action.

6. *Reflective level: l'égoiste passioné;* beginnings of internal conversation or thought, which "reproduces within ourselves the discussion of an assemblage, and which does not permit the giving of assent until after an inward discussion" (*ibid.*, 155).

7. *Ergetic or rational level: the system-builder;* beginnings of self-imposed discipline of the will and the thinking-process (work, ascetic morality, and logic); appearance of systems of philosophy judged by the test of inward consistency.

8. *Experimental level: the scientist;* subjection of human desire and human logic to further discipline through application of the test of conformity to external nature.[6]

9. *Progressive level: the genius;* appearance of the idea of progress and the conscious quest of progress; a stage still lying far above the average level of human intelligence.

Conduct at these higher levels is more difficult than conduct at the lower levels, in that it is more *efficient* (adapted to its specific end), more *complex* (hence more acutely *conscious*), and more elaborately *systematized* into long unified sequences of acts. It requires higher mental *tension* to bring it to successful completion, and a single act consumes more mental *energy* or *force* than a multitude of lower-level, lower-tension acts,—as may be seen in cases of "derivation," when the patient performs a multitude of low-tension acts in place of a single high-tension act from which he shrinks (*ibid.*, Lecture I, *La force et la tension psychologiques*).

The questions to consider in attempting to describe the "evolution" of moral and religious conduct are therefore the following.

1. At what level does the type of conduct with which we are concerned first appear?

2. What characteristic forms does it assume at each successive level thereafter? Does it tend to persist or does it tend to become so radically transformed that it may be said to disappear?

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[5]For parallel between levels of mental evolution and the well-known levels of mental degeneracy, see *B. J. Ps., loc. cit.*, 209.

[6]Cf. levels 6, 7, and 8 of Janet's hierarchy with Comte's "theological," metaphysical" and "positive" stages.

Janet paused to remark that generalizations about "man" and his moral and religious nature are foolish; it is necessary to specify what grade of man is to be studied. Most differences of opinion about the nature of morals and religion arise, he said, from the fact that different levels of conduct are studied by different investigators. For example, the wide variations between Durkheim's and Leuba's[7] descriptions of religion arise from the fact that Durkheim studies conduct at the social, elementary intellectual, and assertive levels, where the rite and the myth are in the foreground, while Leuba studies it at the reflective level, where the divinity first enters upon the scene.

I. Preliminary Description of Moral-religious Conduct

Acknowledging the difficulty of framing a definition of moral or religious conduct which will apply at all levels of conduct, we must nevertheless describe their nature in a general way at the very start, if we are to recognize them when they appear in the course of mental evolution. In this preliminary description, we need not distinguish sharply between moral conduct and religious conduct, for it is only late in the history of mental development that they become differentiated.

What, then, is the distinctive function of moral-religious conduct, which distinguishes it from all other types of conduct, at all the levels of conduct? The answer is, in a word, the function of *government*, organization, control. Other types of conduct are defined by their content, their object, their specific goal; but moral conduct mingles with all conduct, and controls the budget of all the mental forces.[8]

To speak of a "budget" of mental forces is not to use purely fanciful language. The control of household and national finances is only a special form of a far more general type of conduct. Wherever you have expenses and receipts you have a budget; wherever you have the attempt to keep the sum of the expenses below the sum of the receipts, you have economy; and the instinct of economy is one of the most elementary racial instincts, and the root of all morality. The economy of one's financial resources is a secondary moral task; the economy of one's mental resources is the primary moral task. In describing the moral task we may therefore most aptly use financial metaphors.

(1) *Mental expenses.* These create the moral problem. That they exist is evident, for they sometimes lead to mental bankruptcy, and frequently lead to various degrees of mental poverty—which is an accurate appellation for mental disease. Fatigue is "the suspension of action in prevision of future bankruptcy," yielding to fatigue is the beginning of all morality. If we study the mentally bankrupt or poverty-stricken—the war has left plenty of them in our midst—we may easily perceive what types of action are ruinous or expensive. For one thing, the cranking-up (*mise en train*) of any action is very expensive. The mentally bankrupt can often continue an action they are incapable of beginning. Again, the effort to surmount

[7] Janet considers Durkheim and Leuba the two greatest figures in the field of the psychology of religion.

[8] Cf. this definition of moral conduct in general with Coe's definition of religion as an organizing tendency immanent in all the mental functions. Religion and morals often coalesce in Coe's psychology, as they always do in Ames'.

obstacles that unexpectedly appear in the midst of an action is of course costly; the weak shrink from it. To end an action costs almost as much as to begin it; many mentally diseased people cannot stop doing what they once have begun to do. Finally, decisions or conflicts of duty take great strength; the diseased always try to get others to decide for them.

What is this "force" that is expended in action, and whose exhaustion means mental bankruptcy? Janet has an aversion for metaphysics, and does not define it; but he is convinced that it is as real and objective as electricity. If we study it long enough, he says, we may come to measure its volts and amperes. Subjectively, its expenditure is accompanied by certain emotions; sadness, fear of action, etc., are the characteristic emotional signs of its exhaustion; but our method leads us to look at it rather from the objective, realistic standpoint.

(2) *Mental receipts.* The ultimate sources of mental energy seem to be physiological: the functioning of the alimentary and respiratory organs, the impact of light upon the body, the influence of electrical currents, etc., etc. Such receipts are taking place all the time, but the balance between receipts and expenditures is always unfavorable except during sleep. Sleep is the great renewer of energy, not because the receipts are then greater—as a matter of fact, they are less—but because expenses are temporarily reduced almost to nothing.

Men's psychological incomes are very different. Here above all the native inequality of men becomes evident. Each man's income is like a fixed salary, hardly varying from day to day. To be sure, the rich man's salary often seems variable; he accomplishes marvels of industry sometimes and little or nothing at other times; but this apparent fluctuation of energy is due to the fact that he never spends his whole income. His wealth is so great that neither moderate nor extravagant spending makes any appreciable dent in his pocket-book. He could always do more if he would. But examine the poverty-stricken neurasthenic, and you will see at once how rigidly fixed is his income. He can always accomplish exactly the same amount of work before collapsing; and if he is wise, he will not attempt to compete with men of larger income.

However, there are certain emergencies when not even the richest daily income is capable of covering the expense—times of danger, when one's own life or that of dear ones is threatened, times when fortune, honor, or native land is at stake. At such times, as James points out in "The Energies of Men," one suddenly discovers that reserves of energy lie at hand such as one hardly dreamed of possessing, and things are accomplished that in ordinary times are impossible. Not only do the actions multiply in speed and vigor, but the emotions run up into the heights of joy and exultation, and the whole world takes on a brighter hue. Sometimes illumination or photism is actually experienced. Joy and illumination are the infallible subjective signs of extravagant mental expenditure; one has the sensations of the man who has "money to burn" and flings it recklessly right and left, regardless of the outcome.

Whence come these unsuspected reserves of mental energy? Well, all instincts, tendencies, and habits carry with them a store of force sufficient to procure their own realization. They have a charge attached to them, as it were. Whatever stimulates the tendency releases the force. Instincts or primitive tendencies carry especially heavy charges. McDougall indeed considers that power belongs only to two or three primitive tendencies—life, maternity, power—and that all others derive their power from these; but Janet, after weighing the issue in the controversy between McDougall and other American psychologists,[9] is inclined to agree with McDougall's

[9] Janet mentioned Hocking as the chief opponent of this view of McDougall's. I should think that Woodworth's "Dynamic Psychology" contained the best critique of McDougall and the best statement of the contrary position.

opponents: that there is no tendency or habit which has not some motor charge attached to it. All stimuli call out reserve energies; but it is only when we are stirred to the depths of our primeval instincts that great explosions of energy occur.

But it is foolish to suppose that we can draw on our reserves forever, or that our reserves are unlimited. Experience shows that reaction, depression, and sometimes ruin follow every such triumphant meeting of an emergency. The whole world is now running along on a low level of mental energy on account of having drawn too heavily on its reserves during the war. Occasionally a sudden drawing on his reserves may make a man's fortune; the result may be the more active functioning of all the functions that tend to fill up the reserves again; and the man goes along thereafter, temporarily or permanently, on a higher level than before, But persistent drawing on the reserves, such as that practised by the alcoholist, is always bad.

We may sum up Janet's description of moral conduct thus: its function is the control of all other functions, for the conservation of mental energy. It is a dynamic type of conduct, and unless we take the dynamic point of view we shall miss it altogether. From a purely material point of view, says Janet, there are only four types of conduct: *rapprochement*, attraction, consumption, and excretion. Moral conduct is none of these, but mingles with them all.

II. Origin of Moral-religious Conduct at the Social Level

Durkheim has insisted—and many another has taken up the cry—that religion is a social phenomenon. Janet admits the truth of the remark, but fails to see any startling significance in it. To be sure, there is a social element in all moral and religious conduct—society and the chief, for example, stand in a very close relationship with duty and deity, respectively,—but all phenomena on higher psychological levels have arrived there by a process of evolution, and certain distantly corresponding phenomena can be observed on all the other levels, even the lowest. In this case, we might just as well say that moral and religious conduct evolved from reflex conduct as from social conduct; for even at the reflex level we find inhibition resulting from an eternal conflict between two reflexes, and inhibition is the prototype of moral control. All higher types of conduct are based on lower types, and so it means little to say that a given type of conduct evolved from social conduct. Janet agrees that moral-religious conduct first assumed its characteristic form at the social level; but the real question remains unanswered: what *aspect* of social conduct gave rise to moral-religious conduct?

One answer to this question has very commonly been put forward. Morality, we are told, originated in the parental instinct, which inspired respect for life, leading to mutual aid, collaboration and devotion. Love

is the basis of morality. From this point of view, remarked Janet, the ideal frontispiece for a text-book of ethics would be a picture of a cat suckling and licking her kittens.

The difficulty of this view is that morality thus defined must be sought far down the scale of evolution; if maternity is morality, physiology becomes moral. Nay more, men are much *less* moral than animals according to this standard. Primitive morality and religion are much less reasonable and logical than the conduct of the mother cat. The instinctive conduct of the animal tends in large measure to be *useful* to him and his mates; but three-fourths of the rites and customs of primitive man are cruel and dangerous as well as absurd. There is hardly a crime or an obscenity that has not been consecrated by custom and religion. From the rational-ethical point of view we have degradation, not evolution, in passing from the highest brutes to the lowest men. It is only later that reason, utility, and love begin to criticize custom and religion. "Only decrepit religions become reasonable." The Mosaic code was not a "disguised hygiene;" hygienic considerations were absolutely foreign to its authors. It is useless to look for the operations of any such motives at the start.

No, the type of social conduct from which morality and religion sprang was something quite different: *imitation*. Here was a clue which Durkheim ought not to have neglected, for he had the studies of Tarde before him; but in his anxiety to separate sociology from psychology he broke the link between the individual and social mind, and so made it impossible for himself thoroughly to explain religion or any other social phenomenon.

How does imitative conduct serve the end of mental economy? This it must do, according to our definition, if it is to qualify as even rudimentary moral-religious conduct; and this it does, as Janet at once proceeded to show. Imitation is nothing more nor less than action induced by the sight of similar action. Now sight, of all the senses, is the most social; it puts all bodies, one's own and one's neighbor's alike, on the same plane; hence to see another acting is to see oneself apparently acting, at least for primitive minds. If all perceptions involve incipient actions, much more is this true in this case. The savage—and indeed many higher animals, for the social level is reached before man appears—cannot see action without almost mechanically reproducing it.[10] Such externally induced conduct demands no effort at all, and so is enormously less expensive than the trial-and-error conduct which furnished the pattern and stimulus. Sheep and idiots incapable of taking a step alone can walk in a r[illegible] with ease.

But how about the bell-sheep? If imitative conduct is economical and hence moral for the group, how is it with the leader? Are not most extraordinary exertions required of him? They are, indeed, at the start. Not only initiative and choice (however haphazard), but also preparation for choice (alertness and watchfulness), are very expensive and often draw deeply upon the psychological reserves of the leader. But after the first step his investment begins to pay him compound interest, for he sees his action reproduced all about him, and a hundred identical visual images mechanically stimulate his muscles to continue in the given direction. The extra force he had mobilized for his heroic effort proves in part unnecessary and diffusing itself through his body appears subjectively as a feeling of satisfaction and joy. Imitation is cheap but not joyous; initiative is costly but joyous, and the expended energy is more than repaid. Hence the leader's lot is enviable in spite of its initial irksomeness, and the desire to be imitated early becomes a spur to conduct.

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[10]It is not strictly true, of course, remarked Janet, that imitation is "mechanical." Only such action can be thus induced as the organism is already predisposed to. But the organism *is* predisposed to all acts capable of performance by another member of the species.

Imitation, then, is an energy-conserving conduct both for the leader and for his imitators, and as such it makes for the strength of the group, and survives and grows as a type of conduct. Here we have the real origin of moral-religious conduct.

III. Moral-religious Conduct at the Level of Elementary Intelligence: Order and Command

Imitative conduct at the start springs up quite spontaneously and unconsciously. Someone takes the initiative because he happens to be the most energetic; the others imitate him almost automatically. But a higher stage of moral evolution sets in when leading and following become sharply specialized functions: when the leader *insists* on being imitated, and the followers may be said to understand and obey, and not merely to imitate him. Here conduct is complicated by the appearance of language; we are at what Janet terms the "level of elementary intelligence." Order and command are the characteristic phenomena at this level; they begin at the previous level among the animals, but their full evolution is impossible until the level of elementary human intelligence is reached.

It is not hard to see how this evolution occurs. Leadership is, as has been said, a pleasant and emotionally profitable experience, in spite of its costliness; hence the leader tends not only to continue to perform his function, but to extend it in such a way as to increase his profits from it, both by increasing his earnings and by decreasing his expenses. In the first place, he increases his earnings (in the shape of satisfaction at seeing himself imitated) by *facilitating* imitation, that it may spread as far as possible. He takes his position at the head of the group, where he is visible to all, and performs the action to be imitated as vigorously as possible—at least at the start—accompanying it with loud cries to attract attention. But in the second place, he soon discovers that he can save much expenditure of energy, and so increase his margin of profit, by ceasing to perform the given action as soon as the others are well started. Eventually, with the advent of language, the stage is reached when the leader omits the action altogether, and only the accompanying cry—"Fight! March!"—remains. This lightening of the leader's functions leaves him free to observe the conduct of his followers. Quick, perfect imitation gives him the most pleasure; hence he tends to demand it with insistence, repaying it with marks of favor and punishing the delinquent. Watch a herd of wild cattle and see how harassed the leader gets at each failure of the herd to respond instantly to his commands. First he renews the action to be imitated more energetically than before, with even louder bellowings. This failing, he begins to snort with anger, and to make threatening motions toward the delinquents. Finally he comes over and prods them with his horns. Thus, by the motives of love of approbation and fear of disapprobation and punishment, the actions of the herd tend to be stimulated, lengthened out, and elaborated, while the actions of the leader are undergoing the opposite process of shortening and simplification. He takes special charge of the *beginning* of the action; they take charge of the *end* of it, under his direction. Finally comes the stage when he *speaks* and they *act*.

The earlier part of this evolution is illustrated by the conduct of a pack of hounds. Both the leader and his imitators both bark and run. Such conduct lies on the "social" level. The stage at which the leader barks and the rest run, so to speak, lies on the level of elementary intelligence. It is

never reached by animals; it is the stage represented by primitive man. Voiced command and obedience thereto, the desire to lead balanced and fostered by the desire to be led,[11] are the most characteristic phenomena of primitive society; and it was out of this situation that there sprang the grotesque codes of custom that are the historical basis of all our morality.

The energy-conserving capacities of this type of conduct hardly need any further illustration. Janet pointed out that two-thirds of the commonly recognized types of mental diseases lie on the level of elementary intelligence, and might be classified as "authoritarian maladies,"[12] involving a mania for command or for obedience. Since the victims of all these diseases are mentally impoverished, they are not capable of real initiative, choice and command; they seek to get the rewards of leadership without its expenses by issuing all sorts of commands, hapazard, and they punish the delinquents not by an expensive exhibition of anger but by piteously exhibiting their suffering at not being obeyed. Another frequent type of authoritarian is the "non-commissioned officer," who gets his orders from higher up and so is saved the pains of choice and foresight, but repeats them with fierce vehemence and gets all the satisfaction of seeing them executed before his eyes. Authoritarians, then, are those who have a mania for commanding what *does not need* to be commanded or has already been commanded. Such cases testify to the rewarding nature of the commander's type of conduct; but there are other cases which testify to the even more economical and satisfying nature of obedience as a type of conduct. There are people who passionately abase themselves before some chosen commander and expend themselves in his service in a sort of delirium of generosity and self-sacrifice,—if only he will tell them just what to do at every step. Thus command and obedience, in the case of primitive man, naturally turned out to the profit of all. To the chief, the obedience of the others gave a delightful sense of security, unknown to the others. He need fear no competitor, for he stands above all competition and regulates it; he does not dread famine like the rest, for he knows that if only a mouthful is left in the tribe it is his; he does not greatly fear the enemies of the tribe, for he has hundreds of men ready to spring to his defence whenever he says the word. And so the future looks serene to him; but it also looks serene to the others, for they know that, whatever happens, they will always be told what to do, and so do not need to worry themselves over conflicting alternatives. Arbitrary and foolish as were many of the commands issued by the chief—and the customs and laws based thereon—it was better to submit to them than to have no chief.

IV. The Assertive Level: Rites and Myths: Pre-religion

With the assertive level new and higher types of conduct arise. The most important of them for our purpose, on account of their close association with what later becomes religious conduct, are the types of action and speech known as *rites and myths.*

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[11] Janet called attention to E. Seillière's studies in the "Philosophy of Imperialism." He agrees with Seillière in making the desire to command one of the prime factors in social psychology, but would not call it "primitive," for it is unknown at the reflex level. He would also point out that the desire to command is complemented by the desire to be led.

[12] Amatory maladies may be classified under this head, for they involve an exaggeration of the desire to control another person, or submit to his control. Exaggerated generosity may be classified as exaggerated obedience.

The best studies of rites have been made in central Australia and among American Indians; and it is upon these studies that Durkheim has based his conclusions as to the nature of the most primitive forms of the religious life. Janet regards these conclusions as, in the main, sound. He waives the objection that the Australians are not really primitive men, but only degenerates. What of it? After all, the mental nature of primitive men and that of certain degenerates are practically identical. Durkheim might have enriched his data—as Janet proposes to do—by studying child psychology and abnormal psychology. Guyau (*L'Irreligion de l'avenir*, 92 ff.) long ago pointed out that children, with their liking for "Just So stories," are natural ritualists; and psychopathic patients are many of them absurd sticklers for rules of the strangest nature. Moreover, many primitive rites survive in our codes of etiquette.

Rites are complicated conducts, mixtures of movements and words, which bear the following characteristics. (1) Their least details are rigidly fixed. (2) Men compel *one another* to observe them (even though they show a tendency to fail to observe them themselves when not under supervision). (3) No reason, either logical or moral, can be given for them; they are not instinctive, and they are frequently contrary to physical requirements, as well as absurd and debasing. There are two chief elements in every rite, a physiological act (such as eating) and an elaborate embroidery of it (represented by the whole code of table manners, for instance). They are best classified according to the species of complication, as Durkheim classifies them, for example: (1) negative rites of abstinence—taboos. These correspond to the various *phobias* of the nervous invalid, who cannot tell what he is afraid of and yet avoids certain things with a horror that is genuinely religious in its quality; (2) positive rites, in which complicating acts are added, not subtracted. According as the added act is pleasurable or painful, we get "ceremonial" or "piacular" rites.

As for *myths*, they are not so primitive as rites, and are usually attached to them as an afterthought. Any explanation of a myth must first explain the rite on which it is founded.

Most explanations of rites and myths err in coming to them with the presuppositions of a higher level of conduct. Spencer, for example, looks upon even these primitive beginnings of religion as an attempt to unveil the mystery of the universe; whereas the savage has no conception of mystery, since he has no conception of natural law; and need of explanation hardly acts at all as a motive at this level. Other explanations, on the other hand, err in trying to understand rites and myths in terms of lower levels of conduct. Durkheim, for example, says that they express the savage's wonder at what he feels in the presence of society, which to him is the source of all good and all evil. But rites and myths are not an inevitable concomitant of *all* social life; cows are social, and know nothing of rites or myths. These arise at a higher level than the barely social level, and to seek to understand them in terms of society alone is to commit the error of "reducing higher to lower" against which Durkheim himself frequently warns the sociologist, and to guard against which he draws a sharp line between sociology and psychology, fearing to see sociology explained in terms of psychology, psychology in terms of biology, and eventually biology in terms of physics.

Our explanation, said Janet, will seek to avoid both these errors, and to understand rites and myths in terms of the "assertive" level of conduct at which they arise. They are, in a word, the inevitable expression of the beginnings of *will* and *belief*. At this early stage the savage, like the child, talks not for any particular purpose, but for the sake of talking. Similarly, he wills for the sake of willing, and believes for the sake of believing—anything that comes into his head, without the least anxiety about consistency.

His only care is that what he wills, shall be carried out *just so,* and that what he believes shall be phrased *just so.* Belief and will are closely related; belief is a deferred act of will; so myth and rite are closely related.

The perfectly spontaneous character of primitive rites can perhaps be best perceived in the case of mimetic rites, in which the actions of the animal on which the tribe lives are aped. The rationalistic explanation of such rites—as found for example in Frazer's "Golden Bough"—solemnly sets forth the theory of "mimetic magic," based on the principle that an action similar to the wished-for action produces the wished-for action, and so, for example, the aping of the stages of growth of the "witchetty grub" makes more witchetty grubs grow. But the thing is much simpler than that. Observe the conduct of a group of sportsmen planning to go quail shooting. They generally feel it best to get together for a dinner before the hunt to "get their enthusiasm up"—particularly if quail are scarce and the prospects are poor. One after another tells stories of previous hunting trips. Heated by the presence of company and possibly also by the liquid refreshment, the speaker gives his imagination free rein. He gesticulates as he speaks; he points the gun, he follows the falling bird with his finger, he imitates its dying flutters. In action as well as in speech he paints the scenes he hopes to see tomorrow. The company breaks up with the firm conviction that there is to be great sport—and no amount of cold logic can disturb such a conviction. Such was the nature of primitive mimetic rites. The ritual imitation of lizards and grubs was only the primitive way of *talking together* about them, and so buoying one another up.

The moral function of ritual conduct—and, be it noticed, we are here approaching the question of the function of religion—is most easily observable in the case of the group of ceremonial rites which might be termed Dionysiac or orgiac rites.

Central among them, of course, stands ritual alcoholism itself. It was, and is, astoundingly prevalent in primitive religion. Initiation, marriage, bargaining, mourning, all had alcoholic accompaniments. Many savages acutally died of the effects of drunken initiation rites. One has only to think of the Greek orgiac cults, and of the Celtic cauldron that was the prototype of the Holy Grail, to realize how long such practices persisted in higher types of religion.

Now the effects of alcohol are well known. It is what may be called a "mental poison," acting psychologically before it works physiologically. Its physiological result is the gradual paralysis of the higher brain centers, expressing itself in growing torpor; but before this phase sets in there is a phase when the psychological reserves, stimulated by the presence of the poison, open their gates and pour out a flood of psychic energy. The results are most fascinating and exhilarating. The *normal* individual is given a superabundance of force, which expresses itself through a hundred channels at once, action crowding upon action, and phrase upon phrase, so closely that they trip each other up. Ideas rush through the mind with incredible rapidity, giving one a sense of unlimited intellectual creativity. A feeling of well-being (euphoria), satisfaction, gaiety, characteristically religious in tone, comes over the mind. The real world, to which it is painful to adjust oneself, drops out of sight; one finds the whole world, oneself included, good and marvelous, and feels an expansive sense of charity toward all mankind; for drunkenness narrows the field of consciousness, and so, *like certain religious states,* diminishes the perception of the real and fills one with false, but reassuring confidence. It is not without significance that James treats specifically of drunkenness in his "Varieties of Religious Experience." To many individuals—Janet cited the case of a timid student—the first drink acts like a revelation and an inspiration. The *sub-normal* individual is often not intoxicated at all; he simply be-

comes normal for once in his life. There are psychopathic sufferers who never lose sight of their suffering and become calm, clear-minded, and capable of reflexion and attention except when under the influence of alcohol. (For details, consult Janet's *L' Alcoolisme et la dépression mentale.*)

If it be clearly evident in the case of ritual drunkenness that the function of the rite is the stimulation of the mental reserve forces and the consequent toning-up of the emotional content of consciousness, it should now become no less clear that this is precisely the function of all other forms of ritual conduct. Some of them may well be classified as forms of "social drunkenness."

Take such rites as those of the festival of "Intichiuma," described by Durkheim. Here you have certain rhythmical acts repeated simultaneously by the whole group, so that every individual gets the impression that every one else is imitating him, while shouts of mutual encouragement keep the ball a-rolling. Each member of the group is "a non-commissioned officer who feels like a general." It is vastly exhilarating; one acquires the same surplus energy and arrives at the same emotional state as that produced by alcohol, without the same danger of harmful after-effects. Similar phenomena may be observed today on New Year's Eve and other popular feastivals.[13]

Or take a totally different type of ceremony: mourning. At first blush, primitive mourning rites do not seem to be calculated to heighten the energies or to tone up the emotions to the pitch of joy. The members of the tribe strike and wound one another *ad lib.*, meanwhile tearing their own hair and howling dolorously. Yet Durkheim is right in pointing out that such rites actually have a comforting and strengthening effect, and gradually lift the mental level of the tribe, the chief mourners included, from apathetic grief up to cheerfulness. Here, as elsewhere, the function of religion seems to be to produce an *increase of power* (*exaltation de la force*) and a heightening of the mental level.

Janet takes issue with Durkheim for setting up *society* as the sole fount of religious inspiration. "Social drunkenness" is only one source of inspiration. Alcohol itself is another. Pulling the hair, or any slight infliction of suffering, as in the case of funeral rites, may also bring out the psychic reserves. It is purely arbitrary to make the sacred coextensive with the social; one could make out just as good a case for the Great God Alcohol. Who knows? A monkey who got drunk may have been the founder of the human race!

But although at this level of conduct we can discern a group of practices and beliefs out of which religion seems destined to arise, and can even determine the function which religion fulfils—the calling out of the psychic reserves and the heightening of the mental *tonus*—we are not yet able to distinguish religious conduct from other types of conduct which fulfill the same function. We are still in the period of pre-religion, when religious beliefs and practices are still spasmodic, and not consistently carried out. There is a perpetual rhythm in the life of the savage, which carries him back and forth between the inspired, wildly enthusiastic, irrational conduct of the religious feast-day and the sober, utilitarian conduct of everyday life. There are physiological limits to religious frenzy, and the savage,

[13]A friend who sat up all night in a Paris café on New Year's Eve told me that some who drank scarcely a drop were as drunk as the rest—"socially intoxicated." Cf. negro revival-meetings.

on the whole, is pretty sane in observing those limits. Between religious spasms, he returns quite frankly to a common-sense basis of living, and does not let logic disturb him: though his myths may affirm that the Fish, the totem of his tribe, is everywhere, he acts ordinarily as if fishes were to be found only in the water.

Yet there always remains the possibility of taking religion too seriously: of carrying out religious practices and beliefs to the point of logical consistency, and so of passing over the borders of common sense into a world where fancy reigns supreme. Such is the case in the great age of religion, which begins with the advent of reflective thought.

V. The Reflective Level: The Great Age of Religion

(1) *Rise of the God-idea and Anthropopathic Conduct: Prayer and its Answer*

The most striking phenomenon which greets us as we enter the reflective period is the rise of the god-idea. With Leuba, whose description he follows quite closely at this stage, Janet considers that there is no real religion where there are no gods. An analysis of the god-idea, and an explanation of its rise and function, are hence essential for the understanding of religion.

The characteristics of gods, or "spirits", may be summed up as follows.

(1) A spirit is a *man*. To man of this period, the gods are thoroughly anthropomorphic and concrete. You may argue that they were only psychological entities; but to the believer in gods, as to the believer in healing springs or osteopathy, the helpful effects which really issue from his belief seem to issue from the object of his belief. Gods, then, behave like physiological entities: they fight, they speak, they give orders and punish the disobedient, like other men.

(2) A spirit is *powerful*. This does not imply infinite power, or any other kind of infinitude. In actual practice, the gods are always finite; each man has *"un petit dieu à son usage."*

(3) A spirit has a special *function*, incapable of performance by an ordinary human being. These functions vary with the needs of the worshipper. Each one is commonly attributed to a different spirit. The unity of God is a philosophical abstraction; real gods are always plural. "Man is not a monotheistic animal" (Guignebert, in a lecture on Hebrew Angelology).

(4) A spirit is *invisible*. Not that one would wish to have it so; but since the gods do not exist, it is obvious that one can go on believing in them only on this hypothesis.

The behavior which goes with the god-idea has been excellently described by Leuba under the name of "anthropopathic" behavior: acting as if an invisible human being were there. The believer *honors* the gods as he flatters men; *humbles himself* as before a chief; *prays* for favors; *gives thanks* for past favors, etc. Such a description is inclusive enough as an account of the *believer's* conduct; but meanwhile, what is the god doing? He must necessarily be answering to all this on his side, else the whole conversation would lapse. Of course he cannot answer directly, being non-existent; but that is not an insuperable difficulty: he answers *through the believer*, or at any rate through the man who specializes in belief, the priest.

The priest is the differentiated function of making the god answer (*faire parler le dieu*); but this differentiated function could never have evolved unless it were implicitly capable of being exercised by each believer. The priest, moreover, would never be believed when he speaks for the god, did he not himself believe he heard the god speaking in him. It is important to add this unique type of conduct, *making the god speak*, to Leuba's list, if we are to get a complete view of the religious process.

How did these strange beliefs and practices arise? The explanation depends upon an analysis of the new characteristics of conduct at the reflective level. Here the new and significant phenomenon is *thought*.

Thought may be said to develop out of action by way of language. Before the rise of language, a man's thought about an object is simply his behavior toward it, and his thought about his neighbor is just his attitude toward him: a man's thought is the *ensemble* of his actions. Ideas or mental images of course exist subjectively in the mind; but they are bound up with attitude or nascent action so intimately as to be a part of it. Speech, however, introduces a new element into the situation. At first, at the assertive level, it is merely the accompaniment of action, spontaneous and uncontrolled. One can trust the child or the savage at this stage to reveal his real motives with absolute frankness. It is a momentous departure when he begins to "talk to himself" instead of "thinking out loud."

How momentous it was may not at first be self-evident. Thought is only speech reduced to the point of imperceptibility. You cannot think the letter *L* without moving the tip of your tongue; many people never read or think without moving their lips. There is only a difference of degree between a loud command, a soft whisper, and a silent thought. Yet how primitive society must have been upset when silent thinking first became prevalent! Henceforth one never could know how to deal with a man; he might be saying one thing and thinking another. The advent of the secret and the wilful lie mark a great stage in human progress.

The idea of a *double*, or spirit, existing invisibly behind that *ensemble* of visible actions that I call my neighbor, was the inevitable outgrowth, *not* of the dream experience, as Tylor thought, but of something that bulks much larger in the life of every man as soon as he begins to think; *duplicity* or double conduct. Animism springs up spontaneously at the moment when you first learn the necessity of distinguishing between the man who talks and acts as if he were your friend, and the invisible, inaudible enemy who lurks behind him. Previous to this there could be no conception of spirits inhabiting stones and trees, for there was no conception of the human spirit as distinct from the human body.[14]

But how did the idea of god-spirits grow out of the idea of human spirits? The connecting link was *conduct with respect to the absent*. The animal is able to assume attitudes only toward that which is present; no physical object present, no stimulus. In man, however, memory keeps up the stimulus; it may be defined as conduct with respect to the absent. Now when the idea of an invisible double arises, it removes much of the barrier that separates conduct with respect to the present from conduct with

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[14] Janet remarked that the whole idealistic tradition in philosophy (*le spiritualisme*) had sprung from this primitive and naive distinction between body and soul (action and thought). As a matter of fact, he said, thought is not a separate entity at all; it is "a little fragment of conduct." It would appear that Janet has not only a behavioristic psychology but also a behavioristic metaphysic.

respect to the absent. In both cases the same effort of the imagination is required; it is to my mental picture of my invisible neighbor that I adjust myself in either case.

A further step: the *dead*, so far as I remember them, belong to the same category. They, like my absent neighbor and the spirit of my present neighbor, are invisible; they affect my conduct just as much as do the merely absent, so long as I remember them. It is commonly recognized, that primitive men, as well as animals, behave toward the dead exactly as they do toward the absent—even to writing letters to them, and taking precautions against their return.[15] The Australians have a curious custom that well illustrates the primitive view of death. They have two sets of burial rites, one at the time of death and the other some time later, at the close of the epoch of mourning. The first is to celebrate the death of the body; the second, the death of the spirit. During the period of mourning, the memory of the deceased has been kept alive; he has been looked upon as merely absent. Now the family is free to forgot; and this is the second death, the death of the spirit. But there are those who do not forget, and who continue the absence-conduct with respect to the departed long after the laying away. A whole society of spirits thus grows up about every hearth and every tribal group—dead, but still active in men's minds. This it is which gives birth to animistic, anthropomorphic *religion* in the strict sense of the word.[16]

So far we have merely explained the belief in the existence of disembodied spirits. We have not explained why these spirits came to play an important part in human affairs, and why exalted functions came to be ascribed to them.

If we look back upon the history of mankind, we shall see the gods becoming more and more inextricably involved in all man's efforts and aspirations, and assuming higher and nobler functions as man's moral nature evolves. All religions are *alliances* between a god and a group, but the human interest on which the alliance is based varies. At first it is based on fear: "Don't frighten us; don't hurt us; give us security!" The first gods were ill-disposed toward men, and this was the best that could be hoped from them. A new type of alliance became possible when there were some good gods to set over against the bad: "Help us to fight against our enemies and against evil gods!" The god becomes the general of the group; direction, guidance, is what is asked of him. Next comes the period of prayer in the proper sense, when all sorts of services and presents are asked of the god. Finally comes the period when, taught by experience, the worshipper asks only for spiritual goods: "Give me moral strengthening" (*le réconfort moral*)! In all ages, however, *alliance with spirits* has been the central religious fact.

What are the psychological motives of this quest of a spiritual ally? They are very simple: the craving for direction and the craving for love. Direction may be defined as a form of authority mitigated by the benevolence of the leader. It is enormously satisfying and "cheap" to the person

[15]Lévy-Bruhl, *Les Fonctions mentales dans les sociétés inférieures*, 360.

[16]It will be seen that Janet regards ancestor-worship, not nature-worship, as the primary form of religion—quite like Tylor and Spencer. He makes no attempt to account for the origin of nature-gods. Would he say that they are simply ancestral spirits that have taken up their abode in some specific natural object and become associated with some specific natural function; or would he say that the conception of double or spirit, first formed to account for the double conduct of men, was then applied, by analogy, to natural objects?

guided. Mental diseases are often temporarily relieved when the patien comes under some strong authority, as when he is called for military ser vice or enters a religious order. The feeble-minded are peculiarly subjec to *Schwärmerei* for some pet father-confessor or spiritual director, and ofte become violently jealous of rival enthusiasts. The craving for love may b regarded as a variant of the craving for direction. It is not based wholl on the sex-instinct, as Freud thinks; it is a demand for aid in all the acts o life. The need for courage, for gaiety, for distraction, play a part in lov quite as considerable as the sex need.

None of these sentiments, be it observed, is specifically religious There is no specifically religious sentiment; the only thing specific abou the religious sentiment is its object, which is a spirit instead of a huma being. But why cannot the sentiments just described be satisfied wit human objects? To ask the question is to know the answer: becaus human directors and human friends are all too fallible and imperfect, an fortune often takes the best of them away from us. What ordinary huma being is clairvoyant enough to know precisely what to tell me to do?— particularly since I have usually made up my mind beforehand, and wan to be told to do just what I have decided to do! Perfect comprehension o all that I hope and fear, perfect appreciation of all my half-smothere efforts and hidden possibilities: this is what I crave of my director and m friend—and where shall I find such an one? After all, a visible but unidea friend is inferior to an invisible but ideal one. What everyone seeks, then is an ideal, invisible, all-powerful, all-comprehending director and friend a god. The need for direction and friendship finds its perfect satisfactio in religion; it is no accident that so many disappointed lovers turn t religion.

We have explained why it is natural for men to seek a divin ally to meet their needs, but we have not yet explained how th god actually meets them; how he answers the prayers that as cend to him. The question is highly important; we are not t suppose that religion ever could have persisted if the gods ha never spoken. We have come, then, to the point when we mus explain our phrase "making the god speak," and show how it i done.

To understand the psychology of prayer—for that is the question no at issue—we must remind ourselves once more that we are at the reflectiv level of conduct, the level where thought arises; and we must remind ou selves of another characteristic of the the thought-process: its *converса tional* structure. Reflective thought is nothing more nor less than a *interior discussion,* modelled after the audible discussion of the marke place and the household, which preceded and provoked it. How is interio discussion possible? By the evoking of the adversary, real or imaginar whose desires and intentions stand contrasted with those which struggl for expression within us at the moment. "We have a parliament withi our head;" and in the debate we impersonate first one orator and the another. All thought is dramatic; but prayer differs from ordinary though in that the drama is taken seriously. This, to be sure, involves a tin *mensonge à soi-même*: but lying, after all, is a highly economical type c conduct, and lying to yourself—particularly when the falsehood is pleasan —is clear gain, for you are in no danger of being contradicted.[17] And ther

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[17] Janet described the case of a young lady who wrote love-letters t herself, and half-deceived herself into believing in their genuineness, as we as wholly deceiving her family. She wept bitterly when the deceptio was discovered, and said the letters had given her great joy.

have never been lacking certain psychopathic persons—mediums, visionaries, dervishes, and the like—in whom the divine half of the conversation has appeared to be automatic and wholly independent of the control of the individual, and so authentically and convincingly divine.

Janet described at length striking cases of pathological impersonation which have come under his observation at the *Salpêtrière*. One young girl is convinced she is under the special protection of Ste. Philomène. After praying to the saint in her normal voice, she goes into a state of somnambulism, in which her whole voice and aspect changes. During this state she disposes pebbles in the form of a cross and makes other signs (as previously requested) to denote that the prayer has been favorably received. She then awakes, and shows every sign of delight at her prayer having been answered. A more extreme case is that of "Madeleine," who alternately plays the rôle of the worshipper at the foot of the cross and of the Christ on the cross—till she has developed a perfect set of stigmata—and who also alternates in the rôle of Mary and the Child, God's wife and God the husband, God's worshipper and God the teacher. In this last rôle she pronounces parables addressed to all mankind—some of them not bad—with a confidence that reminds one of the "thus saith the Lord" of the prophets.

The scientific study of such cases, remarks Janet, dates only from the development of Spiritualism and Psychic Research, about 1850; but the phenomena are as old as mankind. Mental alienation in all its forms has always been regarded as divine; and the mentally alienated have always been the chosen mouthpieces of the gods. This does not always mean permanent mental alienation; in Madagascar, for example, a whole tribe intoxicates itself with a kind of beef-blood broth, after which different individuals in turn, as they reach the pitch of frenzy, impersonate the god and speak for him to the rest. It was out of such religious impersonations that drama developed.

In all such divine utterances, strangeness (*bizarrerie*) and complication are marked characteristics, and increase—as in the case of mediums—according to the number of believers who are present. Dead languages and archaic terms play a great part in religious revelation. "All this," benevolently remarked Janet, "is not *tromperie*, as the 18th century philosophers thought; it is necessary, to distinguish ourselves from the god."

(2) *Psychological Analysis of Some Typical Religious Phenomena: A. Religious Faith: Its Loss and Restoration*

There is no religious phenomenon that throws so much light on the nature of religion as faith; and there is nothing that throws so much light on the nature of faith as scepticism. By scepticism we do not mean the scepticism of the "tough-minded," who have found their moral tonic outside of religion, and can discuss religious issues flippantly, but rather the scepticism of the sincerely religious,—always a painful thing. The two types of scepticism, like the two types of belief which are their opposites, are quite unlike, psychologically. Keen religious thinkers have long insisted, with Pascal, that religious truth is different from other truth, being discerned by faith and not by reason, by the heart and not by the head. Psychological analysis proves that they are perfectly right.

Religious scepticism, as a matter of fact, is commonest where one would least expect it: in the monasteries. Tardieu in his book on "*Ennui*" has described very carefully a mental disease which he calls *asedia* or "mental incompletude," which was rampant in the monasteries in the

Middle Ages. It is characterized by loss of the sense of reality—not only God's reality but also the world's reality. In some cases the victim came to doubt the existence of the other monks. It was probably to combat the creeping-on of this mental numbness that the monks resorted to flagellation and other extreme forms of asceticism.

Janet has himself observed three stages in the progress of this disease. In the first stage the patient—usually a stanch believer with no intellectual difficulties—complains that his love for God has vanished ("*Je n'aime plus le bon Dieu*"). He believes in God, but no longer takes any pleasure in the thought of Him. Prayer sounds hollow, and is accompanied by no "emotional harmonies." The case is precisely similar to that of the man who suddenly loses all affection for his family, and to whom the whole world becomes emotionally neutral. The second stage is that of scepticism proper. Belief in God vanishes, "like a light receding and going out," as one patient put it. Often no intellectual cause can be assigned. The third stage is that of the *inversion of the sentiments*. Bunyan's impulse to blaspheme the name of Christ is a case in point. The patient is seized with the obsession to contradict just those beliefs that are most precious to him. It is a horrible experience, unknown to the intellectual sceptic, who is usually untroubled by his doubts.

The psychological explanation is simple: it is purely a matter of the progressive impoverishment of the mental energies. Religion brings force in the end; but the initial act of belief which sets God before the mind takes a certain amount of energy, like any other act of reflection. Getting God's answer takes more. Some are so mentally depleted that even this slight strain is too much for them. When the patient has *just enough* strength to summon up the idea of God, you have stage (1); when he has not quite enough you have stage (2), in which the *word* God is brought before the mind, unaccompanied by the arguments for His existence or the recollection of past benefits; when mental bankruptcy is close at hand, and would ensue if the effort to believe were carried through, you get stage (3), in which the patient flees from the act of belief, impelled by an unconscious instinct of self-preservation.

The cure of this disease, involving a recovery of faith and a new sense of mental power and stability, is what is known in America as "conversion." Janet expressed some surprise that American psychologists of religion should have given so much of their attention to so simple a phenomenon, and remarked that outside of Anglo-Saxon countries such experiences tended to have a less specifically religious connotation. He classified James' cases into "little conversions" (involving the reformation of drunkards and other moral delinquents), and "big conversions" (from doubt and fear to faith and confidence). The secular labels for these experiences would be *resolution*, and *the cure of mental depression*. Janet has often observed such cures brought about by purely physiological means. Moral influences, of course, are a great aid. General Booth hit upon the most powerful of these influences when he remarked that the most helpful thing in the world to a degraded man is the belief that some one else is interested in him. This friend, added Janet, need not necessarily be a human friend; the idea of an ideal, divine Friend, powerfully suggested to a man's mind, is even more effective.

B. The Social Psychology of Faith: Fanaticism and Proselytism

The nature of religious faith is again revealed in the attitude which the religious believer takes toward unbelievers. Just as the psychology of the religious sceptic differs from that of the

philosophical sceptic, so does the psychology of the "defender of the faith" differ from that of the advocate of a philosophical theory.

In a purely philosophical discussion each disputant is at the start convinced of the truth of certain propositions, and yet he is resigned to the possibility that he may be defeated in the argument, and have to change his convictions. The reorganization of one's convictions under the influence of new truth is indeed a fatiguing operation, but to a healthy person it is essentially pleasurable, and to be welcomed on the whole. Hence the philosophical disputant is polite to his adversary—knowing he may be the victor—and observes the rules of the game with strict honesty, much as if he were playing a friendly game of cards, except that the arbiter in this more dignified game is not Luck, but Truth. Unless these virtues of resignation to possible defeat, respect for the adversary, and intellectual honesty are present, philosophical discussion comes to naught.

In a religious discussion, on the other hand, precisely these virtues are characteristically lacking. Dishonesty of the most flagrant sort is rampant in religious discussion; time and time again has Janet discovered incorrect citations in apologetical writings. As for politeness, one hardly needs to insist upon the fact that religious writings combine a ludicrous pride with an equally ludicrous scorn of the adversary. The name of the "-ism" one is defending is always accompanied by resounding epithets,—"*l'auteur se grise de mots sonores*,"—while no religious writer can mention materialism or atheism without calling it names: "abject materialism," or what-not.[18] As a typical instance of the religious man's lack of scientific resignation, Janet quoted a phrase from a letter sent him by a man who was taking the course: "Whatever you may say, you will change nothing in my religious convictions." The religious disputant is like a child who will not play unless he wins. The conclusion is obvious: in religious discussion we are not engaged in a mere playful skirmish; we are fighting for our lives. Religious discussion is less like a friendly game than like a hand-to-hand grapple with a brigand who threatens our life and property. In such a struggle, all rules go by the board.

But if the aggressions of unbelief are a matter of life or death to the believer, the aggressions of religions are—quite literally, in many cases—a matter of life or death to the unbeliever, and to adherents of rival faiths. Proselytizing zeal is a characteristic mark of every real religion; and proselytism means determination to convert by hook or by crook, by reason or by force, and never to rest content while a single heathen remains outside the fold. It is noteworthy that conversion by reasoning is very rare; the sentiments must always be appealed to. In earlier times converts were more apt to be *frightened* into the fold; now they are more apt to be *seduced* into it by the promise of benefits.

Miracles have figured enormously in evangelistic arguments. They are not to be defined as inexplicable, irreproducible events (here Janet recanted an earlier definition of his own). They are essentially events following some religious act and bearing the official religious signature. A woman who recovered her speech in some strange fashion that science could not explain would not necessarily call it a miracle; but if the mysterious healing ensued upon a pilgrimage to our Lady of Lourdes, it would be a miracle. For us today, inexplicability is the signature of the divine *par excellence*; but it was not always so. For our simple-minded girl, who typifies the primitive attitude, the pebbles laid *in the shape of a cross* were a sufficient signature of the divine. Miracles tend to be the court of last appeal in arguments between an evangelist and his rival.

[18]Cf. what a Catholic friend said to me of Janet himself: "*C'est un matérialiste de gros ventre.*"

But when arguments break down, what then? For a scientist or a philosopher, the failure of a proposed miraculous test would be conclusive, but for the evangelist anger, not resignation, is the reaction. All other methods failing, *persecution* is resorted to. All real religions have inflicted martyrdom as well as suffered it. If Protestantism has indulged in persecution in spite of its principle of individual testing of truth, that simply proves, remarks Guyau, that it is a religion and not a philosophy.

How explain religious persecution? Murisier, in his "*Maladies du sentiment religieux*," has attributed it to an instinctive impulse of self-defence. We suffer if we cannot unify our beliefs into a system, particularly if this system of belief is what we live by. The existence of persons who dispute our convictions breaks up this unity and causes great pain, especially to the weak and sensitive, and those who are already troubled by doubts. It is the doubter to whom his neighbor's unbelief causes the most acute distress; such phenomena as the Inquisition were not the result of an excess of faith, but of apprehensive uncertainty in an already shaken Church.

Janet thinks Murisier's explanation of persecution on the whole sound, but would add that the desire for mastery that is at the root of imperialism is also influential. Your converts give you a feeling of power and increase your own faith. Desire for intellectual unity, desire for relief from mental depression, and desire for the encouragement and sense of power that others' agreement brings with it: these are the three chief motives of persecution.

C. Demonic Possession

The phenomenon of demonic possession is found in all religions, and may be regarded as the characteristic religious malady in the life of the individual, as persecution is in the life of society. Wherever you have the belief in spirits you find cases of it; and if it is uncommon today in civilized society, except among Spiritualists, that is only what is to be expected.

Demonic possession may be classified as a derangement of the active functions. In contrast to certain manias in which the patient is acutely sensitive to external criticism, the possessed man realises that his distress has its source within him; and yet the acts which trouble him, performed obviously by himself, are so contrary to his own habits and character that they seem to be those of a foreign person, hostile to himself, who has somehow got possession of him. This is the interpretation universally made wherever the belief in spirits exists.

Possession may be best understood if we compare it with prayer. Both are double conducts, in which the subject plays two rôles; but in the former case the subject considers his second personality to be evil (a devil or demon), while in the latter case he is sure it is good (a god, or saint).

Why is this? Well, to start with, the physiological and moral effects are bad in possession and good in prayer. Janet's patient who held converse with Ste. Philomène also held converse with a bad spirit whom she called Joseph; Ste. Philomène made her healthier, morally stronger, and happier; Joseph gave her indigestion and bad circulation, and tended to break down her will, making her suggestibility rise and reflectiveness fall. Thus she had no more difficulty in discerning the two than had Ste. Teresa in discerning the good God from the evil spirits.

But why have possession and prayer opposite effects? It is partly due to the content or character of the personality evoked, but chiefly because in prayer one remains master of the inward drama—the divinity's speech can be stopped at will—while in possession the second rôle gets beyond

control, and the first rôle disappears. A demoniac is like a spectator at the theatre who gets so engrossed in the play that he thinks the hero's life is really in danger, and shouts "Stop!" to the villain. Possession is simply a case of bad prayer.

Prayer, or the carrying on of two rôles, is indeed a difficult and dangerous operation. If you don't succeed in making the second personality real, you get "spiritual dryness," the first stage in *asedia*, as we have already seen; while if you make it so real that you get carried away with it, you find yourself possessed by a demon. Because of this double danger, religion has always sought to take away from the average individual the function of being his own God, and getting his own answers to prayer. The priesthood tends to acquire a monopoly of divine revelation, and it is always hostile to the prophet, who claims to receive direct revelation from official channels.

D. Ecstasy

Religion has always attributed great importance to certain pathological mental phenomena; but as fast as science explains these phenomena, she tends to retreat to other strongholds of inexplicability and abnormality. In antiquity, idiocy was looked upon with awe; fifty years ago, it was somnambulism that forged to the front; today, since somnambulism has been explained, it is ecstasy that is regarded as the authentic form of communion with the divine.

But ecstasy is just as susceptible of psychological explanation as somnambulism, if we will only turn from the accounts given by 15th and 16th century mystics of their own experiences—accounts confused by the use of traditional literary phrases borrowed from other mystics—and study ecstasy in its present-day form, as it appears in the psychopathic clinic.

Ecstasy has many forms, as various as the mental content that is brought into it. In addition to religious ecstasy, one might enumerate social and political ecstasy (cf. those of Nietzsche and J. J. Rousseau), amorous ecstasy, and literary ecstasy. As a typical example Janet preferred to take one of these religiously neutral cases, that of a young man who was seized with the idea that he might become a great poet, and who, as he finished his mediocre little volume, was filled with such a sense of exaltation and illumination that he felt "obliged to close the blinds" lest the neighbors should see the radiance that surrounded him. The experience so convinced him of the excellence of his verse that he has continued to write for five hours a day the rest of his life, although he has never had a thing accepted; and he has often declared to Janet that the glory of Victor Hugo and of Napoleon is not to be compared with his. If one remarks that his "glory" is not very evident to the casual observer, he lays his hand upon his heart and remarks with an ineffable smile: "J'ai *la gloire*."

Beneath all the various forms in which the ecstatic experience appears there is a constant psychological and physiological pattern. Physical movements are reduced to a minimum; the patient wishes to be let alone. The psychological *tonus* mounts up, and a wave of calm, passive, beatitudinous *joy* sweeps over him. The actions become unreflective, and hence *automatic*. The subject feels inspired, uplifted, *illuminated*. Literal photisms sometimes occur. An emphatic *conviction* grows that whatever happens to be in the mind is true, and immensely important. Complete conviction would be objective and active; ecstatic conviction is not really complete; its forcefulness is due to the fact that the patient blurs the distinction between objective and subjective, as the young literary man

blurred the distinction between objective and subjective "glory." So in religious ecstasy one feels the "immediate presence of the divine," but objects to raising the question whether the divine is objective or subjective.

Ecstasy in some ways resembles somnambulism, but differs from it in that the memory of the experience is preserved, and the effects of it often last all through life. It is best classified—paradoxical as this may seem—under the head of *agitation*. It belongs to a small group of manic disturbances in which—unlike other mental disturbances—joy and not depression, certitude and not doubt, power and not weakness, are the prevailing notes. It differs, however, from these cognate maladies in that the maniac expends his increased energy in motor agitation, while the ecstatic is motionless; and so the forces which would have been expended in activity heighten the subjective experience of joy and certainty to a much greater degree than in the active forms of the disease. In both cases a fit of depression or "dryness" follows the experience, showing that the mental reserves have been heavily drawn upon.

The value and dangers of ecstasy are equally evident. It is an experience that religion treasures, and mistrusts. It is treasured; for religion treasures all that adds to personal happiness, which is religion's goal. It is mistrusted; for the natural outcome of ecstasy in its extreme form is the belief that I, the ecstatic, *am* God, which gives me such absolute confidence in myself that I am apt to become a religious revolutionary. Hence the cautiousness with which Mysticism has always been handled by the Church.

(3) *Do the Gods Exist?*

Our study of certain religious phenomena,—particularly the phenomenon of faith,—while it has certainly deepened our impression of the *value* of religion as a moral stimulant, has undoubtedly raised in our minds the question of the truth of religion. It is the time now to examine the metaphysical affirmations of religious belief, and to ask ourselves whether the gods who function psychologically in the human act of prayer really exist, or are simply ideas.

If we are to ask this question intelligently, observed Janet, we must not fail to realize that the verb "to exist" has different meanings at the different levels of conduct, and that there are different sorts of existences corresponding to these different meanings. We may distinguish, for example, between *things* and *facts*. A thing is something that is real at the perceptive level; it exists inasmuch as it is perceived. A fact is something that is real at the experimental level; it is believed in because experimentally verified. Religious entities are neither things nor facts; they correspond rather to the assertive and reflective levels, and have a different mode of existence: they might be termed *beings* or *realities*.

Belief in a fact of science and belief in a religious reality are two entirely different matters. In the former case, belief comes step by step, beginning with the bare hypothesis of existence and gaining in strength bit by bit as the hypothesis gains confirmation from experience. In the latter case, belief comes all at once, *a priori*, and no amount of experience can discredit it. Religious believers are like the young lady to whom it was hypnotically suggested that she had undergone an operation of a somewhat major nature; her head had been amputated. The belief so crowded other possibilities out of her mind that when brought before a mirror she saw her reflection only up to her neck, and when asked to feel herself and see if her head were not still there, she never moved her hand up beyond the supposed place of amputation. In other words, she ad-

mitted only positive instances, confirming the hypothesis, and no negative instances. The argument "How can you *see* that your head is gone if you have no eyes," had absolutely no effect upon her; she was argument-proof as well as experience-proof.

We have mentioned that religious belief comes all at once; we may add that it tends to go all at once. "All or nothing," says the religious man; "either my belief is absolute, divine truth, or else it is a lié." There are no degrees of probability in religious truth; when it ceases to be considered absolutely certain, it is violently repudiated, and the loss of belief in it is often accompanied by nervous collapse. Scientific or philosophical truth does not thus engage our loyalties; one would never die for a scientific theory or a philosophical proposition; but for a religious belief one may die as one does for country. We believe in "realities" more violently than we believe in facts.

But the most momentous differences between facts and realities is this: facts are believed-in because they exist; realities exist because they are believed-in. If you ask *why* I believe a fact, I very properly proceed to "prove" it to you. If you ask "why" I believe in a religious reality, I shall, if I am honest, tell not my *reasons*—they are for my neighbor, not for me, and never convinced anyone anyhow—but my *motives*. Religious beliefs arise at the assertive and reflective levels of conduct, where desire and belief are identical. What I wish were true, that I believe to be true. The gods are realities inasmuch as they give perfect satisfaction to a fundamental, human need: the need of alliance, of friendship, of companionship and direction in the struggle of life.

Today there can be no question of the fact that religious beliefs are being disintegrated by being forced to live in a philosophical and scientific atmosphere. Religion is a synthetic act, philosophy is an act of analysis; and wherever analysis is applied to religion, it withers away. For example, reflective analysis shows us that the primary religious act, prayer, has three moments in it: the worshipper's petition, the worshipper's response to his own petition, and the glow of satisfaction resulting therefrom, which starts the process circling round a second time, and so on. But the worshipper never analyzes his act; he takes it as unitary experience, bringing satisfaction and *hence* justifiable. Let him once analyze it, however, and at once he perceives how fearfully problematic is the supposed "reality" he meets in it. Stop the Egyptian priest at the moment when he is about to speak through the tube that leads to the mouth of the image of the god and makes the temple to resound with the thunders of the divine response; ask him whether it is really a god or a man who is speaking; stop our feeble-minded girl at the moment when she is arranging the pebbles in the shape of a cross, and ask her whether it is really Ste. Philomène who is doing it; and you will forever destroy the religious faith of that priest and that girl. Analysis kills religion.

And yet there lingers in the minds of all of us a predilection for the old beliefs; they are part of the habits and tendencies that form our social heritage; and they are so true to human desire that the "will to believe" half convinces us they are true. Many a hard-headed, free-thinking old peasant turns to the Church in time of bereavement for the comfort he cannot find, and sinking himself for the time below the level of analysis, finds in the mysterious drama of the Mass—the living presence of the *Bon Dieu*, the promise of resurrection, the august countenance of the priest, the silent sympathy of his neighbors who crowd the aisles—an experience of renewed courage that half convinces him as it fully consoles him.

(4) *The Fruits of Religion: Morality and Logic.* Before we turn our attention to the period of religious disintegration at

which we have hinted, it is but proper that we should recognize the services which religion has rendered to contemporary civilization. Foremost of all is this: it has *made* morality in the modern sense.

Some years ago there was a significant controversy between Spencer and Paul Janet[19] on the origin of ethical duty. Spencer said that duties are simply commandments of the chief, in the first place. Paul Janet questioned this. (1) Moral commands, he pointed out, have a certain *dignity*, a categorical quality, which the chief's commands do not always possess. One may question the rightness of the chief's command; one never questions the rightness of a real moral command, even when one disobeys it. (2) Obedience to the chief is conditioned on his *presence*; obedience to a moral command is just as imperative in secret as in public. (3) Obedience to the chief involves *ennui*, and even *humiliation* before both the chief and one's fellow slaves; obedience to the moral law brings *pride*, accompanied by a feeling of censorious contempt for the disobedient. "*La vertu vous égale à Dieu*"—you feel like a master, not like a slave.

Both Spencer and Paul Janet were too anxious to form an artificial cleft between religion and morality to perceive the obvious solution to all their difficulties; *viz*, that duties are the commandments, not of the chief, but of the gods (who, to be sure, speak mainly through the chief). Against this hypothesis, all Paul Janet's objections are powerless. (1) The superior dignity and categorical quality of duty are due to the fact that the gods are more powerful than any chief; a god is to a chief what a general is to a corporal. (2) It is obvious that, since the gods are invisible and *may* be watching you at any time, resistance against them is impossible; and so one obeys in secret as in public. (3) As for the pride that accompanies moral obedience, it is due to the fact that the god's command is uttered within us, invisibly, and we get the sense that we are commanding as well as obeying. Since the gods are psychological entities, moral autonomy is a fact.

Ever since the rise of religion, morality has borne the religious stamp; and in the great epoch of religion it was simply an appendage of religion. It is absurd to deny this historical fact, as Spencer and Paul Janet and that whole generation of philosophers tried to do. A striking confirmation of it may be found in the pathology of the moral experiences, which is substantially identical with that of the religious experience. Moral doubts and "scruples," moral "dryness," correspond to religious *asedia* and may go to the same extremes. Corresponding to demonic possession we find cases of obsessing "voices" that tempt the conscience satanically. Corresponding to religious ecstasy we find a form of moral ecstasy in which one becomes convinced that one is morally perfect. Morality has much the same function as religion; it *comforts* and *directs*. Obviously it is a simple outgrowth of religion. To try to keep morality alive (in its traditional form) while destroying religion is to try to keep a tree alive after cutting its root.

The type of morality fostered by religion may be best described as egoistic. This is not a depreciatory term. Egoism is a great step forward in moral progress, and does not arise until the reflective level is reached. Hitherto man acted on each desire as it arose—only to regret it afterward. Now he learns to call all his desires into consultation before making any decision, latent as well as clamorous desires, and learns to balance the future against the present. This "calculus of interests," as Bentham called it, is enormously difficult; neurotic people are incapable of it; for them, egoism

[19]Paul Janet (1823-99), the well-known author of *Les causes finales* (1874), was the uncle of Pierre Janet. They collaborated in some experiments in telepathy.

is an ideal so high as to be practically unattainable. Religion has here furnished its greatest contribution to moral progress, the greatest "aid to reflection" ever devised: the fear of future punishment. Future pain needs to be exaggerated in order to make it counterbalance present pleasure in the moral calculus. This is just what religion does, with its picture of Hell. Thus, subordinating and organizing his desires, man for the first time becomes *ego*, an individual; and this achievement is in no small measure due to religion.

Logic as well as morality bears the imprint of religious influence. It is ridiculous, as a matter of fact, to separate logical laws from moral laws. Logic is only intellectual morality. The need for it arose with the appearance of language. You cannot perform two contradictory actions at the same time; matter resists you; but you can say or think two contradictory things at the same time. From the moment when man first began to speak, he began to *dire des bêtises*. Hence the regulation of language became as important to the tribe as the regulation of conduct, for uniformity of opinion was one of the conditions of social unity. The principles of logic originally took shape in moral form, with religious sanctions attached. "Do not forget your promises immediately; remain faithful to yourself for a time at least:" this was the original form of the principle of contradiction. "When your chief (behind whom stands the god) has one opinion, do not dare oppose him": this command is at the basis of all logic.[20]

VI. The Ergetic (or Rational) and the Experimental Levels: The Decline of Religion

(1) *Influences Making for the Destruction of Religion*

There are four important types of conduct which appear for the first time at either the ergetic or the experimental level: work, education, philosophy, and science. All of them are, directly or indirectly, fruits of religion; but all of them tend to exercise a destructive influence upon religion—an instance of the general law that things tend to destroy themselves by their own fullest development. A fire tends to put itself out with the carbonic acid gas it generates; and when religion creates too many virtues, they turn against her.

A leading characteristic of the morality engendered by religion is that it is *ergetic*: work and effort play a large part in it. Work may be defined as the mustering of extra force for a given result by the exertion of *continuous* effort. It is not a primitive tendency, and it plays a small part in primitive morality. It requires that an artificial force be given to ideals and aims which, lying in the dim future, have not of themselves sufficient force to sustain the inconstant will during long-continued exertions; and it requires that the passions which tend to interrupt all continuous effort be disciplined and subdued. This is precisely what religion has done with its doctrines of divine supervision and future punishment. It has led to absurd excesses of asceticism, no doubt; but the development of the power to resist pain and repress passion has been incalculably important in the progress of the race. The motives appealed to may have been egoistic; the end in view may have been the eternal happiness of the individual; but self-sacrifice and disinterested heroism have paradoxically enough been the fruits of this egoism. The truth is that all lines of conduct that are entered upon as means to ulterior ends have a tendency to become ends in them-

[20] Janet's debt to Durkheim at this point hardly needs to be pointed out.

selves. Thus the ascetic virtues laid down by religion as means to eternal happiness came to be "their own reward," and ergetic morality, in which effort, sacrifice, and altruism predominate, came to be more independent of the religion that engendered it. At last came the time when morality felt sure enough of its own strength to turn upon religion and seek to expurgate it.

One of the chief by-products of the ascetic or ergetic temper is *education.* In primitive morality, systematic education has little place. Hereditary instinct as modified by social imitation shapes primitive conduct. Real education is *work* of the most strenuous sort, involving the stifling of the inclinations at every step. As such, it is obviously a child of religion; and yet the characteristic methods of education are dangerous for religion. *Analysis,* for example, kills passion. We have already seen how it tends to destroy belief in prayer. The *syllogism,* again,—which is primarily a tool of instruction,—creates a new ascetic morality of the intellect. Love of truth emerges for the first time. Truths, unlike religious "realities," are accepted not because they please us but because they conform to certain categorical rules like the rules of the syllogism. Now for the first time falsehood is regarded with horror; *agnoscatur veritas, ruat coelum.* The consequence is inevitable: logic turns against religion's absurdities as morality has turned against its indecencies.

A period of rapid theological evolution now begins. The local gods are arranged in families and hierarchies; many of them are identified with others that have similar functions; monotheism is the eventual outcome of the process. The function of speaking for the god in oracles and prophecies tends to come under sharp scrutiny. As a result, revelation tends to be relegated to the past, and its interpretation monopolized by a priesthood. The gods themselves take on new characters; their commands become more moral and less arbitrary.

During this period of ethical and logical evolution a new phenomenon arises: *philosophy.* Philosophy is religion evolving faster than she is willing. This, at any rate, is what philosophy has been at its most fruitful periods. Cousin and his school have come in for a good deal of abuse on the ground that their philosophy was merely a rationalization of traditional dogma; but this is what the classical philosophy of Socrates and Plato really was. Any philosopher who tries to ignore tradition, and start *de novo* on a basis of pure reason, simply cheats himself, as Descartes did. Particularly absurd was Descartes' attempt to deduce such a religious dogma as the existence of God from logical principles. Religious concepts can be built up only out of religious motives.

The one really sincere and straightforward philosophical argument for the existence of God is Anselm's ontological argument. Anselm does naively what Descartes does unconsciously: he bases everything on the will to believe. For if the ontological argument be translated into untechnical language, so as to expose the psychology that lies behind it, it will read as follows: "God is so good that it would be a pity if he did not exist." It is a passional argument, arising out of the assertive level of mental development, where I believe firmly whatever I wish were true. It might be compared with the following arguments: "This wine tastes so good that it must be good for me;" "This story is so good that it must be true."

Philosophical religion is of course more moral and more logical than traditional religion; but on the whole it proves an unsatisfactory substitute. Matters are greatly complicated by the fact that the gods at this period come to be taken as *explanatory principles.* Explanation is not primarily a religious interest, but more probably grows out of magic. The function of the gods is not so much to satisfy the rational need for explanation in the case of extraordinary phenomena as to guide and sustain the believer in his

practical life. But now the magical interest comes to be fused with the religious, and philosophy seizes upon this secondary aspect of the gods rather than upon the fundamental one. The result is that although the "God" of philosophy is pure of all moral imperfections and logical inconsistencies, and although he still bears, after all amputations, a most striking resemblance to the God of religion, he is practically unsatisfactory to the religious man. Philosophical religion is like the "rational drink" which a benevolent doctor once invented, compounded of molasses and water. It looked like wine, it smelled like wine, it tasted like wine; but nobody would drink it. Our good physician, remarked Janet, had omitted one important ingredient; *"il avait oublié l'alcool."*

But finally there issues from the ascetic, ergetic impulse created by religion an enemy more formidable than philosophy: *experimental science.* Science is the most radically inhibitory influence to which human conduct has yet been subjected. It is so different from philosophy that Janet puts it on a separate mental level. Ergetic morality had already inhibited all instincts that warred against the welfare of the whole individual life. Logic had gone farther and inhibited all mutually self-contradictory beliefs and modes of conduct growing out of them. But not all even mutually consistent beliefs and not all impeccable philosophical systems *succeed,* when used as means of adjustment to the external environment. This test of success has always had an inhibitory influence on human opinions; trial-and-error conduct has always governed the ordinary affairs of life, even when religious morality was at its height; but the *conscious cult of success,* the effort to subject all beliefs to the impartial and stern judgment of external nature, was a new and momentous departure. The ego had been subjected to the authority of logic; now human logic itself was to be subjected to the overruling authority of Nature. Verification had at first been left to the individual; next the agreement of other minds had been required; now impersonal instruments became the tools of verification.

Obviously, the effect upon all types of conduct and belief was bound to be far-reaching. In morality, the scientific spirit insisted on eliminating all harmful or merely arbitrary customs and rules, and leaving nothing but reason and hygiene as the guides of life. In religion, it completely undermined the process of acting and self-deception which formed the warp and woof of the religious life. It destroyed the credibility of the miracles on which religious apologetic chiefly based its arguments; and when religion retreated into the past and entrenched itself behind the marvels of the Age of Revelation, historical science stormed the redoubt and chased religion out of her refuge. All she could do was to capitulate, accept the scientific spirit with as much grace as possible, and try to become experimental herself. Thus it is that within the last century we have seen the rise of a number of new types of religion, or substitutes for religion, all agreeing, in spite of their vast differences, in their willingness to submit to the experimental test.

(2) *The Break-up of Religion: Substitutes for Religion*

We may best understand the odd phenomena which meet us in recent religious history if we remind ourselves once more of the three moments in the act of prayer: *interrogation,* arising out of the need for direction and companionship: *answer,* coming through a secondary personality which the worshipper feels to be more or less detached from his own; *satisfaction,* or the glow of joyous feeling which ensues on all stimulation of the energies and sends one back to repeat the experience. Each one of these moments involves and reinforces the others. Now

a little reflection will convince us that religion in recent years has been breaking up into its constituent moments. The interrogatory moment, or the search for the gods, has been taken over by philosophy—and the search will go on for ever, for by philosophy's methods the gods can never be found. The responsive moment, or the answer of the gods, has been taken over by Spiritualism. The moment of satisfaction has been taken over by Romanticism or the Religion of Sentiment. We have already seen that philosophy fails to meet the characteristic religious need for guidance and inspiration; it may be ignored as a substitute for religion. Spiritualism and Romanticism, however, really meet that need to some extent, and so deserve more consideration.

Spiritualism, or the attempt to converse through "mediums" with disembodied spirits, is a very ancient phenomenon; but modern Spiritualism, which developed about 1850, is different from all that went before in that the atmosphere of prayer and belief which used to make it very easy to get spirit-responses is exchanged for the atmosphere of scientific curiosity. Modern spiritualism is analytic; it tries to isolate the answer of the god from the question of the believer, and scientifically to control the conditions of its production, with a view to proving that extra-human agencies are really at work. It feverishly seeks for mediums, or specialists in self-deception and other types of unconscious trickery, and then sternly tells them not to cheat.

The progress of investigation along these lines is gradually discrediting Spiritualism as a candidate to fill the vacancy left by religion. First it was definitely proved that the table-tipping ascribed to the "spirits" was performed by the medium. This compelled a retreat; the believers insisted that mystery still remained: whence came the ideas and the impulses of the medium, so strangely automatic? Were they not inspired in the medium by a "spirit-control"? This led to the study of psychological automatism and the whole realm of the subconscious. These studies have been very fruitful for psychology, but very disastrous for the Spiritualistic hypothesis. The only thing that remains unquestioned is the sincerity of all genuine mediums: they are people who *think* they are acting involuntarily, and not tricking.

A more wide-spread and promising movement than Spiritualism, though not so well defined, is Romanticism. Its fundamental proposition is that wherever you find joy, strength, and satisfaction, there you have the immediate evidence of the divine. As exponents of this cult Janet named such oddly assorted people as Renan, Spencer, Henri Bois, Ritschl, Newman, and James.

James is of course the classic example. Like all Romanticists he has a scorn for *rational* demonstrations of the existence of God; he does not even mention the proofs from miracle and tradition. But he fills his "Varieties of Religious Experience" with satisfied people, strong and joyous because of their religious experience, and rests the case for the validity of religion on its capacity for creating these "sthenic emotions."

In so doing, however, he betrays the weakness as well as the strength of the Romantic position, and sober religious thinkers have not unreasonably taken alarm at his book. For one thing, he has been led by a sure psychological insight to include among the joy-producing and hence at least quasi-religious experiences such pathological phenomena as alcoholic drunkenness, "anaesthetic revelations," and extreme forms of ecstasy. But this, as Vernon Lee points out in her book on "Vital Lies," is to "betray

religion into the hands of the psychologists." If all that stimulates the sthenic emotions is divine, alcohol and laughing-gas become gods, and ecstasy, which has been frowned upon by such religious leaders as Bossuet and Arnauld, becomes the very soul of religion. As soon as secular parallels are drawn to all the typical religious experiences—and psychology is capable of doing this—the collapse of this particular argument for religion will become evident to all. A still more fundamental objection to James' argument is slyly insinuated in Boutroux's admirable introduction to the French edition of the "Varieties:" do enthusiasm and joy always go with the truth? Is not the scientific spirit the negation of this spirit? How, then, is the fact that religion engenders these emotions an argument for the *truth* of religion?

Why, after all, observed Janet, should one assume that men might be brought back to religion if it could be proved that superior spiritual beings existed? Suppose the Spiritualists definitely proved the reality of "spirit-controls." Suppose James' hypothesis were substantiated; that there exist superior minds continuous with our own, that occasionally make incursions into our subconsciousness. *Should we really be happy to meet such beings*? They would be to us what we are to the animals and to men of lower intelligence; has it been on the whole a happy experience for animals and savages to come into contact with our superior intelligences? One wonders if a real race of gods would not treat us as H. G. Wells' men from Mars are imagined to do. The gods of religion are no such superhuman intelligences; they are only as big and great and wise as we wish and need them to be. They answer to our needs as only our own creations could do. Even enemy-gods like our devil are conceived to be stupid as well as wicked, so that we can outwit them and get the satisfaction of struggle and victory. An interesting book might be written on the "psychology of the devil;" he has been a great comfort to humanity for centuries. Real gods would not be so easy to outwit if evil, or to conciliate if well-disposed; there is all the difference in the world between writing a romance and conducting a real courtship.

Modern romanticism has not, as a matter of fact, been confined to those who continue to believe in the reality of spiritual beings. The search for emotional stimulation has been carried on quite outside the pale of religion, and the waning of religion has swelled the ranks of the sentimentalists and mystics. Seillière's studies on the various types of what he calls "mysticism"—which now fill twenty volumes—have given us a wealth of data on this whole subject. He mentions four distinct varieties of mysticism.

(1) *Erotic mysticism* (*le mysticisme passionnel*). Since Rousseau, a new type of love-story has pervaded literature, in which the hero and heroine are not praised for rising above passion, nor viewed with a mixture of censure and pity for succumbing to it, but in which on the contrary passion is glorified and worshipped as something divine. After Rousseau, the classic exponent of this tendency is George Sand. A cognate tendency to worship other emotions—in fact, all emotions—as divine runs through the whole literature of the Romantic period and has been revived in very recent years. It tends to become the worship of instinct, since the instincts bear the highest emotional charges.

(2) *Social, political, moral mysticism*. Here the object of worship is one's own class, nation, or race, and in certain extreme cases one's own personality (moral perfection). Socialism, nationalism, and race imperialism are cases in point. Gobineau, H. S. Chamberlain, and Nietzsche may be mentioned as exponents of this type. The tendency of all these is to strengthen the self-confidence and self-assertiveness of the individual and group.

(3) *Aesthetic mysticism.* Some people stimulate themselves by the worship of beauty as others do by the worship of their group. The desire to believe in one's own distinction and superiority is served by this cult quite as well as by social mysticism. To believe that I am an artistic genius is quite as stimulating as to believe that I belong to a superior group or race. Flaubert may be mentioned as a typical æsthetic mystic.

(4) *Philosophical mysticism.* A great deal of the philosophy that we have been flooded with since the Romantic epoch is not inspired at all by a serious desire to know, or to systematize recognized facts. It has been written to satisfy the emotional needs of the author. He has never taken the trouble to study the facts of science or to ponder upon the meaning of life; but he finds it emotionally stimulating to contemplate the Absolute—whatever that may mean—and to compose a grandiloquent tone-poem upon the theme of Creation, well orchestrated with sonorous philosophical terms.

Janet objected to Seillière's use of the term "mysticism," which he thinks should be applied only to certain specifically religious phenomena; but he recognized that all these forms of what he prefers to call "romanticism" or "sentimentalism" have something closely akin to religion. He pointed out that the pathology of all Seillière's "mysticisms" is identical with that of religion.

For example, a recurrent phenomenon in the literature and in the political, aesthetic, and metaphysical thinking of the Romantic period is *obscurity.* What is obscurity? It is not identical with abstruseness or profundity. If one ponders long enough on a difficult mathematical theorem or on a profound scientific theory, it becomes perfectly clear; but obscurity becomes ever more obscure the longer one ponders over it, for it is based on equivocation. The typical Romanticist never defines his terms, and follows an involved order of ideas that eventually throws not only the reader but himself also off the track, and leaves him free to prove whatever he wants to prove. He writes not to illuminate the subject but to intoxicate himself, and to convince himself that what his heart longs for is really so. The obscurity of Romanticism is nothing but the self-deception (*mensonge à soi-même*) which we have already found to be characteristic of religion.

But why blame the Romanticist? He is a man whose means of stimulation has been taken away by the disintegration of religious dogma, and who is frantically seeking a substitute. When religious faith collapses, some people take to drink, others take to Romanticism; there can be no doubt which is the less harmful.

(3) *Final Definition of Religion; The Future of Religion*[21]

The question may be raised whether romanticism and similar movements are not more than substitutes for religion; whether, since they perform the same stimulating function as religion, they may not be called religions—and so one might speak of some one of them as the religion of the future.

This, observed Janet, is purely a question of definition. If you consider it wise to stretch the term religion to cover far

[21]It is worth noting that for Janet the question of the future of "religion" is not the same as the question of the future of the great organized religions. Since Egyptian religion, he says, there has been no real religion. Christianity and other so-called "universal religions" are not religions, properly speaking. In them, the influence of ethical and logical criticism is already far-reaching. Would Janet say that certain elements in them may survive the death of religion?

more than its traditional territory—as Höffding does when he identifies religion with all conduct which tends to the conservation of values[22]—you may do so. Any psychological term like "association" may be taken in such a general sense that it covers the whole field of psychology; but is it wise so to take it? Is it not better to take it in some specific sense? Perhaps if we begin with a general definition of the religious function, and then proceed to specify the characteristic way in which religion performs that function we shall be in a position to judge between the merits of general and specific definitions.

There can be no doubt about the function which religion performs; it is that of *stimulation* (*excitation*).[23] In less technical terms, religion satisfies the need for more abundant life (*vivre plus et vivre mieux*); but as we have seen, the source of all that enriches life lies in the psychological reserves. The maternal instinct, the instinct for approbation, etc., are all treasuries which are refilled as often as we empty them; the problem of richness of life is the problem how best to stimulate these instincts and spend the resulting strength. One may, therefore, say, if one wishes, that religion is all that stimulates the instincts and so heightens the powers.

But this definition is obviously too broad. Religion belongs to the class of stimulants, but are all stimulants religious? Alcohol, danger, and love are all stimulants, and they produce phenomena psychologically identical with certain religious phenomena. A young American woman subject to chronic mental depression related to Janet how her first glass of whiskey raised her from doubt and fear to faith and courage—in terms strongly suggestive of religious experience. Is whiskey a god, then? No, obviously not; there has been a process of selection going on in the history of mankind, whereby certain stimulants have been recognized as inferior to others on account of their harmful after-effects; religion belongs among the superior stimulants.

We can go farther and say precisely to what specific class of stimulants it belongs: *social stimulants*. Durkheim is perfectly right in calling religion a social phenomenon, and if we follow him and make religion *the cult of social stimulation*, we shall not be far from the truth. The need for direction and inspiration through friendship, as we have seen, is the fundamental need which religion satisfies. There was a period—the period of rite and myth—when society itself, through the commands of the chiefs and the social intoxication of communal rites, supplied these needs.

But religion proper is something still more specific: *stimulation through alliance and friendship with unseen spirits*. It evolves out of the worship of society which Durkheim describes, and may still be classified as a form of social stimulation, for the relation between the worshipper and his god are thoroughly social relations; but it does not arise till the belief in unseen spirits arises, and it perishes with that belief.

[22]Query: has Janet quite accurately caught Höffding's idea?

[23]In his University of London lectures, already cited, Janet defines *excitation* more exactly than he did in these lectures. It implies a process which not merely draws upon the reserve *forces*, but heightens the mental *tension* as well, raising one to a high moral level characterized by "*des phénomènes d'adaptation et de calme*," a level at which complex moral acts become easy. It is at such moments, he says, that "the new memories and new habits are formed which become points of departure for new [and higher?] tendencies" (*loc. cit.*, 222). A very high estimate of the importance of religion and other "stimulants" is here implied.

For a hundred centuries, perhaps, men have literally *lived* upon the little psychological discovery that the energies can be mightily heightened by communion with ideal imaginary beings. Morality, logic, philosophy, and science have been the fruit of it. But the children of religion have become conscious of two vital inconveniences in her. In the first place, she is imperialistic. To feel that one has omnipotent allies is to become overbearing, intolerant, and oppressive. In the second place, she is prone to self-deception. A certain *naïveté* is required to perform the fundamental religious act of prayer, and this *naïveté* has been rendered increasingly rare since the Middle Ages by the ravages of logic and science.

Shall we then predict the rapid disappearance of religion? Not at all. *"Tout commence et rien ne meurt parmi les hommes."* Not all men have risen to the level where religion is no longer natural to them; and for such men religion, now pruned of many of its defects by long-continued criticism, may continue to be a blessing. Children all pass through a religious stage; and all of us are apt to be seized by neurasthenia and sink back to the religious level. Do not rail at the aged philosopher who turns to religion as his mental faculties decay, and who calls in a priest to comfort his last hours; he is doing a perfectly natural and inevitable thing.

But for the strong? and the disillusioned? We need not worry about them. There are all sorts of stimulants that can take the place of religion, and humanity always discovers them when it needs them. One little-noticed substitute for religion is destined to do perhaps more than all others to put religion out of style: scientific *psychotherapy*, which studies and remedies scientifically those states of mental depression for which religion is the sovereign but imperfect popular remedy. People who are mentally well do not need religion, and, as one may observe for oneself, they are usually not religious. A second substitute for religion that is increasingly coming in among scientists is the worship of *progress*. The idea of progress was hardly known in the world before the illusions of the year 1000 A.D. were dissipated; but now it is becoming central in men's minds, bringing with it the ideas of creativity and novelty, and stimulating new types of sentiment and conduct, such as toleration, respect for the liberties of others, cultivation of individual initiative, and admiration for inventors. The central maxim of this new cult may be stated in the words of Guyau, whom Janet acknowledged in closing to have been the chief influence in his own religious thinking: "to be confident in ourselves and in the world."

Conclusion

A. C. McGiffert is wont to express the opinion that in the history of religious thought the most important contributions have been made not by the men who have settled problems, but by the men who have raised problems. From this point of view, Janet deserves to be awarded a *croix de guerre* by those of us who are interested in the progress of religious thought; for he has raised enough problems to keep us busy for some little time. I do not intend to enumerate and neatly solve them forthwith;[24] this is an exposition, not a polemic, and is long enough already. Besides, it is good for our souls to let such pungent criticisms as Janet's strike in and rankle awhile. The habit of presenting the arguments of an opponent only in combination with nullifying objections is deadening to thought. I merely wish, in closing, to set the reflective processes of the reader in motion—supposing they are not already under way—by setting up a few interrogation points, and pointing out some of the significant contributions which Janet has made to the philosophy of religion.

I should like, in the first place, to raise a query with regard to Janet's account of the relation of the philosophic interest to the religious interest. He remarks, quite rightly, that the "God" of philosophy, being a mere explanatory principle, is useless as an object of religious worship; but he is mistaken, I think, when he infers from this that the explanatory motive is foreign to the religious motive.

The instinctive basis of religion is complex; and McDougall is right, I believe, in including curiosity among the instincts which go to make up the religious sentiment. Leuba has shown that the explanatory motive is one of the factors which combine to produce the god-idea. The "high gods" of certain African tribes have no other function than that of explaining the origin of creation; later in the evolution of religion, as the need for organizing the thought-world appears, the creator-gods come to coalesce with the more utilitarian gods who preside over war and agriculture and the common affairs of life.[25] Hence any conception of the divine which is to satisfy the religious interest must also satisfy the philosophical interest; a *petit dieu à notre usage* (such as Wells' God) will eventually prove as unsatisfactory to our religious cravings as a remote Creator-God who takes no interest in us, for he is nearly as much at the mercy of the cosmic forces

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[24]Thus for example, I shall not even raise the question of the validity of the behavioristic psychology as a final, metaphysical account of personality, although I am convinced of its inadequacy, and feel that much of Janet's argument would lose its force if his behavioristic presuppositions were criticized. I leave that critique to more competent psychologists. It is true that in his articles in the *British Journal of Psychology* Janet asserts that he does not claim metaphysical validity for his psychology; but my impression was that in his course at the *Collège de France*, in spite of half-ironical disclaimers, he was really venturing into the field of metaphysics.

[25]See also G. Belot, Note sur la triple origine de l'idée de Dieu, *Rev. de Mét. et de Morale*, 16, 1908, 717ff.

as we are. The problem of the place of man in Nature is both a philosophical and a religious problem; and the question of the possible existence of a higher mind than ours in control, or partial control, of cosmic evolution is not so irrelevant to the religious question as Janet would have us believe.

Again Janet remarks that philosophy and science, in so far as they are analytic mental processes, tend to destroy religion, which is a synthetic mental activity. He infers that as the rational mental level, where analysis arises, is higher than the level where religion arises, religion is destined to disappear in the natural course of mental evolution—surviving only in children and neurotics.

But, if this be true, humanity has a dark future indeed; for religion is not the only synthetic mental activity that is destined to disappear. The appreciation of poetry, painting, music, and the beauties of nature, as well as the appreciation of moral grandeur in other human personalities, and the pursuit of ideal perfection in our own,—all this, too, is destined to vanish, for all active, appreciative and enthusiastic moods are apt to be stricken-through and killed by the dissecting-knife of analysis. It cannot be denied that in certain scientists and philosophers some such soul-withering process seems to have taken place; but ought one to call such abnormal individuals the highest products of mental evolution? Is not the continual predominance of the analytic mood, with the *aboulia* which tends to result from it, a definitely psychopathic state? Think of such a man as Benjamin Constant, an abject failure as a moral personality, but a brilliant success as a critical intelligence, writing a successful psychological novel in which his own moral turpitude is laid bare! I do not mean to imitate Bergson in disparaging the intellect and regarding its appearance in evolution as a cosmic blunder; I simply mean to suggest that in the highest type of intelligence there should be a healthy alternation between straightforward activity and critical analysis. Religion belongs to the same class as vital processes like nutrition and literary self-expression. Analysis may and should be employed to guide these processes; but should it be permitted to destroy them?

Another query: is the religious state of mind, to use Lévy-Bruhl's expression, "impervious to experience?" Is it fitly represented by Janet's case of the woman who persisted in believing her head had been amputated, all evidence to the contrary notwithstanding?

I think it is true that religious dogmas tend to be modified by experience much more slowly than the practical arts and physical sciences, and that, consequently, our views of the ultimate nature of things stand more in need of revision; but I think that this is largely because the cosmic environment "hits back at us" much less forcibly and regularly than our immediate physical environment. I am convinced that the history of religion shows many cases of the modification of theological hypotheses under stress of circumstances. For example, the Hebrew prophets used their conception of the divine moral order as a means of predicting future historical events—unsuccessfully, in many instances. When failure came, they sometimes rationalized it to fit in with their previous dogmas, quite like Levy-Bruhl's savages; but not always. Sometimes they gave up the dogma and framed a new one—notably in the case of the dogma that suffering is a divine punishment indicating guilt on the part of the sufferer. I cannot escape the conclusion that in the course of this slow and halting process of theological reconstruction a genuine approach to

the truth about the cosmic conditions of human life has been taking place, and that religious symbols like the idea of God consequently have a cosmic as well as a human significance.

A final query: is religion only for the weak, or chiefly for the weak? Such is Janet's conclusion; but is it a wholly consistent conclusion from the facts as he himself sets them forth?

If the reader will consider the diverse effects of mental stimulation, I think he will agree that a permanent or protracted raising of the level is probably indicative of a strong or "rich" individual, while sharp reaction and permanent depression following stimulation is probably indicative of a weak or "poor" individual. Is not the conclusion obvious? Religion or any strong stimulant may be positively *dangerous* for a neurasthenic individual; his friends must guard him against revival meetings as they guard him against alcohol and drugs.[26] It is the strong individual, who is tempted to drowse along and do less than he is able, who *most* needs religion. What happens when such an individual—a Wesley or a Luther—"gets religion" is written large across the face of history. He generates a wave of energy that galvanizes whole masses of men and seems to persist for centuries. If religion can produce such results without stultifying the intelligence, and if the energy it generates can be diverted into the right channels by a simultaneous sharpening of the moral sensibilities, then, in the name of Humanity and Progress, why not have more religion! Let us have scientific psychotherapy too, and anything that will help tone us up for the task that faces us; but in my judgment, any one who, having no insuperable intellectual obstacles in his way, and having not yet exerted himself to the verge of exhaustion, rejects religion or any other wholesome stimulant because he feels no *need* of it, has a poor conception of the load of moral responsibility that rests upon every human individual in a time like this.

But it is after all one of the functions of religion to create the vision of duty which makes one conscious of the need of inspiration. It is one of the merits of Janet's conception of religion—and here I touch upon what seems to me to be his most significant contribution to the philosophy of religion—that it ascribes to religion not only a stimulative but also a directive function, and shows the vital unity of the two functions: the gods, according to his theory, conserve human energy by satisfying the human need for direction or guidance.

This, it seems to me, is a much juster view of the function of religion than that of certain sociologists who regard religion purely as a stimulant. T. N. Carver, for example, once remarked in a lecture that the function of religion in the state was like that of a brass band in a regiment: it invigorates but must not presume to lead. Similarly, Giddings once defined religion as "all that stimulates the will to carry on." It seems to me that Janet's definition of religion describes the facts much more adequately than those of either Carver, Giddings, or Durkheim, while at the same time recognizing the relative truth of such broader definitions. All four would agree on the function that differentiates the genus; but none of them marks off the

[26] I do not mean to say that religion can do nothing for the neurasthenic; on the contrary, the Emmanuel Movement has proved that the skilful minister may make a distinct contribution to psychiatry. Besides, the patient's need is for something more than physical and mental health. There is still a place for the "cure of souls".

species so exactly as Janet, or notes so clearly that when we get into the sphere of social stimulants a new function, the directive function, comes into play.

Perhaps it would be well to point out that religion as Janet defines it never occurs, historically, in an isolated state. It always occurs inextricably mingled with other stimulants. The inspiring influence of the religious group upon the religious individual is almost as important as that of the god himself, and it is not surprising that Durkheim should have considered it the chief thing in religion. Moreover, even after the rejection of grosser stimulants like alcohol, religion has always continued to use certain stimulants, like music and rhythmic motion. Even the best of these stimulants of the outer sphere inspire *without directing;* but if they are employed in the service of a religion whose god-idea is moral, they may become an integral part of that religion, and take their direction from it.

I have only one modification to suggest in Janet's admirable definition of religion: the species "religion" within the larger species "social stimulant" should be so differentiated as to indicate the rôle which the speculative interest has played in religion. The gods—to repeat Leuba's contention—grew in part out of the desire to explain creation; and a large part of the stimulating effect of communion with them is due to the belief that they control the universe. To guide, to inspire, and *to protect* are their most prominent functions; and the third is not the least important. Whether there is any evidence for protection is another matter; many earnest thinkers assure us there is not; but religion is vitally interested in the question, and will not let it drop until it is definitely decided in the affirmative or the negative, or until the boundaries between a legitimate and an illegitimate faith in Providence are definitely settled—which means that discussion is bound to continue for some time yet.

Meanwhile we are greatly indebted to Janet for the whole of his spirited analysis, and in particular for his original theories on the origin and function of religion. The "duplicity" theory of the origin of belief in spirits should excite much discussion. The analysis of religious faith is particularly subtle; no religious person can read the comparison of religious and philosophical discussion without crying "*Peccavi!*" Doubtless the present exposition of Janet's views has been unconsciously colored by the religious leanings of the writer; but he would be deeply chagrined to discover that it was so, for he cherishes the hope that a genuine working partnership may some day be established between religion, science, and philosophy.

THE EFFECT OF VARIED INSTRUCTION ON THE PERCEPTION OF LIFTED WEIGHTS[1]

By Albert Clayton Reid

I. Introduction

Our object in this study was to trace, and if possible to measure, the influence of varied instruction upon the discrimination of lifted weights. We gave our *O*s three different instructions. The first required them to discriminate weights as material objects of the outside world, and thus involved what Titchener has called the stimulus-error;[2] the second required discrimination of the pressure-patterns set up on and in the fingers by the act of lifting; and the third called for a like discrimination of the pressure-patterns set up in the wrist.

Since certain experiments, which Friedländer[3] and Fernberger[4] have performed, bear upon our problem, we shall first take account of them.

Friedländer defines his problem as follows.[5] "In the usual way of viewing the outside world we perceive most of the different sensory qualities as properties of objects." "It is possible," however, "to follow up this process of objectification, without recourse to theories *ad hoc*, since, even to the developed consciousness, sensory contents may be presented which are free from any attributive relation to objects." Friedländer, therefore, set himself the task of "studying the changes which sensation suffers when it is referred to an external object, and ascertaining the conditions which are necessary for this objectification" in the field of the perception of weight.

From a preliminary experiment, in which *O* stands, holds his forearm and hand in a horizontal position, receives a weight on his fingers, and then moves his forearm up and down, Friedländer obtains an account of the phenomena which appear, on the one hand, "when we attend to the sensations of pressure and of force (*Kraft*) in the lifting member and abstract from the object (*A-Einstellung*)," and, on the other hand, "when we direct the attention upon the lifted object itself (*G-Einstellung*)."[6] Friedländer characterizes the two *Einstellungen* as follows:[7]

"*A-Einstellung.* I close my eyes and try to forget completely that an object is resting on my hand, and direct my attention solely to the sensations in the moving extremity. I succeed in doing this only with strong concentration, since the idea of an external heavy something has a strong tendency to come into consciousness. There arises a complex of manifold, spatially extended sensations, which cannot at the same time be fully attended to in all its parts. What force themselves upon consciousness at

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[1]From the Psychological Laboratory of Cornell University.

[2]E. B. Titchener, *Experimental Psychology*, ii., 1905.

[3]H. Friedländer, Die Wahrnehmung der Schwere, *Zts. f. Psych.*, lxxxiii., 1920, 129-210.

[4]S. W. Fernberger, An Experimental Study of the 'Stimulus Error,' *Jour. of Exp. Psych.*, iv., 1921, 63-76.

[5]*Op. cit.*, 129 ff.

[6]*Ibid.*, 133.

[7]*Ibid.*, 134 ff.

first are rising and subsiding tactile [for Friedländer this term covers sensations of pressure, contact, and kinaesthesis] sensations in the fingers. With a more exact analysis, these turn out to be a complex of two kinds of sensations, distinguishable in quality and in intensity. In order to make this differentiation easier for me and more distinct, I first let the loaded fingers rest upon the table; I then notice an aggregate of pressure sensations on the inner surface of the fingers; it is not a unitary, uniform pressure." "If I now lift the weight, a new group of sensations appears distinctly in addition to the pressure sensations, which remain approximately constant; we designate them sensations of strain (*Spannung*) or force. These are seated in the deeper parts of the fingers; they are not, however, so definitely localized as the pressure sensations; and they show, above all, a great tendency to go out from the hand into outer space. I may, conversely, bring the sensations of force to consciousness and isolate them to a certain degree, if I weaken the pressure sensations by covering the hand with a thick leather glove. When I have performed this isolation and impressed upon myself the characteristic peculiarities of each of the two sensory groups, I succeed tolerably well in analyzing them out of the whole into which they fuse when they are present simultaneously." "Diffuse tactile sensations of slight intensity above and below the elbow are also noticeable."

"*G-Einstellung.* I open my eyes and turn my attention to the object which rests on my hand. Immediately the sensations in the arm, hand, and fingers recede in consciousness almost entirely, and the object is filled with a sensory something which appears to me directly given as its weight (*Schwere*). This weight forms a unity with the visual appearance of the object. It is a heavy object that I perceive. Only for some moments do I succeed in forcing the sensations in the fingers back into consciousness; for the most part, the pressure sensations persist simultaneously with the weight content, but they are very indistinct and take a merely secondary place. The weight content itself is unitary and cannot be analyzed further."

Friedländer first reports a series of 16 experiments on the conditions of the perception of weight.[8] While these experiments present many points of interest and in certain cases challenge repetition, they do not bear directly upon our own problem, and we pass them over without comment. He then undertakes two experiments with the object of determining "whether, in the comparison of two pressure intensities, the delicacy of sensitivity to differences suffers a change through objectification." These experiments, since they bear upon our problem, we shall present in detail.

Friedländer had already established experimentally that, when the sense of force is ruled out, the sensations of pressure are by themselves adequate to the perception of a heavy object;[9] but when he came to the present experiment he found that two of his three *O*s could not make a comparison under the *G-Einstellung*. In the first experiment,[10] *O* placed his left hand in a plaster cast, and received two brass cylinders, in succession, on the inner surface of his fingers, which were protected by a hard rubber disc. The time of presentation was 4 sec., and the interval 4 sec. In one experimental series (*A*), *O* was to "close his eyes and, under the *A-Einstellung*, to compare the sensations of pressure in the hand." In a second series (*G*), *O* was to "open his eyes and, under the *G-Einstellung*, to compare the heaviness of the weights." "Series *A* and *G* alternated from day to day. Same order of variables in both." On days 1 to 12, *E* presented the weights (standard 500 gm.; 18 variables, ranging, by steps of 20 gm., from 340 to 680 gm.) in both time orders, and *O* made 72 comparisons daily. On days 13 to 18, *E* presented the same set of weights, but only in

[8] *Ibid.*, 155 ff.
[9] *Ibid.*, 160.
[10] *Ibid.*, 187 ff.

the first time order, and *O* made 54 comparisons daily. On days 19 to 44, which yielded the results published, *E* presented, in the first time order, a new set of weights. The variables now ranged, by steps of 10 gm., from 390 to 590 gm. in series *A* (21 variables; but variables 390, 580, and 590 were presented less often than the other variables), and from 420 to 590 gm. in series *G* (18 variables). *O* made 54 comparisons daily. Throughout, *O* was ignorant of the stimulus differences, was left free to judge either weight in terms of the other, and his judgments were to take the form 'much lighter,' 'lighter,' 'doubtful,' 'equal,' 'heavier,' and 'much heavier.'

The results of the experiment showed $G>A$. "For the one *O* the limens were somewhat lower under the *G-Einstellung*, and the scattering was almost equal in both cases; as a whole, sensitivity to differences was somewhat finer in series *G*". "This result is surprising," Friedländer comments,[11] "for the introspections of this *O*, which are in agreement with those of other *O*s, led us to expect that objectification noticeably diminished sensitivity to differences. This *O* had no difficulty in objectification, and in *G* he actually compared the heaviness of the weights alone. But this way of comparing, he felt, was unnatural and uncertain, because attention was diverted from the 'main thing,' the pressure sensations." Friedländer offers two alternative explanations. The one is that "the density of sensation is lessened when objectified, since it spreads throughout the volume of the object; and, if this decrease in density has the same effect as a decrease caused by a weakening of the stimulus, a refinement of sensitivity to differences with objectification is to be expected." The other possibility is "that a greater exertion of the attention involuntarily evoked by the difficulty of the task has favored the comparison of the objectified sensations."[12]

On the other hand, a second experiment by means of the aesthesiometer with the same *O* (standard weight 1200 gm.; variables ranged, by 50 gm. steps, from 800 to 1700 gm. in series *A*, and from 850 to 1750 gm. in series *G*; point of application, the back of the last joint of the middle finger of the left hand) showed a "considerable diminution of sensitivity to differences under the *G-Einstellung*;" $A>G$. "The reason...is that in this set-up *O* did not succeed in objectifying pressure; the *G-Einstellung*, therefore, only served to divert the attention from the contents to be compared (the pressure sensations in the finger)."[13]

Of the two *O*s who were unable to objectify the pressure sensations in the lifted weight experiment, one was eliminated early, while the results of the other —$A>G$—reflected the difficulty of the *G-Einstellung*. $A>G$ was also the result of the same *O*'s experiment with the aesthesiometer.

Friedländer thus makes the difficulty of the *G-Einstellung* account both for the result $A>G$ (which he obtained three times) and for the opposite result $G>A$ (which he obtained once). In addition, he offers an alternative explanation of the latter result. "Whether this explanation or the other can claim greater probability," he concludes, "must for the present be left undecided."[14]

Fernberger carried the problem over to the active perception of weight.

Citing Titchener, he raises the question of "the distinction between the stimulus attitude and the process attitude in the formation of sensory judgments. In the case of the process attitude, the observer is instructed to judge process only and, indeed in most cases, only one attributive aspect of a given process." "Under the stimulus attitude, on the other hand, the observer judges not process, but the stimuli themselves." "The equi-

[11] *Ibid.*, 190 f.
[12] *Ibid.*, 200.
[13] *Ibid.*, 192.
[14] *Ibid.*, 200.

vocality of the judgments under the stimulus attitude is well shown"[15] by Titchener, Gates, de Laski, and Friedline. "The use of lifted weight stimuli presents a situation in which the distinction between the two attitudes should be important. All of the earlier work in the field has been performed under the stimulus attitude,—the observers have been instructed to judge the difference between the weights themselves."[16] "It has been shown that the processes involved in the formation of the judgment are exceedingly complex and that these processes actually vary from individual to individual observer and for the same observer at different stages of practice." "Here then would be an excellent field in which to discover whether the judging under the stimulus attitude or the judging of some particular attributive process will have any effect upon the statistical limens."[17]

Passing to a consideration of Friedländer, who "has shown that it is possible to have the observer adopt either an objective attitude or an attitude of judging only one of several elements in a complex, and that the adoption of one or other of these attitudes may affect the threshold values," Fernberger maintains that Friedländer's "instructions for the subjective attitudes (especially the kinaesthetic) were still too indefinite and equivocal and were therefore capable of further refinement. Also his statistical conclusion regarding the effects of the attitudes upon the measure of sensitivity are [is] not conclusive."[18]

Fernberger, therefore, repeats the experiment. He used hard rubber stimuli, presented in the first time order only, with the space error ruled out. A metronome, set to beat 72 in the 1 min., regulated the rate of lifting. *O* rested between double lifts, if he so desired. The standard weight used was 100 gm.; the variables were 88, 92, 96, 100, and 104 gm. Three *Os* took part in the experiment: one *O* had had a great deal of practice, another had had some little practice in the lifted weight experiment, while the third was totally unpractised.

Three sets of instructions were used:[19] instruction *A* directed *O* to judge solely in terms of "the intensity of the pressure sensations on the tips of the fingers;" *B* called for judgments on the basis of "the intensity of kinaesthetic sensations localized in the wrist;" *C* required that *O* "take the stimulus attitude and... judge the weights themselves." Three categories of judgment—greater, less, and equal—were permitted; if for any reason *O* failed to live up to the instruction, he was to report 'failure.' 1250 judgments were obtained from every *O* in every series. "Introspective characterizations were taken for every series so that it could be assured that the observers were making the proper attentional abstractions and were forming the judgments on the basis of the processes desired. An examination of these introspective characterizations shows that the instructions were properly fulfilled."[20]

Inspection of the threshold values[21] reveals little uniformity in results among the *Os*. Combining *hu* and *hl*, we find that

$C>B>A$	for *Os* P and B;
$C>A>B$	for H; while
$C>B>A$	expresses the general tendency.

In no case, however, is the difference in the values of *h* very large. The values of *L* in the different series present a greater variation:

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[15]*Op. cit.*, 63.

[16]This statement is in flat contradiction to that of Friedländer, *op. cit.*, 132. Cf. the discussion in Titchener, *Exp. Psych.*, ii., 2, 1905.

[17]*Ibid.*, 64 f.

[18]*Ibid.*, 65 f.

[19]*Ibid.*, 67 f.

[20]*Ibid.*, 68 f.

[21]*Ibid.*, 71.

$A>B>C$	for P; Lu is negative in C;
$C>B>A$	for B; Lu, however, is negative;
$B>A>C$	for H; Lu is negative in C.

If, however, we consider the position of the *Ls* for B in *C*, then $A>B>C$. P alone exhibits considerable differences in the size of the limens. In the intervals of uncertainty

$A>B>C$	for P;
$C>B>A$	for B;
$B>A>C$	for H.

Again, P alone gives rather large differences. The point of subjective equality varies very little.

$C>A>B$	for P;
$C>B>A$	for B;
$C>B>A$	for H; and
$C>B>A$	expresses the general tendency.

Fernberger's comment on the differences in the results is that "these differences cannot be understood, we believe, without also considering the stage of progressive practice in which we find each of the subjects." "For the unpractised subject, the judgment is made largely on the basis of pressure sensations on the tips of the fingers, while as practice continues, these are gradually lost until there is a complete shift to kinaesthetic criteria."[22]

In conclusion,[23] Fernberger finds that the differences in instructions do not bring about any significant variation in the nature of the curves of the psychometric functions, in the coefficients of precision, in the point of subjective equality, in the presence of the time error, and in the effect of progressive practice in lowering the point of subjective equality. The size of the intervals of uncertainty, however, varies with the different instructions. "For the unpractised subjects, the interval of uncertainty for *A* is smaller than that for *B*. For the practised subject, the interval for *B* is smaller than that for *A*. There seems to be a tendency for the interval obtained under *C* to be smaller than either of those for *A* or *B*. The stimulus attitude, however, may lead to an equivocal sort of judgment which results in a low degree of subjective assurance; which in turn results in a tendency to increase the number of equality judgments and thus leads to a larger interval of uncertainty."

It is clear that further work is needed. We cannot consider Friedländer's work satisfactory as regards either his conditions, his statistical data, or his attempts at interpretation. He operates with different numbers of variables under the two *Einstellungen*, presents some variables less often than others, employs only one time order, alternates the instructions from day to day, allows *O* to judge in terms of either weight, has but two *O*s, and, finally, obtains only a small number of judgments (41)[24] for every variable. Secondly, he publishes the data of only one *O*, and combines the judgments of different categories and for different variables. Finally, he offers the same explanation (the

[22] *Ibid.*, 73 f.

[23] *Ibid.*, 75 f.

[24] Except variables 390, 580, and 590. These he presents less often than the others, and then computes the frequency for 41 presentations. *Op. cit.*, 188, 190.

difficulty of the *G-Einstellung*) now for the result $A > G$ and now for the result $G > A$,—even in the case of one and the same *O*.

Fernberger, like Friedländer, works with only one time order; he does not keep the interval between double lifts constant; and he has but three *O*s. These, however, may be relatively unimportant matters; it is of far greater importance that Fernberger has not given any detailed discussion of the carriage of his instructions.[25]

We remark, first of all, that in the case of so complex a perception as that of lifted weight the change from stimulus instruction to process instruction does not at all guarantee a change from common-sense attitude to psychological attitude on the part of the *O*. A highly trained and reliable *O* will of course do his best, with full awareness of what he is about, to effect the prescribed change of attitude; but the untrained *O* may very well shift his attention from weight to pressure-pattern, as from one object to another, without any corresponding change of total attitude.[26] It is therefore altogether possible that the changes of instruction carried generically different meanings to Fernberger's trained *O* and to his other two *O*s.[27] We remark, secondly, that verbal understanding of an instruction, on the part of untrained *O*s, does not necessarily imply its assimilation and its realization in practice,—still less the steady maintenance of the correlated attitude. Fernberger allowed his *O*s[28] to judge 'greater,' 'less,' 'equal,' and 'failure;' and the relative number of 'failures' reported by *O*s in different stages of training is in this regard highly significant; yet Fernberger tells us nothing of the number or distribution of such reports. He thinks, moreover, that his partly trained *O* varied her criteria of judgment under a given instruction.[29] Lastly, he attributes certain differences of result to "the stage of progressive practice"[30] of the three *O*s, on the ~~assumption that the 'natural' early criterion of a difference~~ in lifted weight, under the stimulus attitude, is cutaneous pressure, while the later criterion is kinaesthetic. This assumption, based on Fernberger's previous work on the Process of Comparing,[31] seems to be too broad, since it leaves out of account, as the previous work does not, such possibilities as visual imagery and rate of lift; at any rate it invites further testing.

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[25] *Op. cit.*, 76.

[26] Especially will this be true if Fernberger's account of the course of practice under the stimulus attitude is accepted as correct.

[27] *Ibid.*, 76.

[28] *Ibid.*, 68.

[29] *Ibid.*, 74 f.

[30] *Ibid.*, 73 f.

[31] S. W. Fernberger, An Introspective Analysis of the Process of Comparing, *Psych. Mon.*, xxvi., 1919, 95 ff., 136 ff.

These and like considerations determined not only the topic of the present study but also the procedure of experimentation.

We, like Fernberger, have given specific instructions; we have worked with 7 *O*s of different degrees of training; we have taken 2500 observations under every set of instructions, and another 1000 as control under whichever happened to be the initial instruction; we have secured after every series a report as complete as possible of the criteria of *O*'s judgments; and we have sought to evaluate all our data, including the extraserial 'doubtful' judgments.

II. Apparatus and Procedure

We performed the experiment in a quiet room of the psychological laboratory. The room was well ventilated, and the temperature kept between 68° and 72° F.

O sat in a comfortable, felt-padded chair at the side of a table, 65 cm. high, fastened permanently to the floor. We removed one arm of the chair, so that its seat extended about 8 cm. under the table. We adjusted the position and height of the chair to the individual *O*, so that he, without leaning over, was able to rest his forearm on the table, 6 cm. from its edge. We kept the position of the forearm constant for every *O* by fastening to the table two felt pads,—the one, 4 by 6 by 2 cm., supported the forearm, near the wrist; the other fitted about the elbow,—and by silver-nitrate marks on the forearm, the one at a point corresponding to an indicator lying between the pads and the other to a line drawn across the pad upon which the forearm rested. Since baize covered the table as well as the pads, the forearm, which was bared up to about 5 cm. above the elbow, touched only material of the same texture.

To eliminate the space error we presented the weights on a carrier bracket.[32] The lower side of the carriers was faced with chamois, and the upper side with chamois and felt. The felt, cut to fit the base of the weight holder, indicated the exact position which the weight should occupy. When the carriers, which moved on a level board fastened to the legs of the table, swung a weight into position, the handle made an angle of 32° with the side of the table and was about 4 cm. immediately below *O*'s hand, which projected beyond the edge of the table. With a free movement of the wrist, *O* could now lift the weight without swinging or turning it, and without changing the spatial relations of his hand and arm.

We used Titchener's modified form of Fechner's weights.[33] The handles, which were turned from smooth-grained cherry wood and unpainted, rolled smoothly and silently when the weights were lifted. The weights did not shift or rattle. To keep the temperature of the handles constant, we tested them at the beginning of every observation period and warmed them, if necessary, by an electric heater; and we used two extra weights to replace the standard if it became over-warm.

The weights consisted of a standard of 700 gm., and 5 variable weights of 649, 674.5, 700, 725.5, and 751 gm. The magnitude of the increment, determined empirically, was such that the distribution of the judgments should be adequate to the method of constant stimulus differences. Every weight was adjusted to 0.001 gm. once a week. Only once did we find an increase or decrease of more than 0.3 gm.; the variation averaged 0.08 gm., and usually was in the same direction for all weights. When the weights were not in use they were covered by a felt blanket.

[32] E. B. Titchener, *Experimental Psychology*, ii., 1, 1905, 116.
[33] *Loc. cit.*

The weights were lifted with the right hand, palm downward. A muffled bell-metronome, set to beat 60 in the 1 min., with the bell striking at every fourth beat, regulated the rate of lift. At the signal 'ready,' *O* placed his arm in position and closed his eyes; *E* then gave the signal 'now,' and *O* began his lifting at the next stroke of the bell. Thereafter, *O* allowed 4 sec. to pass before he began the next double lift. The movement of the hand always began with the hand open and extended in a comfortable horizontal position. At the stroke of the bell, *O* lowered his hand and grasped the handle; at the next beat, he raised the weight about 4 cm.; and at the third, he lowered it and raised the hand to its previous horizontal position. *E* then swung the second weight of the pair into place, and, at the next stroke of the bell, *O* lifted the weight in the manner described. Hence the interval between two lifts of one pair of weights was 1 sec.; the interval between double lifts was 4 sec. In order that the movement made in lifting a weight should become rhythmical and without pause, and that the procedure should become automatic, every *O* performed a practice-series of about 500 double lifts, which extended over a period of two weeks.

We presented 80 pairs of weights during every period of observation. These fell into 4 groups with 20 pairs in each one. A warming-up series of 10 pairs came before the start of every series. A rest period of 1 min. followed the warming-up series and intervened between the separate groups. No interruption occurred during a period or group. We repeated the 'doubtfuls' which occurred within a group immediately after that group. We presented the weights in both time orders. The time order and the order of the variable weights were haphazard for every pair of weights.

The period of observation was 30 min., and the *O*s worked, so far as possible, for one period at the same hour of the day on the six days of the week. The exceptions to this rule were as follows: on one day of the week N observed, throughout, at 1.30 p. m. instead of 11 a. m.; during the second term H observed twice daily, with an interval of 1 hr. between periods; and for the second half of this term S worked twice on Fridays (and omitted Saturdays) with 1 hr. between periods. No observations were made after 3 p. m.

The observers were Dr. L. B. Hoisington (H), assistant professor of psychology, and Miss F. H. Burns (Bs), Mr. F. L. Bixby (By), Miss E. F. Möller (M), Mr. J. P. Nafe (N), the writer (R),[34] and Mr. M. G. Scheck (S), graduate students in the department of psychology; Bs and By were in their first, M and S in their second, and N and R in their third year of graduate work. They varied considerably in training as *O*s: H was highly trained; M, N, and R had a moderate degree of training; Bs, By, and S were untrained. All the graduate *O*s, except M and R, who had performed a class experiment, were totally unpractised in the lifted weight experiment; and, with the exception of the experimenter (R), they worked without knowledge of problem and conditions.

When H, the highly trained *O*, had practised lifting the weights until the movements became automatic, *E* gave him the following instructions: "You are to lift the weights as you have been instructed; report the gross phenomenology experienced in lifting a pair of weights." H reported distinct pressures on and in the fingers and thumb, somewhat vague smooth pressure and strain in the wrist, slight strain in the forearm, and an indistinct superficial pressure on the back of the hand. Since the strain in the forearm and the pressure on the back of the hand were indistinct, we decided to follow Fernberger in disregarding them.

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[34]Mr. S. Feldman kindly served as *E* when the writer was himself the *O*.

The experiment included three sets of instructions, each one of which should give *O* a different problem and, therefore, a different attitude.

Instruction *A* demanded that *O* judge the weights as weights, that is, as objectified, material things. This instruction was as follows:

"In this experiment a series of weights will be presented to you in successive pairs. You are to lift the weights as you have been instructed, and you are to judge the second weight of every pair in terms of the first.

"Think of the weights as *material things* lying before you; it does not matter how you perceive them psychologically, *so long as your perceptive attitude remains constant.*

"Your judgments are to be (1) independent, that is, they are to bear solely upon the relation of the second weight to the first weight of a given pair; and (2) immediate, that is, they are to be passed without reflection.

"Your reports will naturally take the form 'greater,' 'less,' and 'equal.' If no one of these terms satisfies you, or if you in any way fail to follow instructions, you may report 'doubtful,' and the particular pair of weights will be presented again in a later series."

Instruction *B* demanded that *O* attend to the felt pressure-pattern set up on and in the fingers and thumb of the lifting hand, and judge in terms of this pattern. After the first sentence the instruction was as follows:

"You are to lift the weights as you have been instructed, and you are to attend to the felt pressure-patterns set up in the fingers and thumb of the lifting hand. You are to judge the second pressure-pattern of every pair in terms of the first."

Instruction *C* demanded that *O* attend to the pressure-pattern set up in the wrist, and judge in terms of this pattern. This instruction was the same as instruction *B*, except that the word 'wrist' was inserted in place of the words 'fingers and thumb of the lifting hand.'

At the start of the first series, *E*, without any comment whatever, handed *O* a typewritten copy of the instruction, which *O* kept and used throughout the series. *E* did not record the first 80 judgments which *O* gave under a new instruction. After 1000 judgments had been obtained, *E* required *O* to reread his instruction very carefully. *E* made no comment. When the second and third instructions were handed to *O*, *E* asked him to read them aloud and to re-phrase them in his own words.

No other conversation was held by *E* and *O*, and no question was ever asked.

Within two or three hours after the completion of a series of observations under a given instruction, *E* asked *O* to state the criteria upon which his judgments in this series had been based. *O* then dictated the statement.

The order in which the instructions were given appears in the tables of results. Two *O*s, for reasons that will appear later, repeated the first set of instructions given them.

500 judgments were obtained from every O for every pair of weights under every instruction. In the check series, 200 judgments were obtained on every pair of weights. The total number of judgments obtained from every O was, therefore, 8500. The frequency of the judgments, the tendency of the judgments towards greater or less, and the results of mathematical treatment of the data appear in the following tables.

III. Quantitative Results

The tables show, in correct temporal order, the quantitative results of every series for every O. Table I shows the distribution of judgments for the first (AB) and the second (BA) time orders, and for AB and BA combined; Table II gives h'_u, h^u, L'_u, L_u, h'_l, h_l, L'_l, L_l, the interval of uncertainty (I.U.), and the point of subjective equality (P. S. E.), for AB and BA for every 250 judgments, and the totals for AB and BA separately and combined for every series. The 'check' series we shall refer to as X. The last three lines of Table II show the values of AB and BA, separately and combined, for the last 1000 judgments of O's initial series; these values (Y) are to be compared with those of X.

IV. Discussion

Instruction and Attitude

The quantitative results evidently call for somewhat extended discussion;[35] if they are to be understood, we must refer them to some standard and account for their deviation from the standard by way of the serial reports furnished by the individual Os. We take as standard the results of H, our longest trained and most reliable O; and we deal with the remaining Os in alphabetical order.

The instructions are indicated, as before, by the letters A, B, C. The sign > means 'better than;' and such a formula as $A > B > C$ means that, in terms of L and h, the results under A are better than the results under B, and these in turn better than those under C. We shall speak of practice presently; meanwhile we add a formula showing the order to be expected if practice alone determined the results. Finally, we write a third formula (*e.g.*, $A' > B' > C'$) which shows, so far as we are able to make it out, the actual *Aufgabe* under which O worked. Modifications of these symbols will, we believe, be self-explanatory.

There can be no doubt that H obeyed instructions, and judged, in so far as it was possible to do so, in terms of the criteria prescribed. His reports show clearly that, under B (his

[35] The X series are not included in the discussion of this Section.

initial instruction), he judged in terms of pressure-pattern, and was able to describe the characteristics of the pattern; under A, he judged the weights as material objects; under C, he judged, as nearly as he could, in terms of wrist-pattern, although he confessed that it was impossible to keep other factors from entering into the judgment.

B: "I perceived the pressure-pattern both in its totality and in its parts; whether it fused into a complete, unitary experience or whether there were more or less separate cores of experience. Intensity, I should say, was the most important aspect in the difference of patterns, but both quality and extent were important; the temporal aspect came in but little. My attitude, except for momentary lapses, was constant throughout, except for a while during the early part when intensity rather than total pattern was focal."

A: "I succeeded in this, that the experience as such, *i.e.*, the pressure-pattern, was never focal for attention. At first visual imagery, and all through the series differences in rate of movement,[36] came in. The weight out there at the end of my hand was the object of attention. Just what aspect of the weights touched off the judgments I cannot say, any more than that the second weight gave rise to the perception of weight greater or less than or equal to the first."

C: "I must say that, even to the end, the experiences from the hand occasionally, and less frequently differences in rate of movement, came into the judgment. The wrist experiences ranged from a smooth contacty quality to a dull pain. They differed also in density, the aches being more dense. They did not differ much in magnitude of extent, but did differ in bulk or volume and in sharpness of outline. The denser, in general, were more bulky and more sharply defined, and meant 'greater.' At times nothing but wrist experience was clear, but for most of the experiments the finger experiences attained a greater or less degree of attensity."

We may therefore write for H the following formulas:

Practice	Quantitative	Qualitative
$C>A>B$	$A>B>C$	$A'>B'>(C'+A'+B')$

Bs. This O appears to have followed instructions A and B, but failed to follow instruction C. Under A, she judged weight of material objects; under B, she judged pressure-patterns, naively, as total impressions of a quasi-material nature; under C, she judged wrist-pattern and rate of movement.

A: "I judged the weights just as something to lift; by a feeling that I get that the second is greater, less, or equal when I compare it with the first. It is just something there to be lifted, just something to be done."

B: "I based my judgments on the pressure-pattern or feeling that I got in my fingers and thumb when I lifted the weights. It was contact and pressure; not strain; just pressure on my hand. At first it was a little difficult to keep other things from coming in; but I soon got over that."

[36] The *O*s spoke of 'rate of movement' when a weight which made the absolute impression of 'heavy' appeared to come up sluggishly, and a weight which made the absolute impression of 'light' appeared to leap from the bracket. So far as *E* could judge, there was no change in objective rate of movement.

C: "I judged in terms of the pressure-pattern that I felt in my wrist. I hardly know what to say,—there was something in the wrist, but not very tangible, not nearly as tangible as the pressure-pattern in the fingers. The pressure in the wrist was a smooth, oily something, not very variable. I kept a sort of 'set' for a certain weight; then, when I got a lighter one, it seemed to rise faster, while the heavier ones seemed to lag behind in some way."

Bs' formulas may therefore be written:

Practice	Quantitative	Qualitative
$C > B > A$	$(A \text{ and } C) > B$	$(A' \text{ and } C'_A) > B'_A$

By's reports indicate that he followed instructions; although he experienced difficulty with C. Under A, he judged weights; under B, pressure-patterns; and, under C patterns in the wrist. His manner of judging under A, however, was naive, and he gave no description of the patterns that he experienced under B and C. Unfortunately By's work was a good deal interrupted by ill health, so that his results can hardly be compared with those of the other *O*s.

A: "Before every pair of weights, I say to myself: 'Now which of these two weights is heavier?' In lifting the first weight I usually get a contraction in my throat or in the calves of my legs; and the meaning 'greater' or 'less' comes from the increased tension or relaxation in the throat or legs. The judgment 'equal' seems to come automatically."

B: "I judged entirely in terms of pressure-patterns on the tips of my fingers and thumb. I do not know how the meanings were carried. They were not weights at all,—distinctly pressures."

C: "I based my judgments upon wrist-patterns. I do not know how the judgment was carried. There were times when the wrist-pattern did not correspond to the stimulus, but I always judged in terms of wrist-pattern. If I had been working under the stimulus instruction, I should have given a different judgment."

In general we may write:

Practice	Quantitative	Qualitative
$C > B > A$	$A > (B \text{ and } C)$	$A' > (B'_A \text{ and } C'_A)$

M, under instruction A, judged pressure-patterns on the finger tips. In the second series, after E had told her that she had not followed instruction, had given her the same instruction again, and had required her first to read it aloud and then to rephrase it in her own words, she followed the instruction. Under C, the criteria were wrist pattern and rate of movement.

A_1: "With the first weight I get a very definite pressure-pattern on the thumb and fingers. During the interval between lifts I am kinaesthetically tense in my knees and elbows; my vocal cords are tense, and I tend to hold my breath. I feel as if I were living in my finger tips; and by sheer force were holding the first pressure-pattern to compare it with the second."

A_2: "I describe the first weight vocimotorly and hold it by throat kinaesthesis. I also describe the second weight vocimotorly to fit it to or compare it with the first. If I have to change, I know the two are not equal."

TABLE II—(Continued)

Observer	Instruction	Time Order	Group of 250	Upper h′	Upper h	Upper L′	Upper L	Lower h′	Lower h	Lower L′	Lower L	Interval of Uncertainty	Point of Subjective Equality
			1	.5316	.0208	.7084	18.064	.5915	.0531	1.0675	27.221	45.28	685.52
			2	.5032	.0197	.7363	18.775	.6997	.0274	1.3120	33.456	52.23	688.38
			3	.6072	.0238	.5609	14.302	.5897	.0231	1.4813	37.773	52.07	688.65
		AB	4	.6677	.0260	.7513	19.158	.7135	.0279	1.1638	29.676	48.83	693.88
	C		5	.7285	.0285	.7948	20.267	.6757	.0265	1.2713	32.418	52.68	694.88
			Totals	.6426	.0252	.7991	20.377	.6758	.0265	1.2712	32.415	52.79	693.56
			1	.3366	.0132	2.4079	61.401	.4061	.0159	1.2368	31.538	92.93	710.61
			2	.3349	.0131	2.4763	63.145	.3800	.0149	1.1361	28.970	92.11	714.12
		BA	3	.3597	.0141	2.2439	57.219	.2796	.0109	1.1940	30.447	87.66	718.99
			4	.3530	.0138	1.7255	44.000	.3316	.0130	1.7954	45.782	89.78	699.54
			5	.3242	.0127	1.8262	46.568	.3647	.0142	1.2844	32.752	79.32	704.69
			Totals	.3454	.0135	2.1236	54.151	.3345		1.3950	35.572	89.72	709.96
M		**Totals AB & BA**		**.4618**	**.0181**	**1.2594**	**32.114**	**.4489**	**.0176**	**1.2768**	**32.558**	**64.67**	**700.22**
			1	.7070	.0277	.3297	8.407	.7998	.0313	.9820	25.041	33.44	690.66
		AB	2	.6195	.0243	.4999	12.747	.7581	.0297	.6620	16.881	29.62	696.45
	X		Totals	.6606	.0259	.4165	10.620	.7674	.0300	.8207	20.927	31.54	693.68
			1	.4378	.0171	1.3026	33.216	.4126	.0161	.6334	16.151	49.36	719.27
		BA	2	.3789	.0148	1.9421	49.523	.4675	.0183	.4118	10.500	60.02	716.33
			Totals	.4229	.0165	1.5838	40.386	.4409	.0172	.5237	13.354	53.74	712.95
		Totals AB & BA		**.4936**	**.0193**	**.9179**	**23.406**	**.5439**	**.0213**	**.6651**	**16.960**	**40.36**	**707.99**
	Y	AB		.6197	.0242	.9295	23.702	.9069	.0355	1.7594	44.864	68.56	682.99
		BA		.6074	.0238	2.1513	54.858	.4229	.0165	1.3918	35.490	90.34	718.10
		Totals AB & BA		**.5534**	**.0217**	**1.5309**	**39.037**	**.3801**	**.0149**	**2.1651**	**55.21**	**94.24**	**699.33**
			1	.4795	.0188	.6990	17.824	.5227	.0249	1.1143	29.146	24.20	691.06
			2	.4710	.0180	.5475	13.961	.5222	.0104	1.1010	28.075	22.16	698.61
		AB	3	.5198	.0203	.3410	8.695	.6355	.0249	.8558	21.822	30.51	691.90
			4	.4165	.0163	.2444	6.232	.5187	.0203	1.0983	28.006	34.23	689.96
			5	.4837	.0189	−.0353	− .900	.4715	.0184	1.1395	29.057	28.15	685.19
	A		Totals	.4725	.0185	.3585	9.141	.5688	.0223	1.0693	27.267	36.40	689.24
			1	.4185	.0176	1.1604	29.590	.4457	.0174	.3338	8.511	38.10	710.63
			2	.6302	.0247	1.0884	27.754	.6210	.0243	.1645	4.194	31.94	711.64
			3	.5216	.0204	.8959	22.845	.5987	.0234	.3602	9.185	32.03	705.72
		BA	4	.4733	.0185	1.6410	41.845	.5282	.0207	.0124	.316	42.16	719.58
			5	.5637	.0221	1.0604	27.040	.6312	.0247	+.0387	+ .986	26.05	713.27
			Totals	.5314	.0208	1.1505	29.337	.5729	.0224	.1470	3.748	33.08	712.18
		Totals AB & BA		**.4789**	**.0187**	**.7695**	**19.622**	**.5318**	**.0208**	**.6163**	**15.715**	**35.33**	**701.01**
			1	.5335	.0217	.1226	3.126	.6111	.0239	.9222	23.516	26.64	689.16
			2	.5644	.0221	−.0856	− 2.182	.5014	.0196	1.3420	34.221	36.40	682.75
			3	.5215	.0245	−.0020	− 2.346	.5365	.0210	1.2080	33.000	30.75	683.46

TABLE VI

Summary

Obs.	Ins.	$>$	$=$	$<$	Diff.	Obs	Ins.	$>$	$=$	$<$	Diff.
H	B	1041	436	1023	+18	N	A	1129	634	737	+382
	A	1127	324	1049	+78		B	1254	520	726	+528
	C	1112	430	958	+154		C	1190	461	849	+341
	$X(=B)$	456	104	440	+16		$X(=A)$	492	174	334	+158
	Total	3736	1294	3470	+266		Total	4065	1789	2646	+1419
Bs	A	1217	914	369	+848	R	B	1161	494	845	+216
	B	1330	909	261	+1069		A	1124	266	1110	+14
	C	1345	876	279	+1067		C	1147	234	1119	+28
	$X(=A)$	525	378	97	+428		$X(=B)$	396	141	463	−77
	Total	4417	3077	1006	+3411		Total	3828	1135	3537	+291
By	A	1437	594	469	+968	S	$B(=A)$	1100	892	508	+592
	B	1509	736	255	+1244		B	1182	965	355	+727
	C	1700	604	196	+1504		A	1001	1168	331	+670
	$X(=A)$	724	168	108	+616		$X(=B)$	386	415	199	+187
	Total	5370	2102	1028	+4342		Total	3669	3440	1393	+2276
M	$A(=B)$	787	1326	387	+400						
	A	962	789	749	+213						
	C	826	1101	573	+353						
	$X(=A)$	412	301	287	+125						
	Total	2987	3517	1996	+991						

TABLE VII

Effect of the Magnitude of the First Member of a Pair

Obs.	Ins.	$S>V$	$S<V$	Diff.	Obs.	Ins.	$S>V$	$S<V$	Diff.
H	B	733	763	−30	N	A	606	819	−213
	A	771	818	−47		B	623	880	−257
	C	680	760	−80		C	715	859	−144
	$X(=B)$	497	328	+169		$X(=A)$	284	362	−78
	Total	2681	2669	+88		Total	2228	2920	−692
Bs	A	340	857	−517	R	B	682	839	−157
	B	246	887	−641		A	863	876	−13
	C	269	911	−642		C	842	867	−25
	$X(=A)$	94	364	−270		$X(=B)$	340	317	+23
	Total	949	3019	−2070		Total	2727	2899	−172
By	A	407	903	−496	S	$B(=A)$	415	749	−334
	B	234	907	−673		B	301	781	−480
	C	181	929	−748		A	277	704	−427
	$X(=A)$	98	380	−282		$X(=B)$	157	271	−114
	Total	920	3119	−2199		Total	1150	2505	−1355
M	$A(=B)$	349	627	−278					
	A	623	738	−115					
	C	486	626	−140					
	$X(=A)$	243	304	−61					
	Total	1701	2295	−594					

C: "I have been making all of my judgments in terms of wrist-patterns. When I did not do this, I reported 'doubtful.' I always held the first pattern vocimotorly and, I think, in the wrist also somehow. If the second pattern was a heavier and duller pressure than the first, I judged 'greater;' if brighter and lighter, 'less.' Sometimes a weight seemed very light and seemed to 'flip' up, and, again, it seemed to be very heavy and to give a slow steady pull. These experiences seemed to be localized in the wrist."

We may therefore write her formulas:

Practice	Quantitative	Qualitative
$C > A > A(=B)$	$A > A(=B)$ and C	$A' > B'$ and C'_A

N made a steady effort to follow all three instructions, but was unable to keep any one of them wholly clear of contamination. Under *A*, his criterion of judgment was a complex of material weight with cutaneous pressure and kinaesthesis recognized as such; under *B*, his principal criterion was pressure-pattern in the fingers, but he lapsed now and again into judgment by weight; and, under *C*, his attention was directed to wrist-pattern, but a good many judgments were determined by rate of lift, and some were touched off as if automatically.

A: "I am conscious of a good many kinaesthetic and cutaneous experiences,—pressure on the skin, kinaesthetic sensations in the hand, wrist and forearm, and something between the elbow and shoulder. I also think of my wrist as a pivot, and that it will take a certain amount of resistance to lift the weight. All of these factors are the index to my judgments."

B: "I based my judgments on the sensations of pressure on the fingers and thumb, giving my attention to these pressures. At times I detected a tendency to lapse into the instruction of the first series, localizing the weight out beyond my hand and judging the difference in weight; but this occurred only a few times and did not go unnoticed long. Generally speaking, my attention was on the pressure-pattern."

C: "I tried to base my judgment on sensory processes in the wrist. The term 'wrist' puzzled me. There was a complex of sensations in the wrist or vicinity of the wrist; I was set to observe these, and did so except in case of 'doubtfuls.' At first I had difficulty in keeping my attention on the wrist, but later I felt greater assurance in this than in the previous series. Often there was something which touched off the judgment without my knowing just how it was done. At times the weight simply bounded up. The weight I was expecting to lift was sometimes much greater or less than that presented, so the movement was greatly retarded or accelerated. This occurred spontaneously; sometimes one, again five or six in close succession."

We may write the formulas:

Practice	Quantitative	Qualitative
$C > B > A$	$C > B > A$	$(C' + A') > (B' + A') > A'_{BC}$

R's reports indicate that he followed instructions *B* and *A*, but not *C*. Under *B*, his criterion was pressure-pattern; under *A*, material weight, visual images carrying the meaning of weight, and rate of movement; under *C*, wrist-pattern, the same kind of visual imagery, and rate of movement. In general, R was aware that he was exceeding instruction *C*.

B: "I judged the pressure-patterns in terms of comparative density, distinctness of lateral limits, and depth. By 'density' I mean something like comparative fineness and compactness of texture; by 'limits' I mean lateral boundaries; by 'depth' I mean extent of the pattern toward the interior of the hand. If the second pattern was more compact, better defined, and deeper than the first, it usually meant 'greater;' if it was less compact, more 'airy,' more superficial, and less well-defined than the first, it meant 'less.' "

A: "I experienced the weights as material objects which I might project toward or against something, somewhat as one uses a cane. I judged in terms of the comparative ease with which the second weight was swung upward. Ordinarily the second weight seemed to rise very rapidly or very slowly and seemed light and 'airy' or heavy as compared with the first. The rapid movement almost compelled the judgment 'less,' the slow 'greater'. Often I visualized the weight as a gray cylinder suspended below my hand. The weight was denser in certain cases, and the more dense usually meant 'greater.' "

C: "The pattern in the wrist consisted of a sheet of smooth, tending toward dull, pressure which I tended to visualize as lying within the joint, an indefinite strain about the joint, and dull pain at the upper right side of the wrist. I tried to compare the second pattern with the first in terms of quality, intensity, and extensity. It seemed impossible to judge in terms of wrist alone. The pattern usually was indistinct. Rate of movement, a distinct lightness or heaviness, and an indefinite visual image of the weight would claim attention, in spite of effort to ignore them. Since they appeared so frequently and, except in rare cases, corroborated the judgment touched off by the wrist, I did not report 'doubtful.' "

We may write R's formulas as follows:

Practice	Quantitative	Qualitative
$C > A > B$	$(A \text{ and } C) > B$	$(A' \text{ and } C'_A) > B'$

S, under instruction *B*, judged in terms of kinaesthesis, cutaneous pressure and rate of movement. In the second series, after *E* had given him a statement showing that he had exceeded instructions, and had repeated *B*, his criteria were pressure-patterns of a quasi-material nature; under *A*, he judged material objects in terms of gross kinaesthesis, helped out by the pressure-pattern in the fingers.

B_1: "There was mainly an attempt to tell what the strains were in the arm and compare them, and to compare the pressures of the handles upon the fingers. The initial lag of the weight served as a cue, that is, the suddenness or slowness with which the weight responded to effort. There was strain in the wrist and in the back of the hand."

B_2: "I have tried to judge the second pressure-pattern in terms of the first with regard to pressure and resultant flattening of the fingers where the handle rests. I have also been attempting to distinguish the patterns of joint sensations in the fingers, and to compare the second with the first. Sometimes kinaesthesis from the joints spreads up into the back of the hand. I always find the pressure-pattern easier to identify than the joint sensations."

A: "I regarded the stimuli as objects to be lifted, and I made the judgments in terms of gross kinaesthesis, that is, of strains in the wrist, hand, fingers, and forearm, without analyzing them critically. Pressure in the fingers also entered into the judgments."

The formulas must therefore be written:

Practice

$$A > B > B(=A+B)$$

Quantitative

$$B(=A+B) > B > A$$

Qualitative

$$A'_B > B'_A > (A' + B'_A)$$

Aside from the influence of practice, the smallest L and the largest h will, of course, appear *where the basis of judgment is most stable.* It is sufficiently clear from our formulas that this condition is realized by instruction A. The psychological pattern in the perception of lifted weight is highly complex; the weight as material object is a matter of daily familiarity. Hence, in general, $A > B$,—and hence, also, the fulfillment of B as B'_A. While the symbol A' occurs 9 times in our formulas, the symbol B' (without subscript) occurs only 5 times, and only in the formulas of H, M, N, and R. Instruction C, at any rate for stimuli of the range which we employed, does not furnish an adequate basis of judgment; there is no single formula in which the symbol C' occurs without addition or qualification. The relative stability of wrist-pattern in the single case of N is, as we shall show later, a result of progressive practice; in all the remaining formulas, except that of H, we have the modified symbol C'_A.

Practice

The large number of observations that we have taken under one instruction, as well as the total number of observations, give us opportunity to fractionate the data to discover the presence and the influence of practice. We may look for the effect of practice in three directions: it may appear within the series (intra-serial practice); it may carry over from one series to another under the same instruction (practice in homologous series); or it may possibly carry over from one series to another under different instructions (practice in heterogeneous series).

(1) *Intra-serial practice.*—In terms of the combined values of h and L, and of the I. U., we trace intra-serial practice in the following instances (Table II): for H in series B, A, and C, his series in correct temporal order; for Bs and By in series A, their initial instruction; and for R in series B and A, his first and second series. M, N, and S show no effect of intra-serial practice, nor do Bs, By and R in their remaining series.

A glance at the primed formulas of Bs and By shows that under B and C they worked with the contaminations B'_A and C'_A. Similarly, R under C, worked with C'_A. It is natural to find in these facts an explanation of the failure of a practice effect. Like considerations apply to N and S. We were surprised to find no evidence of a practice effect in the A and $A(=B)$ series of M. This O discovered that she had misread her initial instruction when she had completed nearly two-thirds of the corresponding series; we then advised her to disregard instruction and to complete the series on the same basis. There is, however, no evidence of practice in the first 1500 observations of $A(=B)$. We cannot explain M's results,—unless by the hypothesis that some contamination of B' and A' escaped her notice.

Martin and Müller were able to distinguish five different modes of the practice effect.[37] The conditions of our own experiment were not favorable to such analysis. So far as we can determine, however, our intra-serial practice operated mainly by way of what Martin and Müller term "concentration of attention;" that is to say, as the series progressed, the O slipped more easily into the required attitude and held this attitude with greater constancy.

(2) *Experimental practice: homologous.*—We find practice in homologous series for H, N, and R ($X>Y$). To show a direct effect of practice in our homologous series, we must however show that the O accepted the repeated instruction as he had accepted the original. H and R, in fact, accepted the repeated B as B', *i.e.*, in the way in which they had taken the earlier B. The results of both Os indicate, in terms of the three values mentioned above, a practice effect. There is a like effect in the case of N; but N, who had taken his first A as A'_{BC} takes his second A as A'. Hence the effect of practice may be, at least in part, due to the clearing-up of the instruction.

The remaining 4 Os show no effect of practice. *Bs* took the first A as A' and the repeated A as A'_B; By took the first A as A' and repeated it as A'; and S took B as A'_B and repeated B as B'_A. We have already remarked that By worked under the handicap of continued ill health. M took the repeated A as A'. We should therefore expect to find her X better than Y, since Y was obtained as B' under $A(=B)$; but our expectation is not fulfilled. If in her case we compare X with the last 1000 observations of her second instruction $A(=A')$, we again find $X<Y$. This result seems to confirm our hypothesis of an unremarked contamination.[38]

(3) *Experimental practice: heterogeneous.*—We had hardly thought to find any proof of total experimental practice, since the different instructions were of differing degrees of difficulty, and since for six of our Os the most difficult instruction, C, came in the third place. N's results, however, show a striking regularity; if we take them in their temporal order we have $A<B<C<X$, and $X>Y$. It seems clear that the progress of the experiment meant for N a progressive stabilization of the basis of judgment; it will be remembered that not until X was given had he learned adequately to fulfill an instruction. We may say, therefore, that N's results, as a whole, indicate an indirect practice effect.

We may note that the reverse condition obtains for By: that is, $A>(B \text{ and } C)>X$, and $X<Y$. The like results of S,—$B(=A+B)>B>A<X$, but $X<Y$,—are sufficiently explained by his (primed) formulas.

Tendency of Judgment and Time Error

Martin and Müller describe two procedures which enable us to determine the presence, the direction, and the (approximate) relative magnitude of what they call general tendency, special tendency, and the physiologically conditioned error (Fechner's time error).[39]

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[37] L. J. Martin and G. E. Müller, *Zur Analyse der Unterschiedsempfindlichkeit, Experimentelle Beiträge*, 1899, 128 ff.

[38] M herself, whom we consulted after these paragraphs were written, tells us that she was subject, during the year, to recurrent nervous strain, and refers the zigzag course of her results to this cause. She is sure that her B'-attitude was uncontaminated; she thinks it possible that her A' should be written A'_B.

[39] *Op. cit.*, 81 ff., 97; cf. E. B. Titchener, *Experimental Psychology*, ii., 2, 1905, 302 ff.

When we apply these methods to our own results, we find that all our *O*s, except H in 4 cases, and M in one case out of 5, give $a_1 > b_2$, that is, they give more *r* cases when the standard comes first and is greater than the variable (Table III). By, R, M, and S follow the same general tendency, $a_1 > b_2$ and $a_2 < b_1$, throughout. For series *A* and *B*, Bs gives $a_2 > b_1$; N gives $a_2 > b_1$ in all cases; and H gives $a_2 > b_1$ in 2 cases. The deviations from the general tendency on the part of Bs and of H conform in part to what we might expect as a result of type. Those of N find no such basis of explanation. In all cases he gives more *r* judgments when a greater is followed by a lesser weight, independently of all other factors. The results of H suggest a very weak general tendency, if not the absence of such tendency.

Four of our *O*s, Bs, By, N and R, are of the positive type (Table III); they give $\Sigma a > \Sigma b$ for every series as well as for the total results. H and M are of the negative type (Table IV), although M in series *X* gives $\Sigma a > \Sigma b$. S gives $\Sigma a > \Sigma b$ in series $B(=A)$ and *B* and $\Sigma a < \Sigma b$ in series *A* and $X(=B)$, as well as for the total results.

A summary of all *r* judgments (Table V) shows that for every series, as well as for the total, more *r* cases occurred with the first time order for every *O* except N, who gave more *r* cases with the second time order in *A* and *C*, and for the total. Thus all *O*s except N agree with the general tendency to give more *r* judgments when the standard is presented first.

We find, further, when we summate the total judgments given (Table VI), that the judgment 'greater' occurred more often than the judgment 'less' for every *O*. The same thing is true for every series except series $X(=B)$ for R. In all cases except H and R the difference in favor of the judgment 'greater' is very-large.

Finally, if we summate and compare all the *r* judgments when the first weight, whether *S* or *V*, was greater than the second, and all *r* judgments when the second weight, whether *S* or *V*, was greater than the first (Table VII), we find that every *O* except H gave more *r* judgments when a heavier weight followed a lighter weight. The difference for every *O*, except H and R, is large, and for Bs, By, and S is very large.

In an experiment, as in ours, where the time order is haphazard for every pair of weights, where *O* cannot possibly know the time order, and where consequently the time order has no chance to exert a continuous effect, the relation of the weights within the pair seems to offer a better basis than does mere time order, or the relation of standard to variable, or a combination of these, for the determination of any tendency based upon relative magnitude of the weights.

Our results show a tendency on the part of six *O*s, slight in the case of R, to judge more correctly when the first weight of the pair is less than the second. One *O*, H, shows (as we have said) a slight tendency in the other direction.

The Doubtful Judgments

The 'doubtful' judgment, as George has shown,[40] is an extra-serial judgment, which indicates a lapse from the *O*'s required attitude. A comparison of the number of these judgments given by different *O*s, and by the same *O* under different instructions, ought therefore to help our estimation of the reliability of the results.

H, R, and N,—these are the 3 *O*s whom for other reasons we regard as the most reliable,—gave respectively 567, 627, and 614 'doubtfuls'. In the course of 8500 observations, on the other hand, Bs gave only 14 'doubtfuls' and apologized for 3 of these; By gave 29 under instructions *A* and *B*, and none in the next two series; M's number, 155, decreased from 87 in the first series to 3 in the last series; and S gave 78,—66 in the first two and only 12 in the last two series. When *O* sets himself a definite task as difficult as that demanded especially by instructions *B* and *C*,[41] it seems inevitable that he should report a large number of 'doubtfuls;' in other words, a small number of 'doubtfuls' is a fairly strong indication that *O* did not perform the task set by the instructions. Bs' apology shows that she was set to give a judgment rather than to follow instructions.

We treated the 'doubtfuls' with respect (1) to the judgment they followed, (2) the immediately preceding pair of weights, (3) the time of their occurrence within a period of observation, and (4) the distribution of the judgments given when their stimuli were presented again. We found nothing which indicated a correlation with the 'doubtful' judgments, except a slight tendency of these judgments to group, as might be expected, about the smallest differences between the presented weights.

The 'doubtful' judgments led those *O*s who reported them more frequently to remark in many cases a shift of attention and a consequent shift in attitude. This shift of attention came at times with distraction from outside, at times with associated ideas, and at times with the absolute impression of unusual intensity (+ or −) of pressure-pattern or of unusual heaviness or lightness of the weights.

Probable Correctness of Differences

We have calculated by Boring's formula[42] the probable correctness of the differences for series *A*, *B*, and *C* for H, N, and R, our three best *O*s. The results appear in the following table.

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[40] S. S. George, Attitude in Relation to the Psychophysical Judgment, this JOURNAL, xxviii., 1917, 1 ff.

[41] It is noteworthy, *e.g.*, that M's 87 occur under the instruction $A (= B)$.

[42] E. G. Boring, this JOURNAL, xxvii., 1916, 315 ff.; xxviii., 1917, 454 ff.

OBSERVER H

Greater Series		*Less* Series		*Greater and Less Combined* Series	
A and B	.30	A and B	.97	A and B	.635
A and C	.78	A and C	.50	A and C	.640
B and C	.59	B and C	.80	B and C	.690

OBSERVER R

Greater Series		*Less* Series		*Greater and Less Combined* Series	
A and B	.87	A and B	.99	A and B	.93
A and C	.99	A and C	.67	A and C	.83
B and C	1.00	B and C	.98	B and C	.98

OBSERVER N

Greater Series		*Less* Series		*Greater and Less Combined* Series	
A and B	.94	A and B	.87	A and B	.90
A and C	1.00	A and C	.99	A and C	1.00
B and C	1.00	B and C	1.00	B and C	1.00

H WITH R

Greater Series		*Less* Series		*Greater and Less Combined* Series	
A and A	.99	A and A	.97	A and A	.98
B and B	1.00	B and B	.94	B and B	.97
C and C	.90	C and C	.99	C and C	.99

N WITH R

Greater Series		*Less* Series		*Greater and Less Combined* Series	
A and A	.99	A and A	1.00	A and A	1.00
B and B	.78	B and B	.84	B and B	.81
C and C	1.00	C and C	1.00	C and C	1.00

The Equivocality of Instructions

We agree with Friedländer that the natural, common-sense attitude to absolute weight under the conditions of everyday life, with eyes open, is "ein schwer zu bestimmendes Mittleres zwischen A und G-Einstellung." The purpose of a differential instruction is, then, to stabilise and refine the *O*'s attitude. But this task is far more difficult than it at first appeared, and we are not yet within sight of its accomplishment. The differential instructions so far employed are, without doubt, equivocal. So much we may say with assurance; for the rest, the following discussion is merely tentative, and is offered with great reserve.

Suppose (1) that an *O* is asked, under experimental conditions, to discriminate weights as material objects. He has been doing this very thing all his life long, but not under explicit instruction; and the wording of the instruction suggests of itself that he might do otherwise. Hence he is not likely, in the experiment, to maintain the naive attitude of daily life. He may, perhaps, revert to it in the extreme cases of absolute impression, when

his judgment is touched off prematurely. In general, however, the experimental conditions—regularity of procedure, limitation of time, exclusion of sight, nature of task—are strongly against it. How, then, will he base his judgment?

It seems that two principal courses are open to him. He may judge (*a*) in terms of the *felt object*, that is, of projected kinaesthesis. This is, so to say, an external criterion of judgment; it is as if *O* discriminated the weights by the active resistance that they offer. The projected kinaesthesis is likely to be accompanied, if the *O* is at all visually minded, by visual imagery that means the stimulus-object. But he may also judge (*b*) in terms of what we may call *objectified-feel*, that is, of the objectified pressure-pattern on and in the fingers of the lifting hand. This appears to us to be a resident criterion of judgment: it is as if the *O* discriminated the weights by the objective effect they produce upon his hand. And, of course, he may judge (*c*) by recourse now to the one and now to the other criterion, or (*d*) by way of the two criteria somehow combined. At all events, the instruction is equivocal; and there is no reason to suppose that the psychometric functions obtained under (*a*) and (*b*) would be the same.

Suppose, next, (2) that the *O* is asked to discriminate the pressure patterns on and in the fingers of the lifting hand. Again the instruction is equivocal. For (*a*) *O* may take the instruction in the sense of (*b*) above, that is, in the sense of objectified feel; one of our untrained *O*s remarked, as a matter of fact, that he could see no difference between instructions *A* and *B*. Or (*b*) *O* may take the instruction as it is intended, as the inducer of an *A-Einstellung*, but may still be baffled by the complexity (qualitative, extensive, intensive, attensive) of the pressure-pattern under observation; indeed, our quantitative results show that the object of judgment is this case is less stable than that set up (whatever it was) by instruction *A*.

Suppose, finally, (3) that the *O* is asked to discriminate the pressure patterns in the wrist of the lifting hand. Since wrist-kinaesthesis is ordinarily projected, there will (*a*) be a tendency, at any rate for an untrained *O*, to take the instruction as a specialised form of (1*a*). The alternative is (*b*) to adopt an *A-Einstellung*,—in which case, as we have seen, the pattern (at any rate with weights of our magnitude) does not afford an adequate basis for judgment. The inadequacy seems to be due not only to complexity—though complexity may at times play the part here that it plays in (2*b*)—but also and more particularly to a partial, fitful mode of appearance.

This analysis, we must repeat, is tentative only; we have not the data for a positive conclusion. If, however, even the broad lines of the analysis are rightly drawn, we can understand how

easily it comes about that instructions *B* and *C* may be contaminated or, perhaps, actually replaced by a self-instruction of the *A*-type. It is plain, at any rate, that the outcome of our experiments bears out, in general, the criticism that we offered above upon previous investigations (pp. 57 ff.); and that a vast deal of preliminary work remains to be done before the problem of this paper can be attacked with hope of definite result.

Summary

In this study we undertook to trace and to measure the effect of varied instruction upon the perception of lifted weights. We tried to keep the conditions as nearly constant as possible; we used 7 *O*s of degrees of training which ranged from the highly trained to the untrained; and we employed 3 sets of instructions, each one of which might, we thought, set for *O* a separate and distinct *Aufgabe*. At the beginning of a series we gave *O* an instruction in clear form, and then in no way influenced his mode of taking the instruction. We allowed 3 forms of judgment, and permitted *O* to report 'doubtful.' We obtained 2500 observations under every instruction, and an extra 1000 observations as a 'check' series, in which *O* worked under whatever happened to have been his initial instruction. At the end of every series we secured from *O* a report of the criteria of his judgments. We give a statistical treatment of the data obtained; and since we find that in a majority of the cases the *O*s' instructions were exceeded or contaminated, we treat the statistical results qualitatively by writing formulas which show, from the *O*s' reports, the way in which they actually took the instructions. We append discussions of practice, tendency of judgment, and doubtful cases.

The quantitative results, given in Tables I and II, indicate the different effects for every *O* of the different instructions. These differences are expressed by the magnitude of the *h*, the *L*, and the I. U., and to some extent by the P. S. E., the influence of the time orders, the typical tendency of judgment, and the doubtful cases. The need of qualitative interpretation appears from the fact that H, a highly practised *O*, exceeded 1 instruction; Bs exceeded or contaminated 2; By, 2; M, 1; N, 3; R, 1; and S, 3. Thus no single *O* was able to follow every instruction. We discuss, so far as the data at our disposal permit, the reasons for the equivocality of our instructions.

It is clear from the results that, in case of a perception psychologically so complex as the perception of lifted weight, change of instruction does not necessarily bring about a corresponding change of attitude. Furthermore, an instruction

may be so difficult that O cannot follow it, and consequently permits it to become contaminated. Hence all quantitative results must be carefully evaluated in terms of reports furnished by the Os.

There can be no doubt that the most stable basis of judgment is the one which gives the most accurate results. In this experiment, instruction A, which demanded that O judge the weights as material objects, gives the most accurate results. Objective weight, therefore, affords a more stable basis of judgment than pressure-pattern on and in the fingers; and this in turn affords a more stable basis than pressure-pattern in the wrist.

IS THERE A GENERALIZED PSYCHOMETRIC FUNCTION?

By EDWIN G. BORING, Harvard University

Some years ago Urban raised the question of the best mathematical formula for representing a psychometric function.[1] His original test of hypotheses, the results of which favored the phi-function of gamma (normal or Gaussian function), was based upon his experiments with lifted weights measured in grams. It is evident, however, that Urban then believed that his conclusions might be of broader significance than for the sensed differences of weights measured in grams. "We thus," Urban wrote concerning these experiments, "obtain the remarkable result that the foundations of the theory of errors of observation are found in the theory of psychophysical measurement,"—because the results of the lifted weights were best subsumed by the phi-gamma or normal hypothesis.[2] One feels all the way through that Urban is working toward a generalized psychometric function, toward one that is generally valid without reference to the particular stimuli used or to their particular scale of measurement.

In the last number of this JOURNAL Urban[3] cites certain data of Hoisington's[4] and of Thomson's[5] as supporting his earlier suggestion that the phi-function of gamma is the generalized psychometric function. His reason is that both these sets of data seem to show that the phi-gamma hypothesis is approached with practice. It is thus plain that he is still thinking in general terms: for, though Thomson's data are for lifted weights, Hoisington's are for the limen of dual impression upon the skin. Urban does not, however, believe that the general indications of Hoisington and of Thomson settle the matter, since their data are insufficient and show at best only a tendency; but he notes that Fernberger's data on lifted weights, already extant, would furnish the basis for a more thoroughgoing study.

Accordingly Fernberger[6] has computed the deviation from the phi-gamma theory of two *O*s in 28 successive periods of practice using both the "heavier" and the "lighter" judgments in each

[1]F. M. Urban, *Psychol. Rev.*, 17, 1910, 229-259.
[2]*Op. cit.*, 243.
[3]Urban, this JOURNAL, 34, 1923, 496.
[4]L. B. Hoisington, *ibid.*, 28, 1917, 588-596.
[5]W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921, 90-95.
[6]S. W. Fernberger, this JOURNAL, 34, 1923, 498 ff.

case. Fernberger concludes that his "results seem to verify the observations of Hoisington and Thomson in that there is a closer approximation of the observed to the theoretical curves of the psychometric functions as a result of practice," especially in the case of *O* VIII.

(1) I find, however, that Fernberger's data do not convince me of his conclusion. Inspection of his four columns of data does not seem to me to show a tendency, either of increase or of decrease. His conclusion is that the sums of the squared deviations shown tend to decrease with practice. When one plots these data, however, it is difficult to note any consistent tendency for decrease or increase; the most obvious thing about the plotted data is their variability. With so wide a scatter the obvious thing would be to call for the combination of adjacent fractions, but there are other ways of determining a slight tendency in the presence of great variability.

With this end in mind I have adjusted by the method of least squares straight lines to Fernberger's four cases. If there is a tendency for decrease of the deviation with practice, the slope of the line should be negative.

Fernberger's Table	*O*	Class of Judgment	Equation of Adjusted Line
1	II	lighter	$y=-0.00018084x+0.017205$
1	II	heavier	$y=0.00026574x+0.005333$
2	VIII	lighter	$y=-0.00025700x+0.015099$
2	VIII	heavier	$y=0.00002491x+0.009809$

In the computations the last three insignificant digits of Fernberger's data were neglected. In the table x is the fraction or period of practice, y is the sum of the squared percentile deviations and the measure of approximation of observation to theory; that is to say, x is from the first column of Fernberger's tables, y is from the second and third columns.

The coefficient of x is the slope of the line, and it will be seen that approximation to theory increases with practice with both *O*s for the "lighter" judgments and decreases with both *O*s for the "heavier" judgments. It is not, it seems to me, legitimate to argue in this case that it is unfair to represent a practice curve by a straight line: the scatter is too great. On the other hand, it is probable that the method of least squared deviations is unfair to such data: the slope of a nearly horizontal line may be entirely determined by one "wild" deviation near one end, which, when squared, is unduly weighted in determining the line. A method of least deviations might be better were it not for the fact that such a line is not strictly determinate.[7]

[7] *Cf.* M. A. Tinker, this JOURNAL, 34, 1923, 115 ff.

I do not think there is any significance in the fact that the slope of these lines is positive for the "heavier" judgments and negative for the "lighter" judgments. It is more likely than not (6/11) that out of four chance cases one would get two positive and two negative. The chances are even that (*a*) the two positives would be for one *O* and the two negatives for the other, (*b*) the positives would be for one class of judgments and the negatives for the other, and (*c*) there would be no expressible generality. I prefer, therefore, to think that there is as yet no demonstrated tendency for the psychometric function to approach with practice the phi-function of gamma.

(2) I prefer to think thus because I believe that Urban is following a wandering fire in his search for a generalized psychometric function. A psychometric function is a relation between relative frequency, on the one hand, and a scaled continuum of stimulus, on the other. The scale of the stimulus is arbitrary; and it seems out of reason to expect that all scales arbitrarily selected would give the same sort of function. What is there common to qualitative, intensive, and extensive scales, to visual, auditory, cutaneous, and kinaesthetic scales, that they should all in some way be measures of the same thing? If the *O* knew the scale and made estimates with respect to it, we might perhaps expect to generalize all these measures as "errors of observation," but the *O* gives the most consistent results when he knows nothing about the measure of the stimulus.

I have sought elsewhere[8] by certain examples to show the impossibility of the "normal" hypothesis being "normal" in the sense of general. My examples at that time were hypothetical, and I have therefore since tabulated the data from an actual case. It is sometimes supposed that physical characters like the height and weight of "unselected" samples of people are "normally" distributed; but a little consideration of the case shows that weight might be expected to be roughly proportional to volume and hence to the cube of height. Now the Medico-Actuarial Mortality Investigation gives the scatter tables for weight and height separately by sexes and by age-quinquennia.[9] The groups are large: the six quinquennia for men from 20 to 49 years of age represent in every case more than 10,000 cases; the quinquennium 25-29 covers 49,709 cases, the quinquennium 30-34 includes 46,299 cases. Inspection of any of these scatter tables shows at once that the relationship is not linear, but curved in the same direction that a cubic relation would be curved. It follows that the distribution can not be normal for

[8]E. G. Boring, this JOURNAL, 31, 1920, 1-33; esp. 26-29.

[9]*Medico-Actuarial Mortality Investigation*, 1, 1912, esp. 39-56; published by the Association of Life Insurance Medical Directors and the Actuarial Society of America.

both; if normal for one, it would have to be skewed for the other. (As a matter of fact, for the 30-34 quinquennium,—the only one I have computed,—the heights are approximately normal and the weights are decidedly skewed.)

The same situation holds for the normality of the psychometric function. It seems to me hopeless even to search for a generalized function until the problem has been reformulated so that such diverse entities as cutaneous extent and kinaesthetic intensity can be expressed in common psychological terms. Of course, if it were discovered that most psychometric functions are normal, the psychologist would immediately be put to it to find out just why apparently discrete systems of measurement or systems non-linearly related to each other produced the same result. The expectation that such a result would be obtained seems to me, however, so slight as to make the problem unsuitable for the expenditure of time and effort in research.[10]

[10]T. L. Kelley, this JOURNAL, 34, 1923, 408-432, although criticising my article (*supra*), has admitted the point, which I seek here to make, both explicitly (416) and implicitly by his attempt to outline a statistical programme free from these objections (418-432).

COLOR PREFERENCE ACCORDING TO AGE

By George M. Michaels, M.A., Columbia University

Several methods have been made use of in order to arrive at conclusions concerning choice of colors by groups of individuals of different ages. J. F. Dashiell, E. J. G. Bradford, W. H. Winch, and A. Engelsperger and O. Ziegler, who were interested in this field of investigation, tried out their tests but did not come to similar conclusions, since their methods of procedure were somewhat at variance and the material of a different nature.

My material is akin to that of Dashiell's, but was used differently from his. He tested with rectangles 13 by 45 mm. cut from papers of 6 colors mounted on a background of neutral gray cardboard 80 by 90 mm. I made use of a circular background of white unglazed cardboard 63 in. in circumference, upon which I mounted the pure Violet, Green, Blue, Yellow, Orange, and Red of the Milton Bradley Series (1921). The colors were placed in such a way as to leave a white field in the center with a diameter of 2 in. Separating each color was a field of white varying from ¾ in. at the top to ½ in. at the circumference of the inner circle. By using this apparatus I hoped to avoid that jerky movement of the eye which tends to antagonize the reaction effect as brought about by a mixed stimulus and is undesirable in this connection.

Through the courtesy of the principal of P. S. 171 Manhattan, I was able to get my data at that school. 535 pupils were available as subjects. All were boys, whose ages ranged from 6 to 15 years. Judging society as it exists today, these subjects were entirely from the working and bourgeois classes. My previous experience with the pupils of this group enables me to say that the environment they come from is practically identical both in the homes and in their outdoor local activities for all. So that in their constant companionship with one another it would not be amiss to say that their aesthetic judgment could not have been biased by any influence these factors might have had upon them.

Materials and Method

The circular color chart was hung up at the front of the room where it could be seen by all the pupils present. No pupil was allowed to discuss the likes or dislikes of what he saw with his neighbor. The teacher saw to it that this command was carried out. While papers were being passed out to them, the boys were asked to look at the colors on the chart. This was all that was told to them; they did not know the exact nature of the experiment; no questions were permitted. After getting the names, grade

and ages of the pupils they were asked to number their sheets along the left margin of the paper from 1 to 6 leaving enough space between every two numbers to enable them to insert a word. They were then told to put down their pencils, fold their arms and pay strict attention to me. The following uniform instructions were given to all grades from 2A to 9A, their ages ranging from 8 to 15 years inclusive.

"On the chart hanging on the blackboard are six beautiful colors. I want you to look at them and pick out the color you like best. (Pause 10 sec.) Opposite number 1 on your paper put down the name of that color. Ready! Look at the chart again and pick out the color you like next best. (Pause 10 sec.) Opposite number 2 put down the name of that color. Ready! Look at the chart again and pick out the color you like next. (Pause 10 sec.) Opposite number 3 put down the name of that color....." This method was continued for 4, 5 and 6, a pause of 10 sec. being interposed between looking at the chart and writing down the name of the color picked out by the subject.

By a preliminary survey in 1A and 1B I found that the children had rather queer notions about the names of some of the colors. This discovery necessitated individual tests for the two grades. Here the color chart was placed on the teacher's desk and each child was asked to step up and point to the colors. "See the pretty colors" was said to him as I ran my finger casually around in a circle several times. "I want you to look at them and point to the color you like best." As the child pointed I made proper note of his choice. "Now look at them again and show me which one you like next best to the first one you picked." After recording this he was asked to pick the "best one next to that." This was done with the entire 6 colors; the data were carefully noted in each case.

Discussion

My object in taking up this task was (1) to test boys' ability (between 6 and 15 years of age) to perceive colors; (2) to get the affective preferences for single colors in all the groups; and (3) to find out the reliability of the preference between the groups.

In the tables is shown the number of times each color received first choice, second choice, third choice, etc. By multiplying the total number of times a color is placed in first position by 1, in second position by 2, in third position by 3, etc., and adding the products of all the values obtained for each position, a result is obtained representing inversely that particular color's position as obtained from all its positions. This is represented as the total value (T. V.). From the total value obtained the colors can then be ranked in their final order or actual rank in the group (A.R.)

TABLE I

1A 6 Years (27)

Color	1	2	3	4	5	6	T.V.	A.R.
Violet	4	3	7	3	6	4	97	4
Green	3	7	5	3	6	3	92	3
Blue	1	5	6	6	5	4	102	5
Yellow	8	2	5	7	2	3	83	1
Orange	5	4	1	4	4	9	106	6
Red	5	7	2	3	4	5	88	2

The reliability which can be placed upon such results might be questionable; but they certainly show the variability that exists among this group in aesthetic judgment. The results, however, agree in a certain way with those of J. F. Dashiell.[1] B finds the fifth position in both observations, and R and G are one place away in the actual rank. Y and O cannot be reconciled at all. V might just as well be placed sixth as fourth, judging from the actual difference in points between the two positions. In looking at the distribution of preference over the 6 possible positions there is no doubt in my mind as to the inability of the children of 6 years to have any constancy of choice. Between the fifth and sixth positions found by Dashiell there is a difference of a total value of 5; I have a difference of 4. He has a difference of 2 between first and second position; I find a difference of 5. The figures in both cases tend to show the inability of young children to choose with some degree of definiteness. When such small differences are obtained in groups of 105 children and 27 children there should be no hesitancy in stating that children, rather than choosing what they always like, choose what their emotional state of mind stimulates them to do at the particular moment. The remarkable attention displayed by the boys, and their eagerness to show what they really like after all, is accompanied by some psychophysical state that causes the individual to react differently according to his emotional set-up. Therefore let us not accept as final any result that may be handed down concerning what colors these 6-year-old children actually would prefer as first, second, third, etc. All appearing with a tinge of brightness, it is more of an arbitrary choice with such children. The only statement that could be made with certainty is that 6-year-old children and younger prefer to place B at the lower end of the scale. As for the other colors, it is just a matter of temperament.

When we compare the above results with those of the 7-year group some interesting observations come to light.

TABLE II

	2A			7.2 Years			(52)	
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	12	4	2	10	7	17	203	5
Green	3	8	18	5	8	10	193	3
Blue	12	18	9	8	3	2	134	1
Yellow	3	7	6	8	17	10	212	6
Orange	7	8	5	10	8	14	202	4
Red	15	9	9	10	8	1	146	2

B shifts from fifth position to first actual rank. Y reverses its position completely, going from first to sixth actual rank. R

[1]Dashiell for kindergarten boys gives the order of preference as R, O, Y, G, B, V.

and G hold their respective positions. Why then should there be such a sudden change of B and Y? Several possibilities present themselves as a means for explanation. Should we consider that at 6 years and under there is no appreciation of the aesthetic sense; or that the individual undergoes physiological changes, which mature at 7 years, bringing into development a physiological mechanism which causes the individual to see objects in a different light? There might be some psychological basis for explanation where the individual has acquired the keener sense of perception to aid him in his judgments. Perhaps his social environment is changed at this period to allow a little freedom of action—a vital influence upon the individual in general. Could it not be that the innate tendency was unable to find a field of action until properly stimulated? These are some suggestions along the lines of which one could work out a case on the evidence at hand. Whether the explanation be physiological or psychological, we are sure that the facts as depicted in Table II point to a physicopsychical change within the individual. The actual values bring out a greater similarity of choice within this group.

TABLE III

	3A			8.1 Years			(62)	
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	6	12	10	10	9	15	235	4
Green	3	3	10	9	14	23	283	6
Blue	24	18	5	5	6	4	149	1
Yellow	6	8	15	5	14	5	223	3
Orange	6	6	9	14	15	12	248	5
Red	18	14	13	10	6	1	161	2

Judging from Table III it would seem that the only definite change between the 7-year group and the 8-year group in preference lies in the color G, that has definitely taken sixth position without dispute. In comparison with the number of boys tested the figures show a more definite grouping with less random choosing.

TABLE IV

	4A			9.2 Years			(58)	
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	13	7	6	7	12	13	211	3
Green	1	6	9	13	10	19	256	6
Blue	29	13	9	2	2	3	118	1
Yellow	2	5	11	16	14	10	239	5
Orange	5	11	9	14	11	8	213	4
Red	9	17	13	6	9	4	175	2

Although the uniformity of judgment in Table IV is not as great as in the preceding, the actual rank assigned is just as reliable, taking into consideration the difference in total values between each ranking color. There are but two places which

might at first thought cause some comment. V has as many firsts as sixths, with a rather equal distribution over the other positions. O is constant in second and fifth positions. These can be set aside for the reason stated above, and also because of the well-distributed values among the other colors which show a definite position. Furthermore, O stands fully ahead of fifth position, with doubt about interchanging with V which is third.

TABLE V

	5A			10.3 Years			(88)	
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	17	17	12	15	6	21	303	4
Green	4	3	11	19	22	29	403	6
Blue	39	21	11	6	7	4	197	1
Yellow	3	12	15	18	20	20	364	5
Red	13	17	16	15	17	10	300	3
Orange	12	17	20	17	16	6	290	2

In Table V B still retains its lead, over all colors, to a striking degree. The variability in the total value is even more marked than in all the preceding results thus far discussed. V and R present a close T.V., which would make it doubtful according to the total value just where to place V. But, as the following tables will show, the latter color is the most inconsistent in position together with O.

TABLE VI

	6A			11.2 Years			(64)	
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	19	15	10	9	3	8	178	2
Green	4	4	4	11	19	22	295	6
Blue	25	17	11	5	3	3	145	1
Yellow	6	4	6	12	23	13	273	5
Orange	6	12	18	14	8	6	216	3
Red	6	12	14	13	9	10	229	4

Notice in Table VI the descending in the scale of R. In the sixth, seventh, eighth and ninth years it held second position. Beginning with the tenth it recedes to third, and in the eleventh year goes to fourth position, although not with a substantial margin. G remains sixth. V has jumped two positions, going into second position from fourth position, which it held in the 10-year group. B, G, and Y are constant.

In Table VII we notice a striking case of exact position, as in Table VI.

TABLE VII

	7A			12.4 Years			(84)	
Color	1	2	3	4	4	6	T.V.	A.R.
Violet	17	17	15	16	10	9	264	2
Green	2	7	7	17	23	28	388	6
Blue	41	20	14	4	3	2	166	1
Yellow	8	8	15	13	24	16	322	5
Orange	11	14	20	18	13	8	284	3
Red	6	19	12	17	10	20	318	4

This group shows no difference from the 11-year-old group. The ranking is the same; and, allowing for the greater number tested in the above group, the total values are practically alike. The similarity of distribution is also very striking.

TABLE VIII

	8A			13.4 Years		(65)		
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	12	8	12	4	14	15	230	3
Green	4	6	11	11	18	15	273	5
Blue	32	14	11	3	3	2	132	1
Yellow	3	8	10	14	9	21	276	6
Orange	7	8	13	14	15	8	241	4
Red	7	22	8	18	6	4	201	2

In Table VIII R advances two positions. V, Y and O descend one position. G advances one, B remains constant. The only real change to take into consideration is that of R. It is firmly established in second place, being enough points away from third and fourth place to warrant its position.

TABLE IX

	9A			14.3 Years		(57)		
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	6	9	8	11	4	19	226	5
Green	4	6	10	9	12	17	244	6
Blue	37	7	7	2	4	0	100	1
Yellow	3	12	8	9	13	12	224	4
Orange	5	5	16	12	14	5	211	3
Red	2	18	10	12	11	4	195	2

In Table IX V and Y shift two places, the former moving down to fifth position, and the latter advancing to fourth position. O and G lose one position, while B and R remain the same as in the 12-year-group. As compared with Table VIII, B and R are safely lodged in their respective positions. The others change positions with about the same number of points as is noticed in the difference between positions in Table VIII.

TABLE X

	9B			15.4 Years		(18)		
Color	1	2	3	4	5	6	T.V.	A.R.
Violet	2	1	2	4	4	5	76	5
Green	2	7	3	2	1	3	56	2
Blue	12	1	3	1	1	0	32	1
Yellow	1	3	3	2	5	4	72	4
Orange	1	1	2	4	5	5	80	6
Red	0	5	5	5	2	1	61	3

In the group of Table X there are 18 subjects; but the distribution for the group is sufficient to show that B is definite, and V and O are variants. G has made a sudden change of four positions, but figures show it to be justified to second place in this group.

Table XI gives us a survey of the choice of all the groups, if they were placed together in one room and asked to arrange the colors according to preference.

TABLE XI
ALL GROUPS

Color	T.V.	A.R.
Violet	2023	3
Green	2483	6
Blue	1275	1
Yellow	2288	5
Orange	2091	4
Red	1874	2

Returning to the tables, we find that Table IV, giving tabulations for the 9-year boys, agrees with the actual ranks as derived from all 535 subjects, and can therefore be said to be a representative group of the series of groups used for the test.

Supplementary

It perhaps has occurred to the reader that, if the subjects were shown names of colors and asked to arrange them in the order in which they appealed to them as most affective, the results might have been different. Tables XII and XIII show that this is not the case; it makes no difference whether the actual colors or the names of the colors are used. The procedure with the actual colors was the same as that used for the above groups. When tested with the names of the colors the following method was used.

The names of the colors used on the chart were written on the blackboard, and the subjects were asked to write them down in the order in which they preferred them. This test was used first, followed eight days later by a test with the actual colors.

Pupils of P.S. 192 New York were obtained for these tests.

TABLE XII
5A 10.4 Years (27)

Actual Color	1	2	3	4	5	6	T.V.	A.R.
Violet	12	3	7	1	4	0	64	1
Green	0	2	1	2	5	17	142	6
Blue	2	12	6	2	4	1	78	3
Yellow	1	4	2	2	13	6	124	5
Orange	3	2	3	14	2	3	100	4
Red	10	5	6	5	1	0	63	2

TABLE XIII
5A 10.4 Years (27)

Color Name	1	2	3	4	5	6	T.V.	A.R.
Violet	11	4	6	2	3	1	66	1
Green	0	1	1	3	4	18	145	6
Blue	1	12	6	5	1	2	90	3
Yellow	3	3	2	4	13	2	108	5
Orange	3	2	6	10	3	3	98	4
Red	9	6	7	1	3	1	67	2

The reader will notice the marked similarity between the total values of the two tables, and will accept the statement that the individual reacts favorably whether stimulated by the original stimulus or by the substitute stimulus. We can therefore set aside the criticism presumed above. The very close results obtained tend to show that each individual likes a definite color suitable to himself. This little experiment brings out several other interesting points. Compare, *e.g.*, the results obtained in this group with the 4A group as shown in Table IV. They are alike except in two instances. V takes first position and B shifts to third position. But we already know the unreliability of V, which I shall show graphically in a table below. Notice, besides, that there is a difference of 1.2 years between the groups. Comparing the two 5A groups we find that Y and G hold their respective positions, whereas the others cannot be reconciled at all (see Table V).

In all there were 11 groups tested. Each group has its own actual rank which is distinct for itself and can be compared with the others in about as many differences as likenesses. The important factor to remark is the individuality of each group. Table XIV shows the number of positions held by each color in the different groups and throws some light on the shifting of positions by V and O. It brings out the same points as are expressed in Table XI, but includes the 5A group from P. S. 192 not included there. The table will also show clearly those colors which were more or less constant and those that shifted.

TABLE XIV

POSITION HELD BY THE COLORS IN ALL GROUPS

Color	1	2	3	4	5	6	T.V.	A.R.
Violet	1	2	2	3	3	0	38	3
Green	0	1	2	0	1	7	55	6
Blue	9	0	1	0	1	0	17	1
Yellow	1	0	1	2	5	2	49	5
Orange	0	1	3	4	1	2	44	4
Red	0	7	2	2	0	0	28	2

Conclusions

(1) There are at least 4 colors which are definite to a certain degree; they are B, R, Y and G.

(2) V and O are very unreliable, and are apt to be chosen in any position, especially below the second position.

(3) There is evidence here that environment and social status have something to do with the development of preference for colors (cf. Tables XII and XIII with Table V).

(4) Beyond the 6-year group age is a more dependable factor in the preference of color.

(5) The variability and unreliability of the aesthetic judgment of the 6-year group is so great that it cannot be compared with the judgments of the rest of the groups.

(6) It is just as good to use the color names for the actual colors in performing this test.

(7) The 9-year group shows as good judgment as all the groups combined. It thus is entitled to *representative rank* of the entire 11 groups.

(8) B, R and G are the most reliable colors in the series.

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SYNAESTHESIA IN THE PROCESS OF REASONING

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The investigation here reported is a continuation of the work on the synaesthesia of a blind subject commenced by Wheeler[1] in 1920 and carried on jointly since then by Wheeler and Cutsforth.[2] The object of these studies has been to make an intensive survey of the mental content of a highly synaesthetic subject, and to compare his mental content with an asynaesthetic subject as check observer. During the earlier part of the investigation an asynaesthetic blind subject was available whose detailed introspections could be compared with the data on synaesthetic processes.

In the present investigation the author was, as before, the synaesthetic subject. In the absence of a trained and asynaesthetic blind subject as check observer, comparative data were obtained from introspections by R. H. Wheeler. The material consisted of analogies, absurdities, simple and difficult abstract problems, all of which had been selected for the purpose of initiating reasoning processes of varying length and complexity. Analogies such as "table is to furniture as dog is to?" or "city is to town as elephant is to?" were designed to initiate reasoning processes of a minimum complexity. Of a slightly more complex nature, perhaps, were the processes of reasoning in locating the absurd features of such statements as: "The road from my house to the store is down hill all the way to the store and down hill all the way back home." Among the simpler problems of a more abstract nature were the following: "If an electric fan were attached to the rear of a boat and the air current from the fan directed toward the sail, would the boat move?" "Does the water line of a boat rise or fall as the boat passes from fresh to salt water?" Finally, more difficult problems were employed, such as: "Justify this statement: 'A first cause is logically inconceivable';" "Justify the statement: 'Being is doing'."

Any problem was discarded whose solution was stereotyped for the reason that the material happened to be too familiar or too easy. The results upon which the following discussion is based were gathered from data on 25 successful problems for the synaesthetic *O* (*A*), and 20 from the check *O*

[1] *Uni. of Ore. Pubs.*, 1, No. 5, 1920; *Psych. Rev.*, 27, 1920, 313-322.
[2] *Uni. of Ore. Pubs.*, 1, No. 10, 1922; *Journ. of Exp. Psychol.*, 4, 1921, 448-468; *Psychol. Rev.*, 29, 1922, 212-220; this Journal, 33, 1922, 361-384.

(*W*). The problems were presented to the *O* orally, with the following instructions: "I shall present a problem to you. Solve it as rapidly as you can and then give as complete an introspective account as possible of your mental processes." The experimental work was performed under optimal laboratory conditions and with the usual precautions employed in an introspective study.

The following are typical introspective data.

Observer A. Problem: Table is to furniture as dog is to what? Response: animal. "As *E* said 'table' my consciousness of the word consisted first of a fan-shaped and yellowish-black cloud appearing in the center of the visual field and moving immediately upward and to the right into the periphery; and secondly, of a fragmentary visual image of a table, with one distinct corner pointing toward me. The first image was a synaesthetic consciousness of *E*'s voice. The second image came into the visual field from the right and beneath the first image; it was colored a faint yellow, partly from the color of pressure sensation, and partly from the dull drab of the word, table. The resulting color was a light gray-brown. From this point on, the synaesthetic awareness of *E*'s voice did not enter into the perception of the problem. Next there appeared a synaesthetic image, bright brown in color, which shaped itself in the form of bars, criss-crossed. It took its place beyond the corner of the visualized table, but within the limits of the angle formed by the two visible table-edges. This was my consciousness of the word, furniture. Between these two sets of imagery for table and for furniture there developed a triangular patch of white, frosty brightness. This meant to me the relationship 'is to.' While it is difficult to describe this latter experience, its mental content was simple. In appearance this frosty-white patch was a structural link between the imagery of table and furniture, relating them spatially at the apex and openings of an angle. For the moment my visual attention centered itself upon this angle, which shortly tended to open a little wider. These latter experiences constituted the anticipation of the meaning 'genus-species'' Then, immediately beyond this angular form, appeared synaesthetic visual imagery of the word, dog—a smoky-blue bar. This changed almost at once into the greyer blue of the abstract concept, dog. With lightning-like rapidity and with no conscious purpose, the frosty-white and triangular image of an 'is to'-relationship developed just beyond the color-patch which stood for dog. This constituted an anticipation of the response. Attention lingered for a moment at the opening of this newly formed angle. No color filled this space. It was a neutral grey. My attention then returned to the first part of the analogy, present to consciousness in terms of an angular form with the color for table at the apex and for furniture at the right of the opening. The color for dog took the place of the color for table at the apex of this form. In terms of eye-movement and shift of visual regard from dog to furniture and back again, I searched for a response-word which might have had the same relation to dog as furniture had to table. My attention would also shift to a void place—neutral grey—to the left of the coloration for furniture where the response-imagery should appear. After a brief hesitation of this sort there appeared, without warning, a yellow synaesthetic image for 'animal.' This yellow patch was my response just as the *word*, animal, would be your response. I then became conscious of vocal-motor imagery for the first time in this experiment. I was trying to translate the yellow synaesthetic imagery into words. This vocal-motor imagery was synaesthetic. The problem for me was solved with the appearance of the yellow visual imagery for animal."

Observer A. Problem: A first cause is logically inconceivable. Response interpreted into words: Correct; a cause is always the effect of a cause. "I perceived the word, first, in terms of visual attention to the 'one'-section of my digits number form. *E* hesitated for an instant in reading the problem, during which time the remainder of the number form was rapidly rising in focality. Then, in response to the word, cause, there was very rapid eye-movement to the right and slightly upward, consciousness of which came to me in terms of a black streak which contained numerous internal movements resembling tiny currents of rain-water running down a window-pane. This experience meant 'cause.' The meaningfulness of this imagery lay in the rapid fashion with which the movement appeared and in the blackness. Out at the end of this movement (for, at the time, the experience meant to me 'movement' and not 'streak') there was nothing in visual attention save neutral grey. For a brief time the visual line of regard wandered over this blank area, turning back from time to time to the region where the imagery of 'first' had gone, and was just fading out. Here there appeared a faded-out, brickish-red-brown, synaesthetic image which by its color meant 'effect.' From this point on visual synaesthetic imagery began to move about rapidly. The imagery which stood for 'effect' slid over as if pushed into the position of the former imagery of 'first', whereupon this latter imagery lost its whiteness—a color which meant 'one'—and took on a faded-out brown. In this fashion the meaning, first cause, had turned into the meaning, effect. There was no attempt to utilize this imagery further. Synaesthetic imagery of 'one' together with eye-movement had meant 'first cause.' When no change came over this imagery at the outset, and eye-movement shifted to a place just below it where there appeared the change of coloration just described, the problem had been solved, implicitly. The temporal and spatial relations of this synaesthetic imagery meant 'cause is the effect of a cause.' The reddish image of 'effect' blotted out the whitish image of 'first cause.' Immediate relaxation set in, and the flow of imagery pertaining to the problem stopped."

Observer W. Problem: Justify the statement that being is doing. Response: Isn't true: cannot be justified. "Consciousness of *E*'s voice at first focal in auditory terms, together with marked attention to and recognition of the words 'being' and 'doing.' Before attention had entirely left the auditory perception of *E*'s voice—while auditory qualities were tapering off in consciousness—there developed a very sudden and rapid comprehension of the problem-situation. The first stage of this was a fleeting visualization of the word, being, in front of me and slightly to the left, typewritten and projected upon a blurred brown and black background. Just to the right and on the same line with the word, being, appeared the other word, doing. Momentarily there was a rapid shift of the visual line of regard from one word to the other, left to right, with attentional emphasis upon the 'ing' part of each word. This was the first stage, or a consciousness which failed to develop further just at this time, namely, that both words had to do with something dynamic. At this juncture there was a momentary tendency to relax, a motor attitude of acceptance of the solution. Apparently, I had taken my beginning recognition of the meaning, dynamic, as a solution of the problem. But this tendency to relax was suddenly interrupted. Attention returned, visually, to the words again, and with a very rapid sweep of the line of regard across the word, doing, there appeared the syncopated verbal imagery 'doing is action,' 'the real is action.' In this imagery hardly more than the word 'doing,' the 'act' of the first 'action' and the word 'real' stood out with any degree of clearness. Then completely relaxed and attention slumped. Could not progress any farther for a time. There was a developing consciousness that the problem had not been solved; could not say why; this was a motor attitude of

questioning, together with a verbal repetition of the problem several times and tendencies to frown. No meanings developed. Verbal imagery seemed detached and meaningless, when suddenly I found myself visualizing the word, being, in large black and white letters in space before me, followed at once by focal auditory-verbal and visual imagery of the word 'state.' There was exceedingly rapid eye-movement from one to the other. This entire procedure was interpreted to mean 'being is static.' Then there was flashy visual imagery of the first part of the word, becoming; very rapid and syncopated vocal-motor-auditory imagery, 'becoming-action,' then implicitly meaning and later explicitly interpreted to mean, 'it is becoming which is action (not being).' Meanwhile there was a very marked growth of bodily strains, particularly about the throat and chest, a tendency to sit more erectly in the chair, followed by a return of the visual line of regard to focal, visual imagery of the word, 'doing'. This sequence implicitly meant that doing belongs with becoming. There was then a complete relation, as if the problem had been solved. Then the meaning developed: 'Isn't so,' verbally and with slight return of visual attention to the position in space occupied previously by the two words, being and doing. This was followed by a momentary tenseness with verbal imagery, 'mistook being for becoming;' whereupon there was set up a long train of very clear-cut vocal-motor-auditory processes: 'It is becoming which is action, or doing; it is not being which is action.' Meanwhile attention was turning toward the task of introspection, and the final meaning developed visually and verbally, 'to become means to do,' 'being means simply to be.' With this came a motor attitude of acceptance."

Summary of Introspective Data. For practical purposes we shall divide the process of reasoning into two developmental stages: (1) comprehension of the problem; (2) development of the solution. The second stage involves, characteristically, the application of an abstract concept to the present situation.

A's procedure consisted first of perceiving *E*'s voice in terms of visual, synaesthetic imagery. The color, behavior and form of this imagery were invariably determined in part by the quality of *E*'s voice and in part by outstanding letters of the words. Secondly, there very rapidly developed the meaning of the problem, likewise in terms of synaesthetic imagery, and generally with the aid of a 'reasoning form' or some schema by means of which the synaesthetic imagery took on definite spatial relationships. Thirdly, *A* then proceeded toward a solution by altering or modifying this schematic and synaesthetic imagery. A solution invariably consisted of fitting visual imagery of the appropriate shape and color into his schema, a process which always involved easily observed eye-movements, changes in the line of regard, and highly focal movements of imagery. These latter movements always contained a certain amount of blackness, which represented kinaesthesis.

Both in comprehending a problem and in solving it meanings accrued to the color, spatial relationships and mobility of synaesthetic imagery. As a datum of consciousness the meaningfulness of an experience was referred to the mobility of synaesthetic imagery. The development of a 'form' was an important

feature in the comprehending of any problem as well as in solving it. For example, *A*'s procedure in completing the analogy: City is to town as elephant is to what? began with the schematization of the problem in the form of an angle with its apex in his direction. Colored imagery, meaning 'city,' took its position within the apex of the angle. Another blotch of color, meaning 'town,' took its place at the opening of the angle. Between these spatialized images appeared the frosty-white color which always meant an 'is to'-relationship. But no sooner had all this imagery appeared than the fact stood out, in terms of visual attention, that this arrangement of imagery did not fit the analogy. City was at the small end of the angle and town at the large end, while a city is generally larger than a town. From this point on *A* found it necessary to reinterpret the entire problem by rearranging his schematic imagery. The opening of the angle closed in and the apex widened out, thus reversing the situation. It was in this fashion that meanings developed in terms of image-behavior. *A* then constructed a second angular form in which elephant appeared at the opening of the angle. The process of searching for the final answer consisted in trying to fit some color into the apex of this angle. As usual the response did not appear in verbal terms but in patches or clouds of synaesthetic coloration. Various species of animals appeared in terms of synaesthetic imagery, localized beyond and above the position of the angular schema. The fact that none of this colored imagery migrated to the apex of the angle, and the subsequent disappearance of the entire schema, constituted a failure to solve the problem. *A* then found himself reattending to the schema by means of downward eye-movements. At the apex of this reconstructed angle there developed a patch of neutral grey which meant 'nothing,' and to which *A* responded in verbal fashion with the word 'nothing.'

The process of arriving at a conclusion in such cases consisted of a progressive course of visual, synaesthetic imagery whose spatial arrangements, coloration and movement were implicitly meaningful. That is, they functioned as meanings without a conscious recognition of the meaning. The schema acted as a core. If visual imagery distorted the schema, the solution of the problem was delayed. In other words, if a conclusion was to be reached, the problem-form must have developed and operated characteristically, smoothly, without misplacement of imagery and without conflicting movements of imagery. The use of problem-forms seemed to be a means of attenuating and mechanizing the operation of visual imagery. It provided for economy of attention and facilitated the use of abstractions. One complex situation could be associated with another with marked facility. The entire procedure of using

these forms was *A*'s substitute for the more common methods of attenuation and mechanization that are made possible by the use of verbal imagery.

A's procedure in solving the simpler problems not presented in the form of analogies was quite similar to that described above. In the boat and fan problem the conclusion that air-pressure from the fan impinging against the sail would not move the boat but would bend or break the sail appeared in the very simple visual imagery of seeing the sail bend over. The instant the sail was seen to bend, *A* relaxed. The *Aufgabe* had been fulfilled. Before the solution could be explained to another person, however, it was necessary to elaborate upon this mental content.

In solving the more complex and philosophical problems we still find this marked tendency to schematize and simplify. For example, in the first-cause problem meanings first developed as in the simpler problems. The solution consisted in the use of domino-like forms, colored to mean causes and effects. Relationships were present to *A*'s consciousness in terms of movements of these forms. A segment or section colored to mean 'cause' moved ahead, taking the place of the segment colored to mean 'effect.' To the left of this series of moving patches appeared a section colored to mean 'first cause.' That there is no such first cause, logically, came to *A* in the reddish-brown color which took the place of the 'first' cause, moving it toward the right. Implicitly this meant a solution of the problem. Explicitly this was interpreted by means of verbal-synaesthetic imagery 'a cause is always the effect of a previous cause'.

In every instance of such use of moving forms watery grey marks of kinaesthesis bordering the edges of variously shaped colorations gave to the experience a synaesthetic-kinaesthetic character. In certain instances the kinaesthetic symbols resembled comet-like streamers trailing behind the moving forms.

W's procedure in reasoning did not differ functionally from *A*'s. *W* comprehended the problem by means of auditory-visual perceptions, incipient verbal processes, and motor attitudes of familiarity or acceptance. He solved his problems in terms of fleeting schematizations, vocal-motor imagery, visual-verbal imagery and kinaestheses of recognizing. As with *A* certain meanings failed to develop explicitly, yet the problem was solved; *i.e.*, the reagent relaxed as if he had explicitly solved the problem. For example, in the being-doing problem, eye-movement from the visualized word 'being' to the visualized word 'state' at its right meant implicitly in this setting and connection that 'being' was synonymous with 'state'. But the explicit relation, synonymous, came to consciousness only with

the appearance of additional imagery. In *A*'s case the majority of meanings were implicit. Images behaved as if they constituted fully conscious meanings. A maturing process, however, was necessary before these implicit meanings became recognized data of consciousness.

The greatest difference between *W*'s and *A*'s experiences in reasoning lies in the patterns of mental contents used. In *W* auditory, kinaesthetic and other non-visual qualities became focalized and operated as such along with various motor attitudes. In *A* mental contents were confined to the visual modality with the exception of those vague, indescribable sensory experiences with which the synaesthetic imagery was associated. On the other hand there are numerous resemblances in the mental processes of *A* and *W*. Both reagents used visual schemata. *W* was synaesthetic in the sense that visualizations of words oftentimes enriched his auditory-verbal perceptions. Both *O*s solved their problems in implicit fashion without definitely recognizing the meanings of their own mental processes, as long as interpretative periods failed to develop. Both found that such interpretative periods as did develop extended into their introspective procedure. Both found that implied meanings matured in terms of incipient motor reactions—motor attitudes of familiarity and acceptance together with verbal imagery on *W*'s part, and visual-synaesthetic verbal imagery with eye-movement on *A*'s part.

The process of reasoning in both *O*s has suffered a short-cutting in its logical steps, so that the noting of similarities, the relating of one situation with another, the transition from simple to complex meanings, shifting from one logical step to another and the like all take place in terms of shiftings in the line of visual regard from one part of a schema to another, or in a change in size of colored imagery, or in eye-movement from one focalized region to another. This short-cutting means that, unless an anticipated stage in reasoning develops rapidly, the preceding stage upon which it is based may vanish before the succeeding step matures sufficiently to carry on the process. In this way, if one set of imagery disappears before its successor appears, the entire process of reasoning is halted. Frequently the process of reasoning took place so rapidly, in this fashion, that the individual did not have time to interpret the meaning of what he was doing. He relaxed as if he had solved a problem without knowing what the problem was that he had solved.

In both *O*s logical steps were a matter of mobility of imagery —the change from one image to another, involving shifts in position and size. There were found no imageless processes or relational elements.

By far the most complex part of *A*'s process of reasoning had to do with the conscious identification of meanings. This process of identification may be called the transition from implicit to explicit meaning. After a fashion *A* identifies the meanings, implicit in his behaving colored imagery, by attending to the changes which there take place. Because *A* is so dominantly visual, this consciousness of change turns out to be hardly more than attention to an altering of position or to an increase in brightness and size. Such changes, however, are very numerous and complex. On the other hand *W* identified implicit meanings, in a general way, by having recourse to motor attitudes of familiarity, and in a concrete fashion by means of verbal imagery together with focal attention upon a particular detail of his visual or other concrete imagery. Only when the process of identification is extremely sudden, vigorous and complex do kinaesthetic processes function in *A* as they do in *W*.

Here we find an interesting difference between *A* and *W*. While in both *O*s implicit meanings become explicit by processes of elaboration, *W*'s procedure is always kinaesthetic and *A*'s procedure is always visual. While *W*'s contents become motor in responding to meanings, *A*'s remain visual although some change must always take place in the visual imagery. For example, familiarity in *W*'s case is a motor attitude; in *A*'s case it is an enlargement and brightening of a synaesthetic visual image. *W* refines his meanings by verbal processes. *A* refines his meanings by changing the visual setting of a colored synaesthetic image. In *W*'s case difficult identifications of meaning are strongly verbal. In *A*'s case they are strongly eye-motor.

Certainty, uncertainty, doubt, acceptance and rejection do not consist of motor attitudes in *A* as in *W*, unless they rise to a very high degree of intensity and complexity. Like familiarity, they are represented in terms of image-behavior. Certainty, for example, consists of a definite standing-out, a focalization of the brightness of visual synaesthetic imagery together with an increase in qualitative detail. Motor phenomena are here confined to focalized eye-movements of scrutinizing these changes. Uncertainty consists of searching movements of the eyes and of changes in the line of regard when synaesthetic imagery fails to mature in any given situation. Doubt is like uncertainty until it becomes intense, when bodily kinaestheses develop. But at this point a peculiar thing happens. The instant the attitude becomes kinaesthetic, the synaesthetic visual imagery by means of which *A* becomes conscious of this attitude either blurs or entirely blots out the visualized object of the doubt. This fact holds quite as well for all the motor attitudes in their intense forms. This explains why localized bodily kinaestheses play so small a rôle in *A*'s consciousness. Their presence is invariably a

condition for the obliteration of any consciousness of the object. The synaesthetic visual imagery aroused in the attitude claims the *O*'s attention to the exclusion of all else at the time. In this way motor attitudes obscure *A*'s thinking and even prevent the normal course of imagery.

Acceptance consists of a very slight nod of the head, a leaning forward toward the visual field, and the sudden dropping of projected imagery toward the lower portions of the visual field. Rejection is a rising of imagery in the visual field with a slight upward jerk of the head or an incipient tendency to tilt the head backward. In case the kinaestheses of either attitude become widespread or intense, *A*'s attention is claimed by the visual synaesthetic imagery of the resulting strains. The first stage of this shift is the appearance of very active, black-brown synaesthetic representations of kinaesthesis along the borders of projected visual imagery. The second stage is the focalization of visualized bodily strains, localized upon the body. *A* finds it impossible to attend to these latter strains and maintain imagery in the projected visual field; hence the interference of motor attitudes with projected visual imagery used in carrying on a process of thought.

It is interesting to note that there is a marked difference between a synaesthetic visual image detached from its indescribable sensory associate and one attended by such an associate. The former is invariably static, in the sense that there is no visual disturbance within it. It is motionless, internally. For example, *A* may have an auditory image of a locomotive whistle; but that it is an auditory experience, in meaning, is merely a matter of subsequent and explicit interpretation from its setting and context. On the other hand, *A* may experience an auditory image of a locomotive whistle with the auditory meaning present. One feature alone identifies the experience as auditory. It is a certain dynamic aspect of the image. Within its own boundaries there takes place a continuous disturbance likened to the swirling movements of smoke rising from a chimney. The image appears phenomenologically as a seething mass of amorphous coloration. It is this dynamic feature of the imagery which gives to it the quality of meaningfulness, and is traceable to kinaesthesis.

The act of attending, in *A*'s case, is a visual act whose kinaesthetic features are usually confined to eye-strain and movement, both of which are present to *A*'s consciousness as whirlings, spirals and lines of various degrees of blackness, all projected into the visual field. For this reason it often happens that the facts of eye-movement and strain are not noticed or are given little attention.

Summary and Conclusions. There were no imageless or non-sensory contents of consciousness either in *A* or in *W*. In *A*'s case no reasoning takes place in the absence of synaesthetic imagery. Synaesthetic imagery is an invariable and an essential component in the development of meaning. Under the *Aufgabe* to solve problems, relationships develop by means of problem-forms and in the shifting of synaesthetic colorations in these forms. Logical steps develop in terms of changes in spatial relationships of imagery, changes in coloration, and in movements of the line of regard. There is found no structural criterion for reasoning beyond a uniformity in the use of schemata. The functional criterion of reasoning consists in the application of a concept to a present problem. We were not interested in making a contribution to the process of reasoning as such. We were merely interested in tracing, further, the rôle of synaesthesia. As in all of our other experiments, no functional differences were found between our synaesthetic and asynaesthetic data. In connection with the process of reasoning we have pointed out that the difference between implicit and explicit meaning holds in reasoning as well as in the simple development of meaning.[3]

We find, in reasoning, no exception to the general rules laid down in our previous researches; that, so far as observer *A* is concerned, synaesthesia is a mechanism in the normal development and use of meaning; that synaesthesia is not an extraneous form of association; that it is essential to the cognitive activities of the subject who possesses it because it is the only structural tool he has of comprehending meanings; that it varies from any ordinary process of perception or conception only in the type of imagery which dominates and in the degree to which the imaginal component dominates the sensory component; and that synaesthesia is not alone a perceptual phenomenon. It has to do with the development of meaning, and is quite as conceptual as it is perceptual. It pervades the subject's entire mental life.

[3]R. H. Wheeler, The Development of Meaning, this JOURNAL, 33, 1922, 231.

NOTES ON THE HORIZON ILLUSION: I

By F. ANGELL, Stanford University

In the third part of Nagel's "Handbuch der Physiologie" Oskar Zoth gives a brief account of the present status of the horizon-illusion theory, and adds to it a condensed statement of his own hypothesis taken from an extended article in Pflüger's *Archiv*.[1] Zoth's theory is that the difference in apparent size of constellations at different altitudes results mediately from the tendency which the eyes have to diverge when raised to look upward, and immediately from the effort to counteract this tendency in fixating an object thus viewed. For a given altitude the commonly cited factors of comparison with intervening objects, aerial perspective and so on come into play and vary the amount of the illusion. Thus the differences in the apparent size of the moon as it rises above the horizon may be the effect of aerial perspective, but the varying size at different altitudes on the same night is the outcome of the divergence-tendencies of the uplifted eyes.

The crucial test which, according to Zoth, puts all rival theories out of consideration is the view of the moon through darkened or colored glasses which obscure all objects in the field of vision save a disk of the moon itself. Under these circumstances where, Zoth holds, there can be no question of comparison with intervening objects, of aerial perspective, or of a flattened firmament, the illusion still persists: the moon seems larger. By elimination the only remaining factor is the upward lift of the eyes, which accordingly must be responsible for the illusion.

If this be true, it follows that the illusion must vanish if the rising moon is viewed with upward gaze, that is, with the chin sunk on the chest; and it must be developed when the zenithal moon is viewed in direct gaze, as when one lies on one's back. Zoth finds that both of these effects take place under the given conditions. He also finds, in agreement with his theory, that the apparent size of after-images of the sun projected against the sky and of the disk of the sun as seen through a fog is conditioned on the upward sweep of the eyes. Applying Helmholtz' suggestion of reflecting the horizon moon to the zenith[2] (and conversely), but using prisms instead of glass plates, he finds that the diminishing effect with upward gaze comes out strongly; the reverse effect, however,—the increase in apparent size—failed to appear when the eyes were lowered: a result which Zoth attributes to an unavoidable tendency to make the surface of the prism instead of the horizon the *point de repère*.

From all this it follows that the horizon-size of the moon is normative and the zenithal illusory, and that the flattening of the firmament at the zenith comes from the lessening of the 'standard' horizon-distances.

In support of Zoth, Alfred Guttmann carried out comparison experiments with surface magnitudes, the members of each comparison differing in altitude by 40°; and in accordance with Zoth's findings he reports that the upper surface seemed to lose in size,—the loss, under the conditions of the experiment, amounting to about 3½% of the linear dimensions.[3]

[1] O. Zoth, Einfluss d. Blickrichtung u.d. scheinbaren Grösse d. Gestirne, *Arch. f. d. ges. Physiol.*, 78, 1899, 363.

[2] *Phys. Optik*, 1896, 776.

[3] A. Guttmann, Blickrichtung und Grössenschätzung, *Zeits. f. Psych.*, 32, 1903, 133.

Guttmann's method, in the first instance, was to compare pairs of cardboard surfaces 2 cm. in width and from 3 to 10 cm. long placed in a perimeter at positions differing by 40° of elevation. In the first series the norm was placed on a level with the eye and the variable above, the comparison being made in the time order norm-variable. On account of the fatigue incurred in gauging the upper surface, the judgments were given in but one time order, so that it is impossible to say how much of the overestimation was due to the Fechnerian time-error.

In a second series, Guttmann compared surfaces on the level of the eye with others placed 40° below in both space orders, but in one time order. He found the error of estimation negligible when the gaze was lowered; but in rising from the lower to the higher position a negative error of 1.46% appeared, which he regarded as confirmatory of his first series. On examining the 4 series from which the average of 1.46% was made up, one finds that the errors occurred only with the 3 and 4 cm. surfaces, while those of 5 and 10 cm. showed no difference: assuredly a curious outcome, which would seem to call more for explaining than for averaging. In general, the experiments were carried out with too little attention to established psychophysical methods; and the most that can be said is that they seem to indicate a small decrease in apparent size of surfaces in raising the eyes from a level to an elevation of 40°.

It occurred to the writer that in experimenting on this problem one might well use the measurement of after-images projected to different altitudes,—a method which had shown its usefulness in investigating other phases of the horizon illusion. Accordingly, the simple apparatus for such an investigation was assembled, consisting of a Hering *Kopf-Halter*, 2 backgrounds, and an inducing surface, *i.e.*, 'stimulus.' The backgrounds were medium-gray paper ruled with lead pencil in cm., placed the one on a level with the line of vision, the other at 40° of elevation, 45.5 cm. perpendicularly distant from the cyclopean eye of the *O*. The inducing square, 3 cm. on the edge, was a piece of black paper on a white background, fixed 41.5 cm. from the center of rotation of the cyclopean eye on the long arm of a lever, so as to be easily pulled aside when the after-images were projected.

It follows from this arrangement that the eyes were raised or lowered, as the case might be, 40° in comparing the size of the after-images on the two backgrounds; and also that the size of the image projected would be somewhere in the neighborhood of 3 cm. square. Six series of comparisons were made on as many days by skilled *O*s with from 10 to 13 judgments in each series, the time of exposure of the inducing square being 12 sec. As the size of the after-image seemed to be very close to a square of 3 cm. on the edge, a square of this size was drawn around the fixation points of the two projection planes; and the process of comparison finally worked itself out into noting how accurately the after-images fitted into their 'frames.' Consequently, notes in the protocol-books read as follows: "Fitted both squares accurately;" "Fitted the lower square but 'spilled' over the upper;" "At first spilled over both bounds of the norm;" "Immediate fit of variable." The 'spilling' indicates the effect produced by the after-image when it did not fit accurately in its 'frame.' At first there was more or less spilling over both frames, which seemed to result from the tendency to shift the gaze from the fixation-pont in the center of the frame to its border, to see if the edges of frame and after-image coincided. The protocol-notes indicate that the images fitted accurately in each frame when the fixation of the center points was sharp and quick. At other times there was so much spilling on the first square before the dimensions of the image were noted that the image became too faint to be applied to the second square. But in general, with or without spilling, it was found that the image fitted both squares accurately.

In 4 of the 6 series the eyes were raised from the level to 40° of elevation, and in the remaining two they were lowered over the like angular distance, *i.e.*, in the reversed space order. Had the two images thus projected differed by 1 mm. in the length of their sides, the difference would have been perceptible; but no such difference was noted, nor indeed any difference.

Granting, however, that the upper image was larger than the lower by 1 mm., the amount is far below the extent of the horizon-illusion according to general judgment, and probably according to careful estimates: though, as these latter have been for the most part measurements of arcs of the heaven, they are only indirectly applicable to the problem in hand.

Zoth seems to think that Guttmann's figures support his theory; but to the writer they seem to bear more the relation of a petard to Zoth's structure than a buttress. At the best, a factor which only amounts to 3.5% is a weak prop for explaining a change in magnitude which is popularly estimated all the way from two to one hundred-fold.[4] Of course, popular estimation is an unstable foundation so far as quantitative results are concerned; but in this case the voice of the people seems to be the voice of the scientifically elect. To Kepler, for example, the illusion seemed "ingens et gigas." From time immemorial the illusion has forced itself on mankind as something remarkable, if not prodigious; and any attempt to explain it by reference to a quantity which barely amounts to a just observable difference for circular magnitudes assuredly lacks compelling power.

In the course of his admirable survey of the horizon-illusion from Aristotle down to the present[5] Reimann raises a weighty objection to Zoth's theory, to the effect that whether the gaze be directed upward, downward, or straight ahead makes no difference: the illusion persists. "I have repeatedly observed the rising and the setting full moon as well as the setting sun in all possible positions of the body without perceiving the slightest differences in apparent size from that found with the body upright."[6] Reimann also finds that his own experiments with surface magnitudes are contradictory of Zoth's findings.

In any case, convergence strain, which is the basis of Zoth's theory, can hardly be regarded as a firm support in view of the researches of Hillebrand,[7] Archer[8] and Bappert[9] on accommodation and convergence. Even under abstract laboratory conditions and for short distances these processes failed in determining distance and size. Hering found no effect of convergence on negative after-images projected in a dark field. The writer also was unable to see any change in after-images as a result of convergence, save in the case of passing from parallelism of the optic axes to strong convergence, where an indistinct shrinking of the image was occasionally noted. Careful introspection, however, showed that the act

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[4]A similar objection has also been raised by W. Filehne, Ueb. d. Betrachtung d. Gestirne mittelst Rauchgläser u.ü.d. verkleinernde Wirkung d. Blickrichtung, *Arch. f. Anat. u. Phys.*, 1910, 523.

[5]E. Reimann, Scheinbare Vergrösserung d. Sonne u.d. Mondes am Horizont, *Zeits. f. Psych.*, 30, 1902, 165.

[6]*Op. cit.*, 165.

[7]F. Hillebrand, Das Verhältniss von Accommodation u. Konvergenz zur Tiefenlokalization, *Zeits. f. Psych.*, 7, 1894, 97-151.

[8]K. W. Archer, Zur Frage nach d. Einfluss von Akkommodation u. Konvergenz auf d. Tiefenlokalization u.d. scheinbare Grösse d. Sehdinge, *Zeits. f. Biologie*, 62, 508-535.

[9]J. Bappert, Neue Untersuchungen z. Problem d. Verhältnisses von Akkommodation u. Konvergenz z. Wahrnehmung d. Tiefe, *Zeits. f. Psych.*, 90, 1922, 167-203.

of convergence gave rise to an impression of a narrowing of the image through muscular strain, so that the incipient judgment 'smaller' came from a sort of symbolic visualization of the convergence process.

Bourdon found that he could vary the size of after-images in the dark according to the distance at which he imagined them projected. The image of the sun appeared to him as large as a franc or as the sun itself, "suivant que je me la réprésente à peu de distance de moi ou à la distance ordinaire de la voûte celeste"[10]. But this is in no wise a convergence effect: in fact it can be developed with convergence or divergence indifferently.

Zoth's crucial experiment was, of course, the test with darkened glass; but several investigators, Reimann among them, have insisted on the delicate and difficult nature of this experiment. The glasses must be smoked to a nicety, so as to be just transparent for the disk of the sun or moon but for nothing else. Noted or not, the slightest impression of the presence of any other object than the disk destroys the value of the experiment. The factor of unnoticed impressions which are effective in determining size or distance effects will be discussed further on: here the writer would enter a general *caveat* against assuming that influences not noted are non-existent.

The number if not the variety of theories seeking to account for the horizon-illusion almost equals the luxuriance of explanation for sleep and dreams: linear and aerial perspective, convergence and accommodation, comparison with intervening objects, and the inductive effects of projection,—combinations of these factors in varying proportions account for a goodly number of theories, not to speak of explanations which promote wonder if not edification. Of the last-named type is Schaeberle's suggestion that the cause of the horizon-enlargement is the thickening of the crystalline lens through gravity, which at once accounts for the enlargement and indistinctness of horizon-bodies.[11] But as physiologists tell us that the thickening does not take place, and as the illusion for most persons persists whether the *O* be prone or upright, the Schaeberle view has had no great vogue.

Such views, however, may be regarded as curiosities or aberrations. In general, as said before, from time immemorial the illusion has forced itself on the learned and the unlearned, on the polite and vulgar alike as something noticeable, remarkable or prodigious; and one aspect of the problem is to look for factors which may account for some of the extreme variations, especially on the side of bigness.

For many effects, depending on assimilative factors peculiar to the individual or to a given situation, it is of course impossible to account: the experiments of the writer[12] in projecting after-images in the field of the closed eyes showed, along with considerable constancy of distance of projection for the same reagent, considerable variation of distances for different reagents: variations which were independent of simple physiological or psychical processes. But it would be a fruitless task to attempt the correlating of the normal projection distance for after-images of any reagent with his estimate of the size of the horizon-illusion, on account of the play of external factors in case of the latter.

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[10]B. Bourdon, *La perception visuelle de l'espace*, 1902, 344.

[11]Schaeberle, A Simple Physical Explanation of the Seeming Enlargement of Celestial Areas Near the Horizon. *Astron. Nachrichten*, 148, 1899, 373.

[12]F. Angell and W. T. Root, Size and Distance of Projection of an After-image in the Field of the Closed Eyes, this JOURNAL, 24, 1913, 262-266.

Among these factors is that commonly known as 'comparison with intervening objects;' *i.e.*, intervening between the *O* and the horizon. Sometimes this phrase means that an estimate of the size of the sun or moon is given as the result of an explicit comparison of the disk of the moon with the size of well-known objects along the horizon-level, and sometimes it means distance-effects coming from the presence of 'intervening objects.' Now the illusion appears very large—perhaps maximal—not from an explicit comparison of the moon at the horizon with known objects, but from projection on or against a known object: an act in which horizon-distances take no part. Thus the writer once mistook the disk of the moon, seen through the top of a group of lofty poplars about a quarter of a mile distant, for the disk of a windmill placed among the trees. The disk seemed about 10 feet in diameter. Again, the moon's disk caught in a pine—an intimate acquaintance of the writer—situate on a ridge about a mile from the point of observation appeared "ingens et gigas." Instances of this kind can, of course, be multiplied indefinitely: the main point at issue is that in these cases we have nothing to do with horizon-distances or the horizon; one gets like effects in projecting after-images in the open.

The problem of enlargement from the dimming or obscuring of the heavenly bodies through aerial perspective is more complicated. While attributing the enlargement, in part, to the larger horizon-distances, Helmholtz, as is well known, held that the greater part came from the dimming of bodies viewed through low-lying strata of the atmosphere: that is, the illusion in the main was like that of the apparent magnitude of mountains on the horizon, varying with atmospheric transparency.

The decisive experiment for this opinion was the projection to the horizon through a plate of clear glass of the zenithal moon, reflected from the surface of the glass. Under these conditions the projected image appeared to Helmholtz but little larger than the moon on high viewed directly. But this experiment, like that with the smoked glass, is nothing less than delicate: the glass must be motionless and without a fleck of dust or moisture, otherwise the glass and not the horizon-sky becomes the surface of reference. In addition, the projection is difficult on account of the dimness of the sky compared with the brightness of the reflected moon. Bourdon[13] and Filehne,[14] however, report that by painstaking experimentation they succeeded in projecting the image of the moon on high to the horizon, where it took on the usual enlargement.

Many facts tell against the dimming, obscuring theory as the main explanation of the horizon-illusion: the reddened rising moon seen through a smoky or dusty atmosphere by no means always seems huge, or larger than the silver moon at the same height. Robert Smith notes that the moon is not enlarged in total eclipse. Both sun and moon seen through fog usually seem relatively small. The writer once saw in a marsh fog a light disk about the diameter of a base ball floating a few feet above the ground, and reason was put to it to show that the ghostly little circle was the "glorious orb of day." Bourdon marshals a page of facts telling against the aerial perspective theory.[15]

In general the enlargement through aerial perspective takes place with dark bodies seen against a lighter background, as is the case with landscape features and "Brocken specters." But acting on bright bodies, like the rising sun or moon, the possible enlarging effects of aerial perspective are so often negatived by other factors that it can be regarded only as a subordinate, occasional influence in producing the illusion.

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[13]*Op. cit.*, 414.

[14]Filehne, Die Form d. Himmelsgewölbes, *Arch. f. d. ges. Physiol.*, 59, 1894, 292-293.

[15]*Op. cit.*, 413.

A SUPPLEMENT TO "BEHAVIORISM AND PSYCHOLOGY"

By A. A. Roback, Harvard University

Fichte once remarked that men were born philosophers or materialists,—a statement which seemed to me at the time I read it puzzling and indicative of intolerance, if not conceit. Perhaps if we apply this dichotomy to our own sphere, we should with greater justice be able to say that there are psychologists by temperament and behaviorists by temperament.

Two recent reviewers of my *Behaviorism and Psychology* show clearly how strongly the behavioristic bias operates in obstructing from view a clear-cut issue. Of these Professor Hunter has at least undertaken the task of reviewing the book in a serious vein.[1] Dr. Hunter, however, is anything but appreciative when he complains that the book of nearly 300 pages with a bibliography and chart is too sketchy; though, only a year before, he was earnestly asking "Who are the Behaviorists?" and was wishing for "a bibliography of this topic for the last decade."

Nor is he consistent to have found such "conspicuous" omissions in my chart of behaviorism as Angell, Judd, Thorndike and Woodworth, when I had listed twenty-one varieties and some forty individuals, whereas he had claimed there were two and only two behaviorists.[2] In Appendix C I have stated the grounds on which the various names have been included in the chart. Now why a functional *mentalist* like Judd should be included in the chart simply because of his doctrine of motor attitudes; or why Woodworth, a dynamic mentalist ("Psychology is the science of *mental* life"), should be treated as a behaviorist; or Thorndike, who by no means rules out introspection as a psychological method; I cannot understand. I have reason to believe also that Angell would be surprised to find himself classed among the behaviorists.

The complaint that the book is polemical I have already forestalled in the preface. Surely the behaviorists must not suppose that they have the monopoly of the art of controversy. But granted that the presentation is "too polemical," I cannot see why it is precluded from being "a good example....of dispassionate criticism." After all it is the argument, not the tone of the argument, which is to be weighed for the purpose of determining its validity. Similarly when I referred to Watson as the "enfant terrible" of behaviorism, I was not conscious of calling him "undignified names;" and he probably would prefer being designated as such to being called an introspective psychologist.

But it is Dr. Hunter's refutation of my four trends of argument against behaviorism that interests us most. Of course even if these were refuted, there would still be scores of special arguments which the reviewer has not touched, and which will be reserved probably for some graduate student to summarize. Let us see, however, how Dr. Hunter deals with these general objections.

To the first, that since there is existent such a thing as consciousness (and Dr. Hunter thinks that even Watson does not deny its existence) psychology must study it, since no other science can take it under its jurisdiction, the reviewer replies that though such study is possible it is

[1]This Journal, 34, 1923, 464.

[2]W. S. Hunter, An Open Letter to the Anti-Behaviorist, *Journal of Philosophy*, 20, 1923, 307.

not important; but since he concedes that this latter conclusion is a matter of taste, I can only repeat the trite proverb *de gustibus* etc., and address my argument again to those whose tastes run in channels similar to my own. After all a denial based on taste is no refutation.

My own conviction, and in this I shall be borne out by many moderate behaviorists, is that one of the most significant discoveries in psychology, with far-reaching ramifications into the fields of industry, artistic production, testimony, social relations, etc., is the differentiation of imaginal types. The whole field of imagery, for which there is no detectable behavioristic equivalent, is valuable enough to cause the introduction of substantial changes both in the ordering of our life and in the understanding as well as the control of other people; and yet Dr. Hunter would have us believe that mental phenomena are "of value only for aesthetic contemplation!"

The reviewer's remaining answers to my criticisms seem to be taken from my book where they were anticipated and dismissed after what I thought to be a careful analysis. To prove my point let me quote in parallel columns from both the book in question and Dr. Hunter's review:

Behaviorism and Psychology	*Dr. Hunter's Review*
"There have always been a number of objective problems under investigation in the various psychological laboratories ever since their inception; and in fact some time before Wundt established the first laboratory, Vierordt, the physiologist, was conducting and directing many experiments which did not differ in method from those advocated by red-blood behaviorists of to-day" (71).	"I now come to a second criticism stressed by Roback. The behaviorists have no experimentation to show on lines divergent from introspection (p. 71). Undoubtedly the author has in mind some other meaning than that which would ordinarily attach to such a statement, but I cannot discover it from the context."

My meaning is quite clear. If objective methods had been in vogue for more than half a century prior to the founding of behaviorism, then why the anti-mentalistic propaganda? And what revolutionary discoveries have been made by the behaviorists as a result of their *distinctive* methods which would warrant the extermination of every and any mentalistic psychology? If behaviorism is *a* method, continuative of the older objective methods, it is welcome; and, under the circumstances, we have no right to expect phenomenal contributions; but when it claims to be *the* method, and not only the *method*, but the *whole science* of psychology, we are wholly justified in asking what momentous results it has achieved *as behaviorism*, not since the days of Aristotle, but since the formulation of the behavioristic programme by Singer, Watson, Weiss *et al.*

To show that Dr. Hunter has not done full justice to my statements, I shall quote again from both the book and the review.

Behaviorism and Psychology	*Dr. Hunter's Review*
"The only behavioristic problems that can be worked out successfully in the laboratory are purely physiological investigations or statistical accounts of acts and movements. It has been quite frequently observed that animal psychologists are strikingly deficient in the interpretation and evaluation of their results. This	"All of the studies in animal psychology, infant psychology, and mental tests, and most of the studies of learning and memory, have been accomplished on lines divergent from introspection in the sense that this method has not been used. Many of these studies have been made by men with behavioristic

defect has sometimes unjustly been attributed to the insufficient mental grasp of these experimentalists, but I should think, the actual trouble lies in the speechlessness of the subjects experimented with and consequently the incommunicability of their experiences" (195).

Again: "If the experimenter is not taking account of the subject's introspection, we have a right to suspect that he is injecting his own introspection into the interpretation and analysis of the data" (72).

leanings. In many cases the authors have felt it necessary to *add subjective interpretations to their observations in order to convince themselves that they are working in psychology,* but such interpretations are irrelevant to the nature and scientific character of the experiments and as such are definitely opposed by behaviorism."

If behaviorists choose to be experimental clerks doing the more routine of recording data, well and good; but if they aspire to systematize the facts into a science, they are seriously handicapped by reason of their one-sidedness. It is just as if a person who was engaged to develop negatives in a photographic room were to conclude that all work should be done in the dark.

A third issue which I raised, and the spirit of which Dr. Hunter has fully caught, is rather shunted in the review. I have pointed out in the book that neither the concept nor the unit of behavior has ever been delimited so that we might know definitely what we are talking about. Dr. Hunter sees the point of the criticism, but thinks it does not matter. He believes that only philosophers need concern themselves about definitions.

It is for such readers as Dr. Hunter that I have included the appendix "How Is Psychology to be Defined?"—only to find that those who need that section most are the very ones to slur it. And though I expressly state "to say what psychology is about implies either tacitly or explicitly its definition; and all we contend here is that in speaking about psychology one must be able to tell what it deals with; formal definitions are not required" (255), Dr. Hunter gives the impression that I am "devoted to clear-cut definitions deductively arrived at."

It is in this part of the review that Dr. Hunter reveals the common foible of behaviorists to handle terms vaguely and then to justify such a procedure on the ground that to analyze our concepts carefully smacks of philosophy. If such were the case, then I should take philosophy to be the *sine qua non* of every intelligent exposition. Since behavior is a physically observable thing, have we not a right to ask "what constitutes the unit of behavior—the gross act, the neural process, the muscle tonus or the action of the glands?" But behaviorists are not concerned over such trifles. Dr. Hunter is even inclined to sanction Professor Max Meyer's circular definition that "psychology is the science studied by psychologists."

One reason, other than that which common sense, not to mention elementary logic, has taught me, for rejecting such a circular definition is the fact that, to my mind, some American psychologists are *not* studying psychology but a sort of *collective physiology*!

The facility with which the behaviorists are juggling their terms is apparent from the manner in which Dr. Hunter approaches the science of psychology from the avenue of human nature ("Its goal is the scientific understanding of human nature in which it emphasizes both 'consciousness and behavior'"): and yet we had all supposed that psychology was concerned with animal behavior too. Dr. Hunter's attitude amounts to this:

Why need we bother about definitions for psychology so long as we understand that it is the science which studies human nature? There may be other Irish bulls to illustrate this attitude, but the impassioned plea of the Hibernian defender of democracy who cried out "One man is as good as another and often a long sight better" occurs to me as appropriate in this connection.

The phrase "human nature" does not carry, I suppose, a mentalist connotation: yet at the same time it may, because of its vagueness, include consciousness. But a science of human nature should properly be called anthropology, and should comprise a number of facts now treated under the head of that science and others. Hume wrote a treatise on human nature, but I am not sure that Dr. Hunter would be prepared to call that masterly work a treatise on psychology; and Hume probably had sufficient grounds for choosing such a title.

What is and what is not human nature? One man eats a pound of beef, another less than half that quantity. We say it is the nature of the one to eat a great deal, and the nature of the other to eat little; but what has psychology to do with this? We often hear it said of a stout person "It is his nature to be fleshy"; yet one is not stepping on psychological territory by making such an observation. If "human nature" is a phrase resorted to only so that both consciousness and behavior might find an equal place under its nebulous roof, room will have to be made for such purely physical traits and conditions as size and shape of the skull, measurement of waist, and what not.

The reason why I am in favor of studying behavior as *a clue to the understanding of consciousness,* and object to its being treated on an equal footing with consciousness in textbooks of *psychology,* I have stated at length in my book (56-57 and 216-217); but I may add here, with special reference to Dr. Hunter's review, that since every science presupposes classification, psychology in dealing with both consciousness and behavior (general concepts which Dr. Hunter apparently admits to refer to two different classes of things) would require two separate classifications which could never be dovetailed, so that psychology would become a veritable scientific dual personality. And some of the most recent textbooks are already suffering from this anomaly.

Even in matters of art we have been enjoined for over two thousand years to observe the principle of unity. How much more then should we observe the methodological canon in science? A textbook with two or three systems of classification, just like a novel with two or three main plots, may be intensely interesting, but it does not satisfy the aesthetic requirement, just as the former violates the prime condition of scientific method.

In reply to the third criticism, *viz.*, that behaviorism refuses to employ the introspective method, my reviewer admits that this "is an important systematic question", but then proceeds to introduce so much irrelevant material in the paragraph that all I can make out of the argument is an attempt at conciliating the subjectivist and objectivist camps, provided "the former will accept a terminology which will discourage the philosopher and yet which will refer to those 'experiences' which all can observe." This terminology, as Dr. Hunter has stated earlier in his review, might be entirely colorless, such as the designation of *alpha* behavior or *beta* behavior applied to instinct. Now I suspect that there is a trace of verbalism revealed in the position of Dr. Hunter and others behavioristically inclined. Does he think the rose of instinct would smell any sweeter if called by a Greek letter? It is surely not the word or sound which counts, but the fact represented by the word. And I can well imagine that controversy would be just as rife in the case of *alpha* behavior as when the term instinct is employed. Some would deny that there is such a thing as *alpha*

behavior, others would contend that such behavior is always purposive, still others would stress the mechanism which releases such behavior, and so on and so on.

A better way, it seems to me, to deal with the situation is to retain the term with its definite connotation; and if there is any possibility of confusion by using the same word for other things, to adopt additional *appropriate* names to designate those things.

Dr. Hunter is at fault when he thinks that the different views on instincts, etc., are due to the fact that "psychologists are steeped in the philosophical history of these terms." It is not philosophy which shapes our views, but the man who makes his philosophy; and in the absence of a philosophical or historical setting, he only adopts *a cruder and more primitive attitude of his own.* That is what I meant when I submitted that behaviorism is a philosophical attitude applied to the subject-matter of psychology, and yet complained of the behaviorist's neglect of philosophical or logical orientation,—a position which Dr. Hunter took to be contradictory on my part.

Another instance of my reviewer's verbalism is his pointing out that "Watson. . . . explicitly provides for what most psychologists have termed introspection." Watson's "verbal response" is, as I understand it, no more than the action of a certain kind of musculature. Introspection according to most psychologists means, if not always the actual process of self-observation, at least the approximate representation of this mental process. The reported introspection of a pathological liar or a raving madman is no doubt *important as behavior*, but *entirely useless as a representation of the subject's mental state.* For Watson, however, the significance in the two different cases would have to be equal, for there is even no 'habit-twist', not necessarily at least, so far as the laryngeal muscles of the pathological liar are concerned.

At this point I must digress somewhat to advert to a peculiar practice of certain behaviorists, which consists in attaching special interpretations to the writings of their leaders and implying that the obvious and plain meaning of a passage is to be taken with a grain of salt. So A. I. G. in his perfunctory review of my book in the *Journal of Educational Psychology* has intimated when he speaks of my "giving a series of quotations some of which may have been unwittingly made;" and so Dr. Hunter would make it appear, from the fact that no fewer than three times in his review does he refer to Watson as a writer who requires a particular exegetical approach ("Now I gather, from what Watson . . . has written, that even he would not deny the existence of consciousness"; "I think when Watson claims that he doesn't know what the terms 'imagery', 'consciousness' and 'affection' mean, that he is influenced . . . ;" "The position can be so interpreted, but no one who knows Watson will believe that to be his intention"). Are then the behaviorists a coterie whom one must know personally in order to understand how to interpret their written statements? Are only some of their utterances intended to be taken seriously, and others meant to be discounted? Or must we hark back to the good old Middle Ages when books of great import, especially religious works, were invested not only with literal meanings, but with homiletical, metaphorical and mystic interpretations?

As for Watson, he is neither an Aristotle nor a Kant to need a commentary; nor is he so far removed from us either in time or in space that we must resort to conjecture and hypothesis as to his intention; nor have his writings been tampered with by inefficient scribes or marred by the ruthless march of time. Watson, to my mind, is certainly the most consistent and about the clearest of all behaviorists. Any reading into his views is therefore out of order.

There is yet another argument of mine which Dr. Hunter attempts to refute; and in order to show that he has missed the point of this argument also, I must again juxtapose the passages at issue for purposes of comparison:

Behaviorism and Psychology	*Dr. Hunter's Review*
"It must be remembered that the suggestion put forth here is not to the effect that our scientific terms must be brought into conformity with the modes of expression employed by the man in the street or that they must fit his categories. The astronomer will not take cognizance of the way the man in the street chooses to think about the celestial bodies. Nor does the physicist mind the curious notions and beliefs about the properties of substances current among the untutored classes. But when our whole structure of civilization rests on a foundation which is coincident with present-day psychological tradition, it behooves the innovator either to modify his stand or else indicate how this foundation can be superseded without causing the *débâcle* of our whole system of knowledge (108).	"In the first place I wish to dissent most emphatically from the assumption that any science should so regulate its conduct as to satisfy some other field. The business of the psychologists is to define their terms and to conduct their investigations in the manner best suited to solve their problems. If psychology gets the facts, other disciplines which rely upon it will be only too ready to incorporate the material. Physics does not conduct its work so that the results will be suited to engineering. Rather engineering takes whatever the physicist can offer which is relevant to its own problems. So biologists seek for the facts in their field in the best way possible, and medicine must use the material or go without."

Seemingly Dr. Hunter dissents emphatically from an assumption which I too point out as untenable. But he does not consider that my thesis was altogether different. In the third part of my book, I have tried from the collective writings of behaviorists to piece together possible applications of behavioristic concepts to ethics, jurisprudence, medicine, religion, education and the social sciences, and found myself floundering in the morass. Now I think we should all have to admit that the pragmatic test is a negative criterion of truth. In other words, if a theory or system is out of keeping with the world of reality, it is not likely to be true. Dr. Hunter's constant appeal to other sciences for precedents is not always well taken. Of course physics does not conduct its work so as to suit the requirements of engineering, but if the facts in engineering and a number of other human endeavors should come in direct conflict with the concepts of physics, I feel certain that physicists would be at pains to examine anew their presuppositions. The question before us is not whether each science has a right to work out its own salvation or not, but whether a certain conception such as the mentalistic is not *fundamental* to all the disciplines depending to any extent on psychology. And I have tried by means of the *reductio ad absurdum* method to prove that a *behavioristic conception is fatal* to all the arts and sciences which deal with human relations. In its own narrow sphere this conception may bring, as Dr. Hunter believes, reliable results, but when it is applied to other fields it simply breaks from overstretching.

Dr. Hunter forgets too that results obtained through objective methods do not necessarily establish the behavioristic point of view and certainly are not incompatible with psychology in the traditional sense; for, as already pointed out, objective methods had been in use for several decades and have contributed materially toward the understanding of mental phenomena.

Because behaviorists and their sympathizers represent me as unqualifiedly hostile to their aims, I must quote from my book the following short passages. "That behaviorism possesses a certain value goes almost without saying. *Its coming into being really affects the future of physiology rather than the progress of psychology.* Physiology, in the broadest sense of the term, will henceforth encompass not merely the workings of the individual organs but the activity of the integrated organism. As a name for this extension of physiology I should propose the term behavioristics" (200). "As a branch of physiology, behaviorism would serve a useful purpose in bringing together still more closely two allied sciences. Both psychology and physiology would gain in this way" (200). "The objectionable feature of behaviorism does not lie in its claims to a separate existence. Indeed in its properly restricted field as outlined above, it really fills a want; and even introspectionists should encourage its efforts and promote its aims as a *coördinating science*" (201).

It is as a missing link that we can prize behaviorism and confer upon it the honor it merits. Far from denying its existence or influence, I should be inclined to say that were it in danger of disappearing entirely, it would be incumbent upon us psychologists to combine with the physiologists and adopt all means to keep it alive.

But why then am I not a behaviorist? Not for the reason that Professor Leary assigns in his very appreciative review of my book, *viz.*, that an anti-behaviorist "thinks man too noble, too high, too self-conscious, too 'X' to be the mere collection of response mechanisms." I do not believe that any mentalist is biased in this direction by the *argumentum ad pietatem*, any more than the behaviorist is possessed by an inferiority complex in wishing to reduce man to a mere muscle-twitch machine. Now that we are willing to share consciousness or mind with unicellular organisms, the nobility, or X-hood supposedly attaching to it cannot be peculiar to man alone; and if there is a something "without the which we are pictures or mere beasts," it is no more the subject-matter of traditional psychology than it is of behaviorism. I decline to treat psychology in terms of stimulus and response, not only because these terms are unsatisfactory in that they easily merge into one another, but for the same reason that I should not be interested in the statement of a man who had taken a transcontinental trip that he boarded a train at such and such a station on such and such a date at such and such an hour and alighted in so and so at such and such a station at ten minutes of five in the afternoon so many days later. Not even if the train and car numbers were given should I be satisfied with such a description of the trip. In court-testimony prosaic details of this sort undoubtedly have their valued place, but for all other purposes it is the *travelling* and the *intermediate points passed* that are important.

As for the claim that psychological laws are better formulated in terms of stimulus and response and that behaviorism allows for greater success in predictability of results, I can only reply "Sufficient unto the day is the evil thereof." When this contention is demonstrated as a fact by actual comparison with the data of traditional psychology or indeed with the common fund of general observation, I shall feel called upon to investigate the matter afresh. Until that time I do not choose to be in the predicament of the learned fellows of the Royal Society who, when asked by James II why a dead fish weighs more than a live one, racked their brains to find an explanation for a phenomenon which was but a fiction. Had it occurred to them to challenge His Majesty's assumption, they would have saved much time and energy.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF NORTHWESTERN UNIVERSITY

Contributed by ROBERT H. GAULT

III. ON THE PSYCHOLOGY OF TESTIMONY[1]

By HELEN MARY CADY

The material for the following study in the psychology of testimony was obtained by staging the following event before three psychology classes (designated here A 9, A 10, A 11) in the college of Liberal Arts in December 1920, before a class in Legal Evidence (L 1 and L 2) in the Law School, and before a fourth Liberal Arts psychology class (C) in March 1921. Class A 9 had 38 members, all of whom were juniors and seniors; A 10 had 61 members, nearly all of whom were sophomores: A 11 had 76 members, nearly all of whom were sophomores. L 1 and L 2 had 49 members, all of whom had had over a year of Law School work. C had 48 members quite evenly divided among the sophomore, junior and senior classes. In all there were 272 people, who made altogether 321 reports. The incident used was as follows.

Ten minutes after the beginning of the lecture period, the instructor announced: "A government official will come here this morning to make an announcement. I advise every student to take the test that will be offered by him as preliminary to obtaining a position in government service. One who takes the test is not obliged to go further in this matter." Thereupon a person dressed in a dark blue suit, no overcoat, sack coat, white collar, gray tie, black shoes, entered immediately by way of the main entrance, facing the instructor, and offered his hand politely. He carried nothing in his right hand, an umbrella and black felt hat in his left hand; he wore glasses, had medium brown hair, was about 5 ft. 10½ in. in height, and weighed about 165 pounds. He wore his watch fastened by a gold chain in his left lapel button-hole, the watch itself falling into an upper coat pocket.

The instructor took the man's hand. The stranger said distinctly: "I will make an announcement to the class." The instructor made no reply. The visitor turned to the class and said: "I shall offer a course of lectures daily, during four weeks, at two o'clock in Room 14 of this building, beginning to-morrow, to train men and women to take important secretarial positions in various governmental commissions at home and abroad. Here are the conditions." He let down a chart showing the requirements and exposed it for 2 min. After he had removed the chart he said to the class: "Those who are interested may see me at the North Shore Hotel to-day at four o'clock." He then turned to the instructor and said: "If you will be good enough to give this test to your class, I will go on and make my announcement before another class." The instructor said: "I will." The stranger gave him the two bundles of papers and left.

The instructor then distributed one bundle among one half of the class for filling out in writing. On each paper the following was printed:

"Test A (For Applicants for Government Service)

"Write a detailed account of all that has happened since the representative of the government entered the room to-day. Include a descrip-

[1]Abstract of a thesis submitted in partial fulfillment of the requirements of Northwestern University for the degree of Master of Arts for the Academic year 1920–1921.

tion of his dress, personal appearance, the chart (reporting the words printed thereon). *No detail is too small to deserve mention.*"

The other bundle was distributed among the other half of the class. On each paper the following was printed:

"Answer each of the following questions in detail. No detail is too small to deserve mention. Quote in quotation marks any words used by either party; do not report them in the third person. If you have no clear impression of a specific point answer either 'I did not notice it' or 'I cannot recall it.' Do not answer 'I don't know' and do not state anything which you do not actually recall." There followed a list of 42 questions, some of them very fair ones, some of them misleading, all of them asking for details covering all phases of the event, including the conversation and the chart material.

In the Law School the procedure was varied by giving half of the class the questions while giving the other half the narratives, and then giving the second half the questions while the first half wrote their own accounts of the procedure.

In going over the results of the study, we find a surprisingly large number of types of answers and reports of details. In the question-answer reports we found 38 types and in the narrative reports we found 25. By 'type' is meant the kind of report, classified according to value.

There seem to be three causes for so many varieties and qualities of reports, and these are, first, the quality of the observation of the event; secondly, the strength of the memory of it; and thirdly, the kind of ability to recount the details. The influence of lack of observation shows in the question-answer reports in the rank in order of frequency of the answer "I did not notice." This answer ranks sixth, whereas there were 38 different ranks. The influence of good memory appears in the perfect answers, holding first place in both kinds of reports. The effect of a poor memory comes out in the statement "I cannot recall," and in all the types of answers not yet mentioned. The influence of native powers of expression and the method of reports appears in a comparison of reports written by foreigners with those written by Americans. It shows also in comparisons, which other investigators have made, between oral and written reports. The question-answer papers may show 38 types of answers, but only 15 are really significant. Of these 15, one is the entirely perfect answer, while 5 others have elements of perfection about them. Perfect answers are the most frequent, with absolutely wrong answers coming third, and omissions fourth. Cases of suggestibility come in the seventh, eighth and ninth places, while answers with fractions of accuracy are in other places.

In the narrative reports 25 different degrees of value were given to the reports of details. Again we find the perfect type holding the first place in regard to frequency. Generalization comes second, and complete error third. Degrees of perfection hold from the fourth through the seventh places, with confusion of time and accuracy taking the next four. A combination of accuracy and error is found in the last section of the table. In each group there were more details omitted than there were included in the reports.

The reports on the oral part of the incident were usually a little better than the reports on the general happenings or on the printed material of the chart. This would not have been the case if direct quotations had been demanded and if general reports had not been accepted. The chart material was reported with greatest error because it was the most difficult to grasp.

A comparison of the question-answer percentages and of the narrative percentages shows that in each case more errors occur when the *S*s are

forced to answer questions than when they are free to choose their own details. Facts mentioned in narrative reports are apt to be mentioned correctly either in general or in detail, while reports made in answer to a list of questions will be affected by the range of the list and by the directness of the questions.

The range of the narrative reports is much greater than the range of the question-answer reports. The narratives cover 88 details, while at most the 42 questions cover only about 50 of these same details.

Each group of *S*s omitted more details in the narrative report than they remembered to recount, one group omitting up to 74% of them. The material which is most often reported correctly is that which has been best presented, that which applies most directly to ourselves, that which falls into a general scheme of organization, and that which can be reported upon with generalizations. The matter which is reported upon with the greatest number of errors is that which deals with very particular information, and that which deals with facts which we habitually treat in some standardized manner. The material which causes the greatest amount of confusion is that which so closely resembles other material presented that it does not stand out with any individuality of its own. The most dangerously leading questions are those which suggest only the wrong answers and do not imply the correct answers.

The most satisfactory method of obtaining accurate testimony is a combination of the narrative and question-answer methods, with the narratives preceding; the second best method is the surprise method in which no warning of a future report is given; the third is that in which a warning is given; and the fourth is the question-answer method followed by the narrative.

A comparison of the work of the men with that of the women shows that there is practically no difference in quality or in range of report.

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STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF VASSAR COLLEGE

XLIX. A FURTHER STUDY OF REVIVED EMOTIONS

By MARGARET F. WASHBURN, DOROTHY DEYO, AND DORIS MARKS

In this JOURNAL 34, 1923, 99ff., there was published a report of experiments on revived emotions of joy, anger, and fear. We have now to give an account of a repetition of these experiments, on a different group of *O*s, with the emotions pride and shame added to the list, and with some differences in the numerical treatment of the results.

The mode of conducting the experiments in the present Study was like that used in the previous one. The instructions to the *O* were as follows:

"I want you to recall some occasion when you were very angry, or "felt great joy," or "felt very much frightened," or "felt very proud," or "felt very much ashamed."

"Let the emotion develop again as far as it will, so that as far as possible you relive the incident and its accompanying anger" (joy, fear, etc.). "Do not speak, but give a signal when you feel the emotion as fully as you can, in this revived form."

A stop-watch was started as soon as the *O* began to follow these directions, and stopped when the signal was given. The time was recorded. The *O* was then asked whether the revived emotion was as intense as the original one, somewhat less intense, very much less intense, or whether there was merely the recollection that the emotion had once been felt. From the reports of the *O*s five grades of intensity of revival were settled upon for use, namely: intense as original, intense (somewhat less than original), moderately intense, weak (very much less intense than original), only remembering that the emotion had once been felt.

The *O* was also asked how long ago the original incident had occurred.

Other questions, relating to the organic and kinaesthetic sensations felt in reviving the emotion, and to the nature of the original cause of the emotion, were asked, but their answers were not used in the present report.

There were 92 *O*s, young women college students.

We may first consider how far the results of this Study confirm those of the preceding Study, relating to the emotions of anger, fear, and joy. To make this comparison, our results will be treated in the same way as those of the earlier Study, and we shall leave pride and shame out of account. As before, we have to deal with the three variables of intensity of recall, speed of recall, and remoteness of the recall from the original occurrence. As before, in considering speed of recall, we have not used the actual times obtained by the stop-watches as a means of comparing one individual's speed of recall with another's, because of the obvious fact that different *O*s could not be relied on to give the signal at the same stage of development of the revived emotion: instead, we have taken under consideration whether a given emotion was recalled more quickly or more slowly than the other emotions were recalled by the same *O*. In the previous report, the remoteness of the original occurrence was dealt with in this same relative fashion: the authors say "we took account not of the absolute length of the interval, *e.g.*, two months, a week, five years, but merely of the relative length, noting which of the three emotions, in the

case of a given *O*, dated from the most remote period and which from the most recent." To compare our own results with those of the earlier paper we have followed a similar plan, although since in these experiments we are dealing with five emotions instead of three, there are five possible positions of relative remoteness in the case of each emotion.

The first point treated in the earlier paper concerns the "relative recency of the original occurrence of the emotions." Our results in the present study are as follows:

For *anger*, the original occurrence was the most remote of the five (position five) in 5 cases; next most remote (position four) in 20 cases; it occupied position 3 in 15 cases; position 2 in 17 cases; position 1(most recent) in 35.

For *fear*, the original occurrence occupied the most remote position in 30 cases; position 4 in 15 cases; position 3 in 17 cases; position 2 in 18 cases; position 1 (most recent) in 12 cases.

For *joy*, the original occurrence occupied the most remote position in 9 cases; position 4 in 13 cases; position 5 in 15 cases; position 2 in 27 cases; position 1 in 28 cases.

The results of the earlier paper showed that "fear dates from further back than either anger or joy," and that "anger tends to be of more recent date than joy." Both of these statements are confirmed by the results stated above.

Secondly, in the earlier paper the relation of the intensity with which an emotion is recalled to the remoteness of its original occurrence was considered. When our results bearing on this point are treated in the same way as those of the earlier paper, they appear as follows.

Of the cases of anger recalled from most recent time, 34% were very intense revivals; of the cases of fear recalled from most recent time, 25% were very intense revivals; of the cases of joy recalled from most recent time, 64% were very intense revivals. Under "very intense" we have included the two highest degrees of intensity: "intense as original" and "slightly less intense."

Of the cases of anger recalled from position 2, 11% were very intense revivals; of the cases of fear recalled from position 2, 16% were very intense revivals; of the cases of joy recalled from position 2, 40% were very intense revivals.

Of the cases of anger recalled from position 3, 13% were very intense revivals; of the cases of fear recalled from position 3, 11% were very intense revivals; of the cases of joy recalled from position 3, 46% were very intense revivals.

Of the cases of anger recalled from position 4, 15% were very intense revivals; of the cases of fear recalled from position 4, none were very intense revivals; of the cases of joy recalled from position 4, none were very intense revivals.

Of the cases of anger recalled from position 5, 20% were very intense revivals; of the cases of fear recalled from position 5, 30% were very intense revivals; of the cases of joy recalled from position 5, 22% were very intense revivals.

These figures do not confirm the conclusion that appeared from the data of the earlier paper, that "revivals are less intense the more remote the time position of the original emotion." They do, however, very emphatically confirm the conclusion of the earlier paper that "joy is more intensely revived than either fear or anger." The sum of the percentages of most intense revivals is for joy, 172; for anger, 93; for fear, 82.

Thirdly, the previous paper discussed the relation between the speed with which the revivals were made and the remoteness of the original emotion. Treating our present results by the same method used in the earlier paper, we find them as follows.

Of the cases where anger was recalled from position 1, 40% had the longest time of recall and 14% the shortest.

Of the cases where fear was recalled from position 1, 5% had the longest time of recall and 41% the shortest.

Of the cases where joy was recalled from position 1, 10% had the time of recall and 46% the shortest.

Of the cases where anger was recalled from position 2, 35% had the longest time of recall and 23% the shortest.

Of the cases where fear was recalled from position 2, 38% had the longest time of recall and 22% the shortest.

Of the cases where joy was recalled from position 2, 74% had the longest time of recall and 33% the shortest.

Of the cases where anger was recalled from position 3, 20% had the longest time of recall and 6% the shortest.

Of the cases where joy was recalled from position 3, 6% had the longest time of recall and 53% the shortest.

Of the cases where anger was recalled from position 4, 35% had the longest time of recall and 5% the shortest.

Of the cases where fear was recalled from position 4, 6% had the longest time of recall and 13% the shortest.

Of the cases where joy was recalled from position 4, 23% had the longest time of recall and 23% the shortest.

Of the cases where anger was recalled from position 5, 40% had the longest time of recall and 20% the shortest.

Of the cases where fear was recalled from position 5, 26% had the longest time of recall and 3% the shortest.

Of the cases where joy was recalled from position 5, 11% had the longest time of recall and 11% the shortest.

These figures fully confirm the two chief conclusions drawn in last year's paper. (1) "There is no relation between speed of recall of an emotion and the time that has elapsed since the original occurrence of an emotion." (2) "Joy is more quickly recalled than either fear or anger. Anger is most slowly recalled." The sum of the percentages of quickest recalls is for anger, 68; for fear, 84; for joy, 166. The sum of percentages of slowest recalls is for anger, 170; for fear, 110; for joy, 57.

A fourth point considered in the earlier paper concerned the relation between the speed with which an emotion is revived and the intensity with which it is revived.

When the results for anger, fear, and joy in this year's study are treated by the same method as those of last year's, the conclusions of the two studies agree perfectly on two points. (1) "In the case of joy there is rather a marked tendency for the more intense revivals to be made quickly." Our present results show 36 cases where joy was recalled in the most intense degree, and of these 47% were the emotions most quickly recalled of the whole group of five. (2) "In the case of fear there is perhaps some tendency for the more intense revivals to take longer." This year's data justify a more positive statement: there were in all 15 cases where fear was revived in the most intense degree: of these 46% were cases where fear was more slowly revived than any other of the five emotions. None of them was a case where fear was most quickly recalled.

"In the case of anger there is no relation between the speed with which the emotion is revived and its intensity when revived." The data obtained from our experiments confirm this statement to some extent at

least: there were 19 cases where anger was recalled in an intense degree, of which 26% were cases where anger was most quickly recalled, and none a case where anger was most slowly recalled. As far as these data go they indicate, then, some tendency for intense recalls to be quick ones, in the case of anger.

So much for a comparison between the results and conclusions of last year's study and our own results. Our data this year not only include those on the additional emotions of pride and shame, but have also been treated by a method which develops them more completely.

First, with regard to the remoteness in time of the original occurrence of the emotions, we have taken account not merely of the relative remoteness, but also of the actual approximate period elapsing since the occurrence.

For *joy*, there were 4 cases where the original occurrence was within one day of the revival; 14 where it was between one day and one week from the revival; 11 between one week and one month; 26 between one month and six months; 23 between six months and one year; 3 between one and two years; 5 between two and three years; 2 between three and four years; 2 between four and five years; 1 at twelve years.

For *fear*, there were 6 cases of recall within one day; 6 from between one day and one week; 10 from one week to one month; 13 from one month to six months; 15 from six months to one year; 9 from one to two years; 10 from two to three years; 4 from three to four years; 2 from four to five years; 4 from five to six years; 3 from six to seven years; 1 from seven to eight years; 2 from eight to nine years; 4 from nine to ten years; 1 from ten to eleven years; 1 from eleven to twelve years; 1 at fifteen years.

For *anger*, there were 8 cases of recall within one day; 17 cases of recall from between one day and one week; 18 cases from one week to one month; 15 cases from one month to six months; 12 cases from six months to one year; 8 cases from one year to two years; 5 cases from two years to three years; 2 cases from three to four years; 2 cases from four to five years; 3 cases from five to six years; 1 case from ten years.

For *pride*, there were 2 cases from within one day; 8 cases from one day to one week; 13 cases from one week to one month; 18 cases from one month to six months; 18 cases from six months to one year; 16 cases from one year to two years; 6 cases from two to three years; 6 cases from three to four years; 3 cases from four to five years; 1 case from seven years, and 1 case from twelve years.

For *shame*, there were 4 cases of recall within one day; 6 cases from one day to one week; 8 cases from one week to one month; 18 cases from one to six months; 14 cases from six months to one year; 16 cases from one year to two years; 4 cases from two to three years; 3 cases from three to four years; 1 case from four to five years; 2 cases from five to six years; 5 cases from six to seven years; 2 cases from eight to nine years; 3 cases from nine to ten years; 2 cases from eleven to twelve years; 1 case from twelve to thirteen years; 1 case from thirteen to fourteen years; 1 case from fifteen years.

It must be borne in mind that none of our *O*s was over twenty-three years of age.

It will be seen (1) that *fear* and *shame* are recalled from much earlier periods than are the other emotions. No doubt, children are more subject both to fear and to embarrassment than adults, since they are living in a world still new to them. These results indicate how lasting is the memory of such disagreeable early experiences. (2) *Anger* tends to be recalled from more recent dates than the other emotions: it has the greatest number of recalls from within one day, and the maximum number of recalls is reached at one month, while those for joy and shame are reached at

six months; for pride between six months and one year; and for fear at one year. The conclusions previously stated with regard to anger, fear, and joy are thus confirmed.

Secondly, on the relation of intensity of revival and remoteness of the original occurrence, we present the following data.

The different grades of intensity of revival were scored as follows. "Intense as original emotion" counted 5 points; "very intense" or "somewhat less intense than original" counted 4 points; "moderately intense", 3 points; "weak" or "very much less intense", 2 points; "merely remembered that I had felt the emotion", 1 point.

When grades of intensity, thus reckoned, are averaged for different periods of occurrence of the original emotion, we find that for *joy*, the average intensity of revivals from within one day is 3.5; from one day to one week, 3.7; from one week to one month, 3.7; from one to six months, 2.8; from six months to one year, 2.65; from one to two years, 2; from two to three years, 2.2; from 3 to 4 years, 1.5; from four to five years, 2.5.

For *fear*, the average intensity of revivals from within one day is 2.3; from one day to one week, 2.6; from one week to one month, 2.5; from one to six months, 2.07; from six months to one year, 2.13; from one to two years, 1.89; from two to three years, 2.1; from three to four years, 2.7; from four to five years, 2.5; from five to six years, 3; from six to seven years, 3.3; for the scattered cases from still earlier periods the intensity of recall does not rise above 2, except that the one case of recall from eleven years has an intensity of 4.

For *anger*, the average intensity of revivals from within one day is 2.5; from one day to one week, 2.64; from one week to one month, 2.5; from one to six months, 2.2; from six months to one year, 2.33; from one to two years, 2.5; from two to three years, 1.8; from three to four years, 2.5; from four to five years, 1. The single case recalled from ten years has an intensity of 2.

For *pride*, the average intensity of revivals from within one day is 4; from one day to one week, 3.37; from one week to one month, 2.15; from one to six months, 2.27; from six months to one year, 2.33; from one to two years, 2.56; from two to three years, 2.83; from three to four years, 2; from four to five years, 1.6; from six to seven years, 3; one case from 12 years, 1.

For *shame*, the average intensity of revivals from within one day is 2.75; from one day to one week, 4; from one week to one month, 3.4; from one to six months, 2.4; from six months to one year, 2.6; from one to two years, 2.18; from two to three years, 2; from three to four years, 2.6; from four to five years, 1; from five to six years, 2.5; from six to seven years, 2.4; after which there is no case higher than 2.

It appears very clearly from these figures that (1) joy and pride are more intensely revived than fear or anger; (2) there is a drop in the intensity of revivals of all emotions with lapse of time since the original occurrence of the emotion, except that for the remotest cases the intensity of recall tends to go up. It will be noted that the intensity of recall from within one day tends to be less than that from between one day and one week. These tendencies may be plausibly explained. When one recalls a very recent emotion, it is very likely an incident of relatively slight importance; hence what the recall might gain in intensity from the recency it loses because of the low intensity of the original emotion. On the other hand, an incident that is recalled from many years back was probably very intense on its original occurrence, and what the intensity of recall would lose by the remoteness of the original occurrence it may to some degree regain by the original intensity of the emotion.

In the next place, we have to consider our data on the relation between speed of recall and remoteness of the original experience. In this Study as in the preceding one, we cannot rely on the absolute value of our recall times as a means of comparing different individuals, but must use the relative speeds of recalls of the different emotions by the same individual. We have reckoned speed as 1 when the emotion concerned was the most quickly revived by a given person; 2 when it was second in quickness; and so on, 5 meaning that the emotion was most slowly revived of the five by a given *O*. The average speed of recall of a given emotion was then obtained by averaging these rank numbers: the smaller the resulting average, the more quickly the emotion tended to be recalled.

When *joy* was revived from within one day, its average speed of revival was 2.7; from one day to one week, 1.6; from one week to one month, 2.6; from one to six months, 2.7; from six months to one year, 3; from one to two years, 3; from two to three years, 2.9; from three to four years, 3.5; from four to five years, 1.25.

When *fear* was revived from within one day, its average speed of recall was 1.7; from one day to one week, 2.5; from one week to one month, 3; from one month to six months, 3.1; from six months to one year, 3.4; from one to two years, 3.8; from two to three years, 3.4; from three to four years, 3.1; from four to five years, 2.5; from five to six years, 4.6; from six to seven years, 2.2; from seven to eight years, 3; from eight to nine years, 2; from nine to ten years, 4.2; one case at eleven years, 3; one case at fifteen years, 5.

When *anger* was revived from within one day, its average speed of recall was 3.3; from one day to one week, 2.8; from one week to one month, 3; from one to six months, 4.2; from six months to one year, 3.2; from one to two years, 4.1; from two to three years, 2.6; from three to four years, 5; from four to five years, 4.75; from five to six years, 4.5; one case from ten years, 5.

When *pride* was revived from within one day, its average speed of recall was 2; from one day to one week, 2.3; from one week to one month, 3.3; from one to six months, 3.2; from six months to one year, 2.9; from one year to two years, 2.7; from two to three years, 2.6; from three to four years, 3.5; from four to five years, 4; from six to seven years, 1.

When *shame* was revived from within one day, its average speed of recall was 2.5; from one day to one week, 2.7; from one week to one month, 4; from one month to six months, 2.5; from six months to one year, 2.9; from one to two years, 3.4; from two to three years, 3; from three to four years, 3.3; from four to five years, 2; from five to six years, 3.5; from six to seven years, 2.6; from eight to nine years, 3.

It appears from these results (1) that the relation between the remoteness of the original occurrence of an emotion and the speed with which it is revived is irregular, so that the time of recall does not increase as the distance of the original experience increases. (2) Anger is recalled more slowly than any other emotion. In six out of the nine 'remoteness intervals' in which all five emotions are represented, that is, up to five years, anger is the slowest of the five emotions to be revived. Fear is the slowest in two of these intervals; shame the slowest in one. Pride is the quickest in three of these intervals; joy and shame in two each; anger and fear in one each.

Finally, we have the problem of the relation between speed of revival and intensity of revival. Our data on this point have been thrown into a form which shows the average intensities (counting "intense as original" as 5 and "merely remembered that I had felt emotion" as 1, after the fashion previously explained) for each position of relative speed in recall.

In the cases where *joy* was the emotion most quickly recalled by the *O*, the average intensity was 3.13; when it was second in speed, 3.2; when third, 2.5; when fourth, 2.73; when slowest, 3.

In the cases where *fear* was the most quickly recalled, the average intensity was 2.1; when second 2.2; when third, 2.48; when fourth, 2.2; when slowest, 3.

In the cases where *anger* was the most quickly recalled, the average intensity was 2.7; when second, 2.2; when third, 2.5; when fourth, 3; when slowest, 1.8.

In the cases where *pride* was the emotion most quickly recalled, the average intensity was 2.7; when second, 2.58; when third, 2.1; when fourth, 2.31; when slowest, 2.2.

In the cases where *shame* was the emotion most quickly recalled, the average intensity was 2.49; when second, 2.6; when third, 2.5; when fourth, 2.8; when slowest, 2.2.

It thus appears that joy and pride may reach their maximum intensity quickly; each shows its highest average in the position of quickest recall, but there is not a steady diminution of intensity in the case of slower recalls. It was found in the earlier paper that "in the case of joy there is rather a marked tendency for intense revivals to be made quickly." Our present data indicate rather that quick revivals of these emotions tend to be intense.

On the other hand, revived fear reaches its maximum intensity very slowly; its average is notably highest in the case of the slowest recalls.

It is, of course, important that we should have some check upon our averages which shall show the variability of the groups. Instead of cumbering our pages with average deviations, we prefer to give a statement of the number of cases of extreme high and low intensities for each of the five emotions.

For shame, there were 8 cases where the emotion was revived in intensity equal to that of the original. For pride, there were 5 such cases-for joy, 3; for anger, 2; for fear, 1.

For fear, there were 27 cases where the *O* merely remembered having felt the emotion; for anger, 26 such cases; for pride, 19; for shame, 15; for joy, 12.

Thus we see that while shame is not so intensely recalled on the average as joy, it has a larger number of cases of very intense recall than any other emotion; and that while joy and pride are near together in their average intensities, pride has more cases of very intense recall and more cases of very weak recall than has joy.

Our study thus establishes, we believe, certain fairly important points in regard to the revival of emotions in memory. *Joy* is on the whole more intensely recalled than the other four emotions investigated. It is more quickly recalled than any emotion but pride: the fact has been repeatedly observed in our laboratory and elsewhere that pleasant experiences are more quickly recalled than unpleasant ones. Joy tends to be recalled from recent date. This may mean, in the case of any emotion, either that it is frequently experienced, or that it is soon forgotten. Our *O*s, all young, in good health and fortunate circumstances, form so favored a group that their joys must be frequent. In recall, joy reaches high intensity quickly.

Anger is recalled in moderate intensity only; it has next to the lowest number of recalls in intensity equal to the original, and next to the highest number of recalls where the emotion was not really felt at all. It tends to be recalled from recent dates. Especially noteworthy is the slowness with which it is recalled: this is decidedly greater than for any of the other emotions. Anger is frequently felt, but is not lasting in the case of our *O*s; it is reluctantly recalled.

Shame tends to be recalled from early periods; and another characteristic of this emotion is that, while the average intensity of recall is not high, it has the highest number of cases where the revived emotion is equal to the original in intensity. While we may smile at many of our former humiliations, no emotion of the group we have studied is capable of being revived with more realistic vividness than shame.

Pride tends to be recalled with considerable intensity, standing next to shame and above joy in the number of recalls with vividness equal to that of the original experience. It reaches its maximum intensity quickly, and does not tend to be recalled from very remote periods, although its dates are not on the whole so recent as those of anger and joy.

Fear is more weakly recalled than any other of the five emotions, and tends to be recalled from earlier dates. When it is intensely recalled, the process is realtively very slow. Fear occupies a unique position in this group through the fact that the situation occasioning it is almost invariably a totally past situation. The situations causing anger, pride, joy, shame, may all be still active in the individual's experience: probably none of our *Os* was still afraid of the situation which had occasioned the fear she recalled. This fact will account for the weakness with which fear is recalled and, together with the unpleasantness of fear, for the slowness with which it is revived.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER

LXV. SIZE *vs.* INTENSITY AS A DETERMINANT OF ATTENTION

By DORIS DEWEY AND KARL M. DALLENBACH

Two studies on the effect of visual size upon attention have already been published from this laboratory. The first, by Curtis and Foster[1], gave such conflicting results—of three *Os*, one showed a positive correlation of size and attention, one a negative correlation, and one no correlation at all—that it seemed advisable to repeat the work. In the repetition, which was undertaken by Bowman[2], both the apparatus and the method of procedure were changed: the apparatus was standardized so as to render the conditions of observation more uniform (*O*'s head was held in a fixed position by the use of a biting board, thus eliminating the errors of chance disposition which might be occasioned by involuntary movements of the head; the intensity of the comparison areas was controlled by shutters, eliminating the noise of the episcotisters; the intensity of the standard cross was constant throughout the experiment, so that the circumstance which caused Curtis and Foster to vary the intensity of the standard as well as that of the variable did not arise); and the method of limits (as being the method first used in connection with this series of experiments upon the conditions of attention)[3] was substituted for the method of constant stimulus-differences which Curtis and Foster employed. Under these conditions Bowman obtained in her experiment unambiguous results: every one of her *Os* showed that there was a positive correlation between size and clearness or attention.

Since different procedures were used in these two experiments it is obvious that the work should again be repeated. It must be repeated under the improved conditions of observation and with the method of constant stimulus-differences before a positive answer regarding size and attention is warranted.

Apparatus.—The apparatus used by Bowman was, with slight changes, used again in the present experiment.[4] *O* sat in a light-proof booth with his head held firmly in position by a biting board so adjusted that the fixation point, 1 m. away and 1 mm. in diam., would fall, if projected, midway between the eyes. Circular openings were used as comparison areas in place of the Greek crosses of the earlier experiments. These areas were respectively 8 cm. and 4 cm. in diam. They were exposed in a horizontal position, to the right and left of the fixation point, and so centered on this point (14 cm. and 12 cm. respectively away) that their inner edges were equally (10 cm.) distant from it.

The exposure was controlled temporally and intensively and the areas were varied spatially from the right to the left of the fixation point in the

[1] J. N. Curtis and W. S. Foster, Size vs. Intensity as a Determinant of Attention, this JOURNAL, 28, 1917, 294 ff.

[2] Alma M. Bowman, *ibid.*, 31, 1920, 87 ff.

[3] L. G. Meads, Form *vs.* Intensity as a Determinant of Attention, *ibid.*, 26, 1915, 151.

[4] K. M. Dallenbach, Some New Apparatus, *ibid.*, 34, 1923, 94 f.

manner already described[5]. The exposure time was 100.3±0.7 σ. The intensity of the standard, whether the large or small area was taken as standard, was 16% of the full intensity of the light that could, within the limits of the apparatus, fall upon it. The intensity of the variable lay within the following range: it could be changed by steps of 4% from 100% to 40%; by steps of 2% from 40% to 20%; by steps of 1% from 20% to 1 %; and by steps following the negative powers of 2 from 1% to 1/32%.

Observers.—The *Os* were Mrs. A. K. Whitchurch (*W*), scholar in psychology; Miss N. F. Gill (*G*), senior major in psychology, and the senior author of this study(*D*). All the *Os* were practised in the observation of attributive clearness.

Procedure.—Every *O*, after a preliminary series by the method of limits which was given to determine the stimuli required in the main experiments, performed by the method of constant stimulus-differences four series of experiments. In the first and second series the small area was taken as the standard and the large area, the variable, appeared to the left and to the right of it respectively; in the third and fourth series the large area was the standard, and the small area was the variable and appeared respectively to the left and to the right.

The following instructions, identical with those of the two previous experiments, were read to the *Os* at the beginning of every experimental hour: "At the signal 'Now' put your attention definitely on the fixation-point. Two circular areas of unequal size will be exposed. Judge which of them, if either, is the more clear; that is, which of them catches your attention the more." The *Os* gave their reports as "right," "left," "equal," or "doubtful." Seldom, however, were more than three or four "doubtful" reports given during an observational hour. When they occurred they were not counted as "equal," as in the work of Curtis and Foster, but were, as in Bowman's work and in accordance with present practice,[6] thrown out; and the experiment in question was later, without *O*'s knowledge, repeated.

As a rule the reports were positive and given freely and without difficulty. Nevertheless, all *Os* at times reported trouble: *G* was worried by the illusion of a decrease in the time of the exposure; *W* by the thought that her reports were spatially predisposed; and all of the *Os* were disturbed by the appearance of Charpentier's illusion[7]. This illusion became so troublesome at times that the observations had to be interrupted.

At the beginning of every experimental hour, a "warming-up" series was taken. The *Os*, however, did not know when this series was ended and the regular series begun. An average of 10 series of experiments was performed at every hour. 100 series were performed during the experiment for every *O* for all of the positions of the standard and the variable.

Results.—The results of this experiment are shown in Table I, which gives for the three *Os* the percent. of illumination necessary for the variable to be reported equal in clearness to the standard. In the third column is given the percent. of illumination necessary when the variable is large and when it appears respectively to right and left of the standard; in the sixth column is given the percent. of illumination necessary when the variable is small and when it also appears respectively to right and left of the standard. In the fourth and seventh columns are shown the degrees of precision of the figures in the previous columns; and in the fifth and eighth columns, the approximate ratios of the illumination of the variable stimulus to the standard.

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[5] *Op.cit.*

[6] S. S. George, Attitude in Relation to the Psychophysical Judgment, *ibid.*, 28, 1917, 33 ff.

[7] This illusion has previously been noted and reported with this apparatus. *Cf.* K. M. Dallenbach, Position *vs.* Intensity as a Determinant of Clearness, *ibid.*, 34, 1923, 283, footnote 8.

TABLE I

Showing for Every *O* and for Each Position and for Both Sizes of the Variable Stimulus the Percent. of Illumination Necessary for the Report of Equality in Clearness

O	Position of the Variable	Variable large Standard small			Variable small Standard large		
		% illumination (L)	Precision (h)	Ratio of variable to standard	% illumination (L)	Precision (h)	Ratio of variable to standard
	left	3.1190	.3297	3:16	17.4424	.0562	17:16
D	right	11.2692	.1119	11:16	64.0836	.0612	64:16
	Av.	5.7905			40.7630		
	left	6.4516	.3158	6:16	12.3284	.5211	12:16
G	right	6.8711	.0661	7:16	83.3290	.0804	83:16
	Av.	6.6613			47.8287		
	left	2.8786	.4140	3:16	7.3726	.1888	7:16
W	right	11.2160	.0902	11:16	88.6290	.0457	88:16
	Av.	7.0473			48.0008		

The data are, with slight individual differences, uniform in direction for all the *O*s, and show in spite of a large space error that size is a determinant of clearness. The space error is constant for all the *O*s; the preferred position is to the left. This result agrees with those of the previous investigations[8], and also corroborates Dallenbach in his study of position as a determinant of attention[9]. If the figures, however, are averaged so as to cancel the effect of position, the results show unmistakably that the small area must be more intensely illuminated than the large in order to be equally clear. When, for example, the small area is illuminated at an intensity of 16%, the large area (four times the size of the smaller) need be illuminated at an intensity of only 5.7% for D; of only 6.7% for G; and of only 7% for W. When, however, the large area is illuminated at an intensity of 16%, the small area in order to be judged of equal clearness must be illuminated at an intensity of 40.8% for D; of 47.8% for G; and of 48% for W.

Besides this method of averaging out the space error we have another and more direct means of demonstrating the effect of size. Fortunately, two of our *O*s observed also in the experiment on Position *vs.* Intensity as a Determinant of Clearness[10]. In that experiment the comparison areas were of the same size, and the exposures were varied between the four cardinal and four intermediate positions; otherwise the conditions were identical with those of the present work. The circular areas were 8 cm. in diam., the size of our large area; they were centered, as in our experiment, so that the inner edges were 10 cm. from the point of fixation; the intensity of the standard was the same, 16%; and the method and the apparatus were the same. In positions 1 and 5 of the earlier work, the comparison areas lay in a horizontal position and the variable was respectively to left and to right of the standard. The objective intensities, at these positions of the judgments of equally clear, may therefore be compared directly with the results

[8]Curtis and Foster, *op. cit.*, 295; Bowman, *op. cit.*, 89.
[9]*Op. cit.*, 284 ff.
[10]*Op. cit.*

of the present work. The only variable moments in the comparison are position and size; the differences which the comparison may bring out must, consequently, be due to the effect of one or the other, or of both.

The comparison of the results of the two *O*s in the two experiments is shown in Diagram I. The fact that this diagram shows one effect when position is acting alone, a second effect when position and size are acting in the same direction, and a third effect (which is consistent with the other two, and which might on the basis of the other two have been predicted) when position and size are acting in opposite directions, justifies us in concluding, especially as the effects are constant and in the same direction for both *O*s, that size is a determinant of attention. To be more specific: when the standard is large and the variable is to the left, *D*, for example, requires for the judgment of equal clearness an intensity of 17.4% for a variable one-quarter the size of the standard, but for a variable of the same size as the

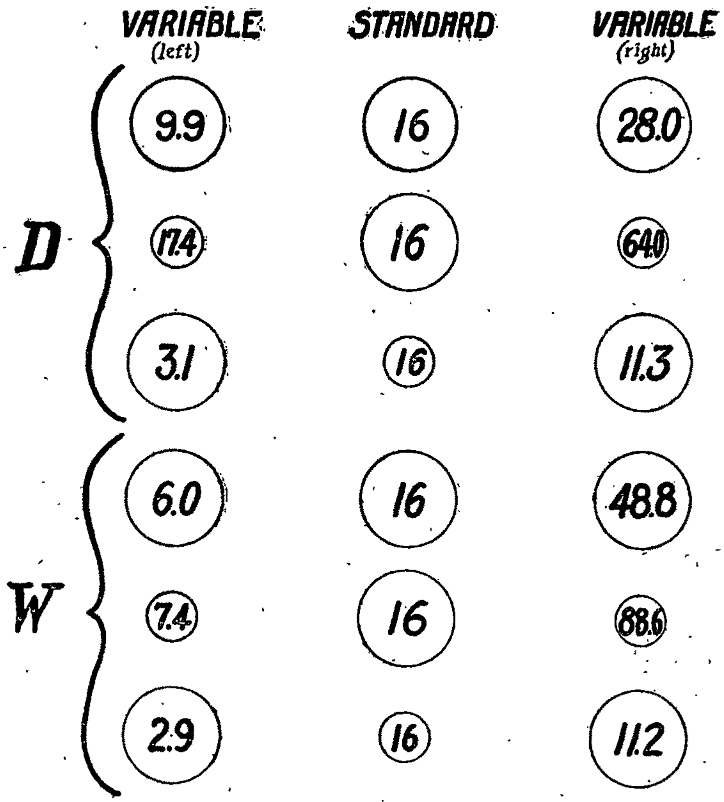

DIAGRAM I

Showing for *D* and *W* the Intensity of Illumination Necessary for the Report of Equality of Clearness when the Standard is large and small; when the Variable is larger than, smaller than or equal to the Standard; and when the Variable appears to the right or left of the Standard

standard an intensity of only 9.9%. The smaller variable lying in the same position and compared with the same standard requires nearly twice the illumination of the larger. Again, with the variables at the right: when the variable is one-quarter the size of the standard, it requires an intensity of 64%, but when it is of the same size it requires an intensity of only 28%. Similar results are obtained when the data of the experiments in which the small area was taken as the standard are compared with the data of the other experiments: size is definitely shown to be a condition of clearness. An analysis of *W*'s data gives similar results.

Summary.—The results of the different *O*s agree in the several parts of this study and show: (1) that there is a large space error; that the effect of size, as previous investigators discovered, is cut across by the effect of position; but (2) that the effect of size is definite and positive,—as is clearly demonstrated when the effect of position is eliminated by averaging or by comparing (under identical conditions) different-sized areas with the same standard.

We therefore conclude: (1) that Bowman is justified in regarding size as a condition of clearness and a determinant of attention; and (2) that the differences in the results of Bowman and Curtis and Foster are not due to a difference in the psychophysical method employed, but in all probability to differences in the rigidity of control and interpretation of the *Aufgabe.*

LXVI. The Spatial Limen for the Four Principal Film Colors

By Martha Elliott, Jean West and L. B. Hoisington

The problem of this Study is the spatial limen, in terms of visual angle, of the four principal colors. Our experiments differ from previous work on the same problem in two particulars: we added Bk and W to the standard Hering colored papers in such amounts as to reduce the experiences to the same tint and chroma, and the color experience was a film and not a surface color.

The film experience differs from the ordinary perception of a colored surface in several respects. The film color is inherently of richer chroma; it possesses a greater intrinsic brightness that often approaches luminosity; it has a kind of 'liveness' not unlike that of colored after-images; and it comes as a volumic mass of greater or less density or apparent penetrability.[1]

The experimental work divides into three parts. As a preliminary, which extended over most of the winter of 1919-20, we sought to determine the white valence, or brightness value, of the four principal colors, and did some work on the limen with unequal illumination of the color and screen and of the space between *O* and the screen. The second part continued the work on the limen with equal intense illumination of the color and screen, but with unequal lighting of the space between *O* and the color. In the third part, the intensity of illumination was equal but less intense, and the *O*s sat always within the dark room.

Preliminary. The *O*s for the preliminary work were Dr. H. G. Bishop, Dr. F. L. Dimmick and Dr. L. B. Hoisington, all members of the Department of Psychology, and all well trained in psychological observation.

The stimuli were the standard Hering colored papers, together with baryta white and velvet black. The *O*s sat about 3 m. in front of a disc

[1] *Cf.* M. F. Martin, Film, Surface and Bulky Colors, this Journal, 33, 1922, 451-80.

made up of some color on the outside and of smaller discs of Bk and W, which could be varied by known amounts, on the inside. The disc showed against a neutral grey background.

The instructions to *O* were: "I shall show you a rotating disc that is R (or whatever the color was) on the outside and grey on the inside. You will judge the brightness of the Gr in relation to the R." Thus, by the use of the method of limits, we were able to determine the brightness value of the separate colors in terms of Bk and W, and from these results to compute their brightness value in terms of white valence. From all these values we computed the composition of discs made up of the colored papers and of Bk and W that should be equal in tint and in chroma. The method used to determine the make-up of these compound discs was, in brief, as follows: let x represent the color valence or the number of degrees of color; y the white valence in terms of white equivalence; and z the chroma or the relation of color valence to white valence.[2] Then the brightness value of 1° of color as determined by observation *times* x *plus* the WV (white valence) of the Bk or W added (it will be $360° - x$ if W, and $.18° - x/20$ if Bk) $= 360y$. Since y, or the WV, is to be equated for any two colors, the left-hand members of the equations become equal, and we have to solve a simple linear equation for x. We can then get the other values by substitution. If we equate R to G and B to Y in this way, we may then equate the results of these two equations by taking the compound disc thus obtained as made up of 360° of the desaturated color. Thus we get our final result, in which all discs show the same WV and the same chroma or saturation. The final values obtained were: $x = 202.47$, $y = 162.13$, and $z = 1.248$. The WV of the colors used was for R, .2133; for G, .4744; for B, .2083; for Y, .7622; for Bk, .05[3] and for W, 1.00; the chroma as defined above was for R, 4.71; for G, 2.106; for B, 4.8 and for Y, 1.31. In the original observations 61.92° of W+298.08° of Bk gave a grey that was equal in brightness to R; 160.81° of W+199.19° of Bk, to G; 60.04° of W+299.96° of Bk, to B; and 269.87° of W+90.13° of Bk, to Y.

We made up compound discs according to these computed values, and rotated them in pairs on color mixers set side by side in a well lighted room, till every disc appeared with every other in both space orders. The *Os* in one set of experiments compared the color experiences as to tint and in another set as to chroma. The three *Os* disagreed as to relative tint, as to relative chroma, and as to the direction of difference; but all agreed that the differences were very slight. An average of the judgments showed that the discs as made up were as nearly equal in both tint and chroma as could be obtained under the conditions.

We thus had the stimuli with which to begin the determination of the limens. The simplest method seemed to be to limit the size of the stimuli by means of a screen set up in front of the rotating disc, with a small circular opening through which the color should show; and the simplest way to vary visual angle, to allow *O* to change his distance from the stimulus.

Accordingly, a table that carried the mixer near its middle and a screen in front was placed at the end of a long hallway, which was marked off into foot units. *O* started from a position close up, where the color quality as seen through a hole 6 mm. in diam. was unmistakable, and withdrew a foot at a time till the line disappeared; or, conversely, he started from far out, where he saw no line, and moved in till he saw the correct hue. *E* kept a record of the positions at which the hue appeared, disappeared or changed.

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[2]For further discussion of these relations see Ruth L. Crane, this Journal, 28, 1917, 585 ff.

[3]We took this value from Crane, *ibid.*, 602.

The results, which in some cases were astonishing, need not concern us here, since there was one obvious source of error: the illumination of the disc and screen was not the same nor was it constant. Neither was the hallway equally lighted along its course. It may however, be noted that under these variable conditions of illumination *O* frequently reported the proper hue of the film color at distances well beyond the average range for color experience. These judgments correlated with a difference in tint between the grey of the screen and the color. Whether a difference in tint between a surface color and a surrounding grey would lead to the same enhancement of color quality we did not seek to determine.

Equal and Intense Illumination of Screen and Color. In this part of the work the brightness of screen and color was subjectively equal: there were also other changes in the set-up of the apparatus, as well as in procedure. The table that carried the color-mixer and the screen stood well back in a dark room that opened into the long hall. The screen was now a double one: the back part had, pasted to its front surface, a grey disc, 20 cm. in diam., which was of the same brightness as the colored discs, and which had at its center a hole 6 mm. in diam. for the color to show through; the front part, which slid up and down in a groove and which also carried a large disc of the same grey, Hering no. 6, served to control the time of exposure and to prevent changes in adaptation.

In order to secure equality of illumination we enclosed two 100 watt, type 2C, mazda bulbs in light-proof boxes, from the front of which extended truncated cones made of heavy cardboard. These cones, 32 cm. long, 15.5 cm. in diam. at their base and 8.2 cm. at the tip, were of such length and size that they gave equal illumination over the center of the screen and over the rotating disc respectively. Equality of the lights, however, did not guarantee subjective equality in brightness of the two surfaces when presented under our conditions; in fact the two surfaces appeared unequally bright when the lamps stood at equal distances from, and at equal angles to, the two surfaces. We were therefore obliged to make the distance of one light from its illuminated surface a variable one. We chose to keep constant the distance of light no. 2, which illuminated the screen, and to vary the other, no. 1, until the screen and the disc appeared equal in brightness. Light no. 2 stood 85 cm. from the screen and made an angle of 17° with it; and light no. 1, on the average, stood 81 cm. from the disc, at an angle of 20°. The position of the variable light differed for different distances of *O*, since the relative distance of disc and screen differed: the nearer *O* sat to the screen, the nearer must the variable light be to the disc. The amount of change, however, was small, about 1.5 cm. for a difference of 8 m. in the position of *O*. Since under these conditions a very small displacement of the light, not more than 2 mm., resulted in a difference of brightness which *O* could sense, it was necessary to attach to the light a long lever which carried a pointer over a scale that indicated the proper adjustment for every position of *O*. These settings were tested for every color, and for a disc of the no. 6 grey, at the beginning of every hour of observation. This method of determining the subjective brightness or tint of a color is highly accurate if only we can make equally accurate photometric determinations, and provided we determine the reflecting value of the stimulus used.

A subsequent test by means of a Sharp-Millar photometer proved, as expected, that the lights at their experimental positions did not give equal physical intensity of illumination. With light no. 1 set as above the brightness of our no. 6 grey was the same as the brightness of a magnesium chloride surface illuminated by 4.5 foot-candles; with light no. 2, 6 foot-candles.[4] The fact that the physical intensities were thus different, while

[4]We are indebted for these measurements to Prof. F. K. Richtmyer of the department of physics.

the greys[5] were subjectively equal, indicates that some disturbing factor had intervened. Later work showed that the film color and the film grey behind the screen have a luminous character which may add to their apparent brightness. If it were a matter of contrast, which by hypothesis we ruled out, the effect should have been in the opposite direction.

As we might have expected, the colors which showed equally bright under general daylight illumination did not do so when brought into the dark room and placed behind a screen, although they were illuminated by daylight bulbs. Since the Y disc contained in addition only Bk,[6] and since it still was of the same tint as the grey, we kept it constant and varied the amount of Bk and of W in the other discs until they matched the grey, no. 6, under our more rigorous conditions.

In order to insure equality of tint *E* placed a disc of the grey on the mixer and adjusted the variable light until *O* reported the complete disappearance of the hole. The observations were for periods of 2 sec., with 15 sec. intervals. Then *E* placed the colors, successively, on the mixer and adjusted the amounts of Bk and W, without in the meantime altering the position of the variable light, till no brightness difference appeared. After the readjustment of all the discs in this manner, *E* presented them to *O* in series along with the grey, and adjusted the light till each one in turn matched the screen. Finally *O* moved away from the screen till the colored discs when viewed through the hole gave only a colorless experience, and again *E* adjusted the light until the greys were equal. It proved that every arrangement of the colored and the grey discs gave equality of subjective brightness for the same position of the variable light when *O* sat at the same distance away.

The changes were considerable. The composition of the discs which exactly matched the grey, and hence may be said to have a WV of 162.13, was for B, 202° of B, 141° of W and 17° of Bk; for Y, 202° of Y and 158° of Bk; for R, 202° of R, 136° of W and 22° of Bk; for G, 202° of G, 81° of W and 77° of Bk. If we work our formula backwards, this result gives a WV of .1004 for B; of .762 for Y; of .1234 for R; and of .383 for G; and a chroma of 9.96 for B; of 1.312 for Y; of 8.103 for R; and of 2.611 for G. Here we assume the value 162.13 as the subjective equivalent in terms of W of the entire disc. As to the validity of this value under the changed conditions, we can say only that the discs matched a grey of this subjective brightness when determined as above. Whether the differences are wholly a function of the change in illumination, or whether they are to a greater or less degree a function of the condition under which the colors appeared, we can not say at present.

In a later series of experiments we tested the relative objective brightness of these different combinations by means of the flicker photometer,

[5]The film grey, whether the stimulus was the grey paper or whether it was one of the color combinations that looked grey owing to the distance of *O* from the stimulus, blended with the grey of the screen when the two were of the same tint. The blend was so perfect that the entire central area of the screen looked the same. This observation naturally prompts a question: does the surface of the screen 'absorb' the film of the hole, or does the reverse take place? So far as our reports permit an answer, it is the latter alternative which happens. It must be noted, however, that the grey of the screen under our weak illumination was itself filmy. This may account for the small difference in the physical brightness between screen and color as over against the relatively larger difference under intense illumination, when the screen gave a surface grey.

[6]The Bk and Y mixture when filmed never, except in two instances for M, gave rise to the quality of G. It is a well-known fact that a Y and Bk mixture on the color wheel under ordinary conditions looks green.

where the conditions of observation were as nearly as we could arrange the same as those in our regular experiments. We used the same source of illumination; but the instrument, owing to its construction, had to stand closer, and also made a different angle with the rotating drum from that which it made with the disc. Although the two *O*s differed slightly in their settings (the values for the one *O* were consistently about .5 unit above those of the other), both obtained nearly the same value for every combination. The averages of the two sets of results in terms of the scale of the instrument are for B, 12.92; for Y, 12.97; for R, 12.82; for G, 12.98. The MV in no case was more than .77, and its average was .51[7]. Since the Bk and W received the same illumination as the color combination, we should have got the same brightness value, *viz.*, 162.13, as in the previous test. It turns out, however, to be 204.96.

We continued the experimental work with the newly adjusted discs during the winter of 1920-21. But the *O*, instead of continuously moving forward or backward until a color appeared or disappeared, now sat in a chair with a head-rest attached while *E* presented the stimuli in haphazard order. When every color had served as stimulus 5 times, *O* changed his position to one .5 m. nearer, and *E* presented the colors in a different haphazard order, and so on, till *O* had taken up at least 10 positions. As B and Y were never visible as hues at the farther positions, and as R and G were always visible in their correct hues at the nearer positions, *E* showed B and Y less often when *O* sat far out, and R and G less often when he sat near in. As a check against any chance variation in illumination, *E* occasionally presented the grey disc; *O*, without knowledge of the stimulus, reported (as always) upon its tint, if a difference appeared. The exposure was for 2 sec., with an interval between exposures of about 15 sec.

The *O*s were Dr. Bishop and Dr. Hoisington and Miss Mabel F. Martin, a graduate student in the department. The instructions to *O* were: "I shall present a stimulus which may or may not show color. If you experience color report its hue; if you do not experience color report 'no color.' If at any time you notice any other aspect of the experience which is unusual I wish you would report it."

Table I gives the limen in terms of distance away (in cm.) and of visual angle for every color for each one of the *O*s computed according to Urban's formula for the method of constant stimuli. These limens are based on the results of 50 judgments for every color by every *O*.

TABLE I.

O	Color							
	Blue		Yellow		Red		Green	
	Dist	V.A.	Dist.	V.A.	Dist.	V.A.	Dist.	V.A.
Bi	549.2	3′ 45″	474.3	4′ 20″	865.0	2′ 23″	971.3	2′ 07″
H	457.0	4′ 30″	448.0	4′ 36″	1027.7	2′ 36″	879.2	2′ 23″
M	812.9	2′ 35″	693.8	3′ 12″			998.4	2′ 04″

O sat for the most part in the hallway, lighted by windows along the west side, and looked through 1, 2 or 3 doorways, depending on his distance from the stimulus, into the dark room; so that different parts of the space looked across were of unequal illumination and he sat, at different positions, within an illumination of different degrees of intensity. As it was impossible to evaluate the effect, if any, on the results due to these variable factors, and as we attributed the glowing, luminous character of the hues, particularly of the R and G, to the intensity of the illumination, we ran the third series of experiments.

[7]Diana Ginsberg and A. J. Rubin made this series of observations. Both had observed in Minor Studies.

Equal and Weak Illumination. This part of the work we did wholly within the dark room, with only such light as the screen and the disc reflected into the room. The opening in the screen was now but 4.5 mm. in diam. and the lights were 150 watt nitrogen filled mazda bulbs behind roundels of Gage's glass whose inner surface was ground. The fixed light was 98 cm. from the screen and made an angle of 28° with it; the variable light was on the average 96 cm. from the disc and at the same angle. The objective brightness of our grey, when illuminated by the two lights at their respective positions, was 1.65 and 1.47 foot-candles as measured by the Holophane Lightmeter.

The *Os*, the instruction and (except as stated above) the apparatus and procedure were the same as in the preceding part. Every *O* showed differences in color sensitivity on different days; hence we planned the work so that *O* should sit at every one of the 10 positions during every hour of observation. On some days, however, we were unable to complete this programme. The total experiment covered about 8 weeks.

Table II gives the results in the same terms as Table I.

TABLE II.

O	Color							
	Blue		Yellow		Red		Green	
	Dist.	V.A.	Dist.	V.A.	Dist.	V.A.	Dist.	V.A.
Bi	378.5	4′ 05″	388.4	3′ 59″	612.7	2′ 31″	610.6	2′ 32″
H	362.7	4′ 16″	329.1	4′ 36″	591.1	2′ 37″	563.1	2′ 45″
M	524.3	2′ 57″	526.7	2′ 58″	992.6	1′ 34″	951.7	1′ 37″

The two sets of results agree in their general aspect from *O* to *O* and with those of Table I, except that the visual angles are for the most part somewhat larger. The limen, in terms of visual angle, is largest for Y except for Bi in part 3; B comes next, and stands very near to Y; G comes next, except for Bi in part 2, but is considerably smaller than B; R comes last, and is only slightly less than G. This result means that the R and the G give rise to their proper qualities when their extent, as stimuli, is less than that of B or Y; and that the liminal extent for R and for G is about the same, just as it is for B and for Y.

The results of M differ so widely from those of Bi and H that they require some explanation. Bi and H did not 'strain' for the color; they assumed a passive or receptive attitude; M, on the other hand, 'strained' for the color, and reported every hint of color experience that came. She often reported colors other than those of the stimuli, and frequently a series or flight of colors. Her results do not show a consistent increase of correct reports with shift of position. In part 2 she gave 65 and 90% correct reports for Y and B respectively at 6, 6.5 and 7 m. from the stimulus; at other positions there were even inversions of the first order. Bi and H not only gave no inversions of the first order, but their results also show a fairly uniform increase of correct reports with decrease in distance from the stimulus. M complained that the 2 sec. exposure was not long enough; it did not give her time to see the color. Both Bi and H reported that they could at times 'guess' the color of the stimulus, although they had not experienced it; hints that under the color attitude passed unnoticed were the cue to the guesses; thus, the colors at the moment of exposure often showed a flash of brightness difference. In part 2, M correctly reported R at every position up to 10 m. except at 9 m.; hence no limen could be determined.

The hues, especially R and G, at the extreme distances at which they were experienced were indefinite nebulous masses, not unlike a small bit

of faintly colored glass, 4 or 5 times the size[8] of the hole in the screen and indefinitely localised in the region of the screen. As *O* came nearer, the color appeared highly luminous, almost iridescent, and somewhat reduced in extent (although still more extended than the physical stimulus), and localised more definitely about the screen. The chroma was extremely high, much better than when the entire disc was the object of observation. The B and the Y never showed this high chroma for Bi and H, although they did so, very rarely, for M; for Bi and H they were relatively dull and unsaturated colors.

There was some hint of an after-effect, but it was not uniform. At times, when the same color came twice in succession (as may happen in a haphazard series), the effect was an enhancement of the color; at other times, when the succeeding color was the complement of the first, the effect was of the nature of a negative after-image. The ground for this statement is that, under the conditions mentioned, once in a while *O* experienced colors with considerable distinctness, whereas in general at the distances in question he saw them faintly or not at all.

The colors differed in their stability. *O* seldom if ever misnamed R when he reported a definite experience of color; all *O*s reported B when the stimulus was G; H and M on several occasions with this stimulus reported a bluish green; H and M also reported R when the stimulus was Y, but they more often reported orange or brown; in a small number of cases M reported G for Y.

Conclusions. We conclude that, when film colors match in tint and chroma and appear behind a grey screen of the same tint, the limen, in terms of visual angle, is about the same for R and G and for B and Y, the latter being nearly twice the former.

The film colors, especially R and G, show a greatly increased chroma over the surface color when *O* sits just within the liminal distance for the film color.

In the region of the limen the colors appear greater in extent than the stimulus as viewed close up, and are indefinitely localised. The procedure of the Study furnishes means to a critical observation of colors which, beside being equated, stand well-nigh stripped of perceptive motives.

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[8] It will be noted that in terms of their intrinsic extent the color qualities show the same paired relationship as they do in their paired antagonism. The intrinsic extent of R and G and of B and Y turns out to be about the same. The B and Y have an intrinsic extent somewhat larger than the size of the hole in our screen. The reduction from 6 mm. to 4.5 mm. in the diameter of the opening did not affect the volumic extent of the colors. Although with our stimuli film colors are in general of better chroma than are surface colors, the enhancement of the chroma of R and G is much greater than it is for B and Y. The luminous character of the colors therefore shows the same tendency for R and G and for B and Y to form pairs.

BOOK REVIEWS

Life and Confessions of a Psychologist, by G. STANLEY HALL. New York, D. Appleton & Co., 1923. 623 pp.

That G. Stanley Hall, the Nestor of American psychologists and educators, belongs to that small group of supersenescents he so ably and thoroughly analyzed and described in his *Senescence*, no one who has noted the amount and quality of his productivity in recent years will doubt. Since his retirement from the presidency of Clark University in 1921, he has been going at a pace few men a quarter or half century his junior can hope to attain, publishing numerous articles and reviews in scientific and popular journals in addition to the above-mentioned volume on senescence of which the present volume is the natural sequence and completion. And now he is breaking all speed laws to complete the publication of the enormous mass of varied but interrelated materials he has been collecting, digesting, and creatively developing, or reinterpreting in keeping with his genetic standpoint, during the past thirty-five or forty years. The stock-taking begun in *Senescence* is carried back not only to the beginning of his own life but to that of his parents and forbears as far as the 14th century. Thus, he finds that he "came of sturdy, old, New England, Puritan stock that had been long enough in this country to be more or less acclimated, that in the moral atmosphere of my home I heard more of duty than of pleasure, that religious influence and tradition were very strong, that generations of toil and life close to nature had toned down the spirit of enterprise and adventure that must have animated my pioneer ancestors in their migrations from the old world to the new, and that after long dormancy the same spirit had shown some degree of resurgence in the excelsior impulsions of both my parents." To his parents he owes his "rather exceptional physical stamina which they transmitted from many generations, for every ancestor I can trace worked with his hands and lived an active and mostly out-of-door life. Most were farmers and the rest followed the sea or were carpenters, and nearly all were pioneers and spent their lives in the country. None were rich, none were traders or profiteers, while none were paupers. Most avoided towns or even villages and led rural lives close to nature. They were frugal, thrifty, economical, and devoid of great ambitions. They were home-staying, content with simple ways, and virtuous, whether with or without piety. I cannot find that any ancestor or blood relative ever committed a crime or was insane or mentally abnormal, and very few were tubercular. Not a few lived to a great age, and despite all the hardships that cut many of them off prematurely they often had large families. So far as I can learn few of their offspring died in infancy, while most married neighbors' daughters, so that there was almost no infusion of other than English blood save occasionally Scotch and Irish.

"As I review their lives I realize that I have no mental aptitudes or moral traits of character that I did not inherit from my parents. . . . I inherit, too, my mother's even sunny temper, her exceptional dread of disharmony or even disagreement, and her exquisite sensitiveness, which makes it hard for me to disagree with any one, especially to his face. Thus in disposition I am an extreme if not almost pathetic pacifist. With this, too, goes, I think, my very strong and deep impulse to get into sympathetic

rapport with the most diverse types of personality and opinion. I want on every subject, first of all, to take in all others' views unchanged and know not only what others think but how they feel, however *outré* their standpoint. On the other hand, if this pristine passion for amicable relations is broken through, I often tend to be not only critical but severe and sometimes to give way to temper, which is my father's diathesis voiced again in my soul. Thus in these and in many other ways I love to think of myself as only the prolongation of my parents' lives and of making patent what was latent in them."

There were two impelling motives for the writing of the book; first, that he might get a better knowledge of himself through the process of psychoanalysis and frank self-revelation, and secondly, to satisfy at last "the long-repressed impulse to tell the inside story of the early days of Clark University and to correct, so far as I could before I die, the long injustice done me by good men who did not and could not at the time know the facts regarding the relations of Mr. Clark to the institution he founded, which made the story of its first decade so unprecedentedly tragic." This story is told in Chapter VII, which, though fragmentary, owing to the fact that Dr. Amy E. Tanner, one-time assistant to President Hall, has written a detailed history of the University based on a collection of documents and records including a hundred-page statement of his own, which may some day be published, will be read with keen interest by former members of the faculty, students, psychologists and educators here and abroad. It is indeed a tragic story, unequalled in the annals of institutions of higher learning, of almost ecstatic hope and enthusiasm, of loftiest academic ideals and most elaborate and comprehensive plans based on an eight months' careful study of practically all the higher institutions of learning in Holland, Belgium, Denmark, Sweden, Norway, Russia, Austria, Germany, Italy, Hungary, France, England, Ireland and Scotland, and conferences with the leading scientists and educational authorities in those countries,—all cruelly dashed to the ground by the aged founder, who was either piqued in some unknown way, or had experienced a change of heart, or suffered financial losses: at any rate, whatever the reason (Mr. Clark never confided in President Hall or any of the men he invited to be trustees of the institution, despite the repeated efforts they made to get from him a statement of his intentions), five years after founding a university that was to be unique in this country, a centre of research and experimentation, a perfect realization of Bacon's dream of a House of Solomon, and a model to all other graduate universities, Mr. Clark abandoned it to its pitifully small resources, $24,000 a year *plus* the income from $100,000 for the use of the library. Even after the first year he cut his annual supplementary contribution from $50,000 to $26,000, the next year to $12,000, and thereafter to nothing at all. Meanwhile, President Hall had gathered around him a most notable group of American scholars, who like himself were lured by the wonderful prospects of the new institution; and when, after the second year, he failed to fulfill the promises he had made or implied, they began to lose faith in him and grew discontented. Nor could he reveal to them the situation as it actually was, for fear of completely alienating Mr. Clark and losing the chance of ever receiving the remainder of his fortune. This effort to shield the founder "was most humiliating to my honor and even to my conscience but the situation demanded nothing less, for the entire future of the institution seemed to hang upon this. Accordingly, I became a kind of Winkelried 'gathering a sheaf of Austrian spears' into his own breast." The third year the great crash came, when a majority of the faculty resigned because they "had lost confidence" in the President; and President Harper of the University of Chicago hearing of this quickly appeared on the scene and secretly engaged them all at practically double their salaries, and then even invited President Hall to "join the hegira" at

a larger salary than he was receiving. "No words can ever begin to describe my own anxiety during the latter months of the third year. Almost every day there were new rumors, some of them wild and senseless, others only too well-founded. I moved my bed and slept with my face against the window opposite the University, to be readily awakened in case of any accidental conflagration, and when it was all over, although we worked from morning to night upon the wreckage of fond anticipations, there was a certain satisfaction that nothing worse had befallen us and that we had really touched bottom."

But if blasted hopes and broken and unfulfilled promises constituted one element of the tragedy, the other element was furnished in full measure by the heroism, the sublime faith, and fine loyalty to their chief on the part of the small band who resisted all allurements and temptations and determined to carry on despite the numerous handicaps and discouragements. And the record of achievement of this little group and the students they attracted during the next three decades constitutes a chapter in the history of higher education of which any institution in the land might well be proud.

The remainder of the book is taken up with accounts of the author's childhood and boy-life, which show him to have been "a rather unusually active youngster in mind and body" who grew up in a rural community "amidst straitened circumstances, simple, and among homely but genuine people"; of his secondary and higher education in this country; of his three years' study in Germany, made possible by Henry Ward Beecher through Henry Sage; his professorship at Antioch College, tutoring at Harvard, return to Germany for another three years, his studies with Wundt, Ludwig, Helmholtz, Kronecker and others; his Boston lectures to teachers suggested and arranged for by President Eliot; his call to Johns Hopkins University after Charles Pierce, George Morris, and William James had been tried out for the position; his life and work there during the next seven years; and the books and monographs published by himself and his students.

Chapters VIII and IX contain a masterful, critical survey of the progress in psychology and educational changes in his day, which will be of special interest to psychologists and educators, particularly his criticisms of the introspectionists, behaviorists, and anti-psychoanalyists, all of whom are in the pre-evolutionary stage and fail to appreciate that the soul is as much a product of evolution as the body and that there are group and social souls every bit as real as individual souls. Their chief faults are that they study mere "cross-sections of the adult mind", are "hyper-methodic", and the foundation upon which they build is physics instead of biology. Also his criticism of the mental testers who "have already shed much light upon individual differences and vocational fitness but have added little to our knowledge of psychology itself", and his own set of eight tests which "seek to evaluate far more fundamental traits than mere information or mental alertness." These are health, ability to draw upon "second breath", free mobilization up and down the pleasure-pain scale, capacity to sympathize aright, love of nature, capacity to sublimate instincts and emotions, activity *versus* passivity, and loyalty or fidelity. Psychologists have wasted an enormous amount of time and energy on five problems that are either insoluble or cases of *ignoratio elenchi*, namely, the psychophysic law, the Lange-James theory, parallelism and interactionism, the structural and functional points of view, and introspectionism *versus* behaviorism.

The concluding chapter is entited "Looking Backward and Forward." This is the most confessional and revealing chapter of them all, particularly the last few pages, which are worth quoting quite *in extenso*. "All things considered, it must, I think, be admitted that much if not most of my work, such as it is, has been done under a heavy handicap, so that I have not been entirely able to discard all traces of an apologetic attitude and I feel that, on the whole, I have not had quite a fair, impartial, fighting chance.

Part of this is doubtless due to my own inherent imperfections. Perhaps the vicissitudes of my life from boyhood up have tended to make me unduly self-conscious; perhaps I have been too tactless or lacking in some elements of both *Gemüt* and *esprit*; perhaps I have been too narrowly focussed upon my themes and my students. I certainly never have formed the habit, as James and Mabie did, of wide correspondence with sympathetic minds at a distance and have never yielded to the impulse I have often felt toward authors that have helped me most, of writing and telling them so, or to enter into epistolary relations with those to whom I feel intellectually most akin, much as this would have added to my joy of life and given me the wider personal rapports I should have so profited by. I think I do not lack the moods of exuberant effervescence and abandon so delightfully shown in many of James' letters, although this in me has its outcrop only in social and personal relations with my closest intimates. At least I have felt intensely that I was never able to quite do justice to the spirit of *cameraderie* and good fellowship that I really feel....Perhaps the most basal trait of my own diathesis is a dread of every form of conflict or disharmony. Any marked disagreement with those nearest me is often almost paralyzing. If I am not assured of sympathy in my social environment it is hard for me to speak or to assert myself, and there is also a crust of diffidence that has to be broken before I can come out of my shell. Thus there is an inveterate dislike of the open, a dread of being conspicuous, a love of obscurity, of the simple private life, of homely commonplace people, that has kept me in touch with the rural friends of my boyhood. It is this that has impelled me all these years to revert so many summers, if I could, to the old life of the farm, to revive and cherish its associations and memories, and thus to find peace, rest, and renewal. If my career had been wrecked at any point—and this is increasingly true of my later decades—I believe I could have found unwonted compensations in leading the plainest sort of rustic life and associating with the plainest people. This to me would have been the apogee of psychic flaccidity, which psychoanalysis would correlate with the almost masochistic subjection of my mother (as shown in the above diary) to the will of God.

"But, on the other hand, in conflict with this quietistic trend has always been a very virile *Aggressionstrieb*, first against early surroundings—physical, social, intellectual, religious—and then against educational, psychological, and other prevalent culture currents, so that many of my own scientific interests have grown somewhat apart from those most cultivated in the American Psychological Association; and this prompted a protestant and somewhat polemic attitude on my part. Thus despite the initial dread not only of every degree of hostility but even of the publicity in which everything of this sort is so prone to result, it always brings a certain exhilaration that relieves life for the time being of tameness and adds a gamey flavor to it. It is in this 'I-can' mood that I have overestimated my powers and drawn too heavily upon the future by beginning many more things than I can ever hope to finish, which will certainly leave me in at least partial bankruptcy in the end. The preposterous childish reveries of being a great musician, orator, etc., in which I still sometimes catch myself in vacant hours, are unreduced vestiges or rudiments of this *élan* and would very likely be the inception point of megalomania or delusions of greatness if I ever fell a victim to this; while the dread of discord would make a rich soil for delusions of persecution......So far failures and disappointments seem to have played a somewhat preponderating rôle. My boyish dreameries of fame, as above noted, were early aborted but left little pain because they were superseded by others. The long and trying period of waiting and hoping in vain for a fit teaching position, with the atmosphere of despondency with which it enveloped me for years, left, I think, some predisposition to somber views. Then came the wreckage of

early expectations for Clark University and hardest of all to bear the widespread impression, by a very few individuals assiduously cultivated, that I had been at first fatuously and uncritically extravagant in my hopes for the institution and later somewhat false to those I had attracted here. The silence that circumstances forced upon me was taken to mean that I had no excuse or justification. As a result I constantly met misrepresentations and disparagements from this source of which three decades have not entirely made an end.

"Again, I was sanguine enough to work for years, as above described, in the hope of establishing an institution that should be devoted to the study and welfare of children and in 1910 felt justified in not only making definite plans but in beginning the development of such an institution, only to see all these efforts come to naught. When I turned to psychoanalysis it was with the ardent hope and belief that we had in it the promise and potency of a new dispensation in normal psychology, and for the more than a decade that I wrought and taught it here I was, so far as these hopes were concerned, little more than a *vox clamantis in deserto*, for up to the present it has found but little academic recognition among psychologists, although I still believe it contains a leaven that will sooner or later pervade the entire lump. Yet again I realize that I have begun and spent much time and effort in domains in which if I do no more (at least unless I should achieve the forlorn hope of finding a fit literary executor) a great body of lecture notes, products of years of diligence, will have to be burned ultimately, so that if I do not sugar off my data as best I can in the form of one or more volumes upon the feelings, and early childhood, at the very least, to say nothing of another on psychogenesis with special reference to the animal soul, psychoanalysis, and the psychological aspects of nutrition, my life will end without a well-rounded period and, as it were, in the middle of its sentence. Of course I realize that here I may be and probably am living in a fool's paradise because from the reception of most of my so far published volumes I have no reason to expect that those now unfinished would do any great good in the world, so that my desire to round them out is very likely in large part a form of self-indulgence, the negation of which will be no loss to the world but only a check to my own egoism. This ambition very likely has come to vicariate, by processes now well known, for my earlier belief in a personal immortality of the soul, which but for my early orthodoxy would have been less. Thus this latter made stronger and more extravagant its present surrogate.

"In view of all these failures, however, I am surprised to find that I am not, after all, overwhelmingly pessimistic but, on the whole, inclined to find hope at the bottom of the Pandora casket. I certainly have been manifoldly blessed in my friends, some of whom have stood by me in every vicissitude and of whom so many have lately expressed their appreciation of what I have succeeded in doing. I surely have helped my students far more than I have the readers of my books. The publication of most of these books has given me, however, great inner satisfaction and all of them have found appreciative readers and every one of them bitter critics. It is certainly one of the great satisfactions of life to be able to register in published form one's own most mature views upon any subject, although the sense of having come to terms with that subject and of having done one's best for it is never unalloyed by the deeper and later realization that far better things might have been and are sure to be said upon it. Well-matured books or memoirs do organize our very neurons and make mentation more unitary and more economic. I have never understood how anyone in a field so rapidly growing and changing as psychology could ever use his own textbook year after year, and the fact that I have never been willing to make such use of any of my own volumes but try to forget them and turn to other things as soon as they are done has probably saved me

from the mortification of a progressive realization of their shortcomings and allowed the secret and comforting hope to linger that if they lack appreciation now they may find it later...... I have often wished I were a better mixer with my equals, that I enjoyed more the social functions so prominent in all our academic meetings and the convivial hours and banquets of learned societies, that I had more intimate personal friends along the lines of my chief interests, that I was a better letter-writer instead of abridging and being generally rather averse to this function, and that I could respond more heartily, when I really have the impulse to do so, to the advances of those who have been my most intimate and cherished friends instead of giving the impression, as I often do to my great regret, of being somewhat unresponsive sentimentally. I believe my whole affective life is as strong and deep, and perhaps more so, than that of most I know, but I have never been able to entirely escape the early atmosphere of repression of sentiment. Only my students know that I am at heart truly the good fellow that I really want to be with both my equals and my superiors. With both these, especially the latter, I have never been able to entirely overcome a certain feeling of inferiority, which began to be somewhat acute at the Easthampton period and to compensate for which has sometimes been a spur to do my best things and even to criticize too severely those to whom I should have looked up as authorities...The dominantly sad note of my life may be designated by one word, isolation. A country farm far from the village; ambition shared by no boys of my age; misunderstood by my father; the fitting school with classmates too advanced and mature for companionship; college, with only a few choice intimates and congenials; the seminary, where I was suspected of heresy, which thus hindered associations or even broke those I had come to prize, as had also happened in my later college course; the years in Europe, where my only friends were foreigners speaking an alien tongue and with no one to advise or counsel; my interest in studies slowly shaping along lines which very few in this country cared for; nearly a score of years after college graduation before permanent and final settlement in the kind of academic chair I wanted; the tragic death of my first wife and six-year-old daughter just after reaching Worcester; the ten years of living alone that followed; the débâcle of my great hopes and plans for Clark University during its third year; the long period of misunderstandings that followed; the uniqueness of our plan which set us more or less apart; some *odium sexicum*, which began with the publication of my *Adolescence* and was intensified by my introduction of Freudianism into this country and by my teaching some of its essentials, although with great reservations (a topic still practically taboo by the American Psychological Association, which was organized in my house and of which I was the first president); some acute experiences with the *odium theologicum* which followed the publication of my *Jesus, the Christ, in the Light of Psychology;* my genetic conception of the human soul as a product of evolution like the body; the crust of diffidence that always had to be broken through at every public appearance; the incessant danger that if this is broken I be negative and give offense by somewhat excessive antagonisms; the disappointments about the Children's Institute—all these handicaps gave me a deep feeling that I had never quite done justice to myself. This, too, may account in some degree for my yet very strong youthful ambition, despite the handicap of age, to finish before I die the work along not one but several of the above lines I have already given so much time and labor to, and thus round out my life by doing a few things which shall be better than I have ever yet been able to do. I never so ardently longed to accomplish something really worth while in the world as now and never saw so clearly just what I want to do or felt so strongly that I can do it if I only have the time and strength. Yet all the time I know that the Supreme Queller may at any time now intervene and cut down all these aspirations

as the mower does the last lush crop of rowan before the snow and ice of winter kill it at its very roots. Thus lust for mental increases after that of physical fecundity fails, a phenomenon characteristic of my stage of life and which I have tried in my *Senescence* to come to terms with and understand, knowing all the while that this feeling of a new inspiration is somewhat falsetto and probably only a symptom. All the while I welcome, cherish it, and will not put it by.

"On the other hand, despite all the above, I must count my life as, on the whole, a happy and fortunate one, and that chiefly because I have been very exceptionally able to follow my own inner interests and inclinations. I love my work and have always been able to find solace in it, not only for all the disappointments that I have met but for all the severer blows of fate. This has been almost literally a life preserver. I wonder if any other line of study could have done this as well as psychology, which is in its larger aspects only the study of human nature. Certainly not for me, with my diathesis and life history. From this point of view I am far older than my years for I have laid aside more of the illusions and transcended more of the limitations with which I started than most. In the views I have attained of man, his place in nature, his origin and destiny, I believe I have become a riper product of the present stage of civilization than most of my contemporaries, have outgrown more superstitions, attained clearer insights, and have a deeper sense of peace with myself. I love but perhaps still more pity mankind, groping and stumbling, often slipping backward along the upward path, which I believe I see just as clearly as Jesus or Buddha did, the two greatest souls that ever walked this earth and whom I greatly revere. If my intellectual interests have been in the past and present, my heart lives in the future and in this sense I am younger than youth itself, the nature of which I would chiefly understand and appeal to. Thus I find even a kind of second childhood in age more charming than the first ever began to be. Hence I believe I have achieved another new birth superimposed on that of adolescence."

An appendix contains an eighteen-page chronological bibliography of the author's books, journals, addresses, articles, and memoirs. Some idea of the prodigious amount of labor expended by this modern Aristotle may be gained from the fact that his addresses, articles and memoirs alone have been bound in twenty-three large volumes. That the "Supreme Queller" may long overlook the subject of this review, thus enabling him to complete the several volumes that are still in his system and which when published will constitute a most notable contribution to the psychology and pedagogy of our age, will be the hope of all his friends and former students and friends of learning everywhere.

The book will appeal to many classes of readers, particularly to psychologists, educators, and all those who have read the *Senescence*. To the student of the history of psychology, child-study, and education during the last half century it is indispensable.

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Lehrbuch der experimentellen Psychologie. By J. FRÖBES, S. J. Herder & Co., Freiburg. Bd. I, zweite u. dritte umgearbeitete Auflage, 1923. 630 pp.

The first edition of this book, which appeared in 1917, has been out of print for two years. The delay of the new edition was occasioned in greater part by the labor of reading and incorporating not only the new literature published before the year 1921, but also the older, the American in particular, which on account of the war was not available when the book was first written.

The author estimates the new material in the volume as more than 150 pages, but the size of the book has not been materially increased. Much of the old subject-matter had, therefore, to be either eliminated or condensed. The task of revision was not easy; but it was faced with courage and, for the most part, has been performed with discretion. Apparently the only mechanical device which Fröbes found he could employ was the deletion of the final phrase or sentence of a paragraph; this, at any rate, has been done on almost every page. In other instances sentences have been dropped from the middle of a paragraph, and in still others an entire paragraph has been given up or recast. Not all of the re-writing has, however, been solely in the interest of condensation; in many instances there is gain in the logical arrangement and exposition of the facts. In this reconstruction the general aim and spirit of the book are unchanged; the volume is still a compendium of psychological facts; and there is no more interest than before in systematic problems. The task of the reviewer is, therefore, principally to take account of the additions and subtractions.

The only accessions to the Introduction are a couple of citations from Baade, one from Köhler, two from Titchener (one of which is a mention of the stimulus error), and a brief reference to Watson's Behavior Psychology. In the first division (Sensation in General) the paragraph of fine print on Geyser's account of the structure of consciousness is dropped; sub-sections 3 and 4 are combined in a new 3; the large print is revised; and a new part (*c*), on the antithesis of act and content, is added. In section 3 (Sensation Psychologically Regarded) the error of the first edition concerning Titchener's attribute of clearness is avoided (see this JOURNAL, 32, 1921, 148).

In the chapter on Visual Sensation the principal additions are frequent digests of passages in Ostwald's *Farbenfibel* and *Beiträge*; an abstract of Lempicka's monograph on the spatial fusion of color-strips, and a discussion of the recent developments of Müller's color theory based on a paper read by Müller at the Marburg Congress. In the history of the development of the laws of color-mixture there is no reference to Maxwell; and in the section on peripheral vision (sec. 6) there is no mention of Baird, Abney, or Ferree and Rand. Sections 9 and 10 have been rearranged; the temporal development of visual sensation during the period of excitation, including *Ansteigen*, adaptation, and the results of periodic stimulation (Talbot's law), now precedes, as it logically should, the after-effects of stimulation (positive and negative after-image); much of the content of both these sections is new.

The chief addition to the chapter on Auditory Sensation is a report of Stumpf's paper on the structure of vowels; there is no part, however, except the section on the organ of hearing and the sub-section on beats, that has not been enriched by additional facts. In Smell and Taste not only are Henning's geometrical constructions given, but his results are also worked into every relevant paragraph; the discussion of adaptation and mixture of odors is practically new. There is little that is outstanding in Cutaneous Sensation; Tung and Malmud on cutaneous fusions, Rubin on paradoxical warmth, Gertz on adaptation of cold and warmth, Murray on tickle, and Titchener, Thunberg, and Becker on pain are the principal accessions. Woodworth, Bourdon, Katz, and Benussi furnish the new facts for Kinaesthesis; and Griffith and Gemelli for the Static Sense. The discussion of the labyrinth as sense-organ is entirely recast. There are few additions to Organic Sensations; Boring's thesis is not mentioned. The chapter on Feeling has been revised throughout, and is now much more specific in the sense that more authorities are cited for particular statements. The recent results of Bickel, Küppers, and Cellerier on the physiological concomitants of feeling are incorporated in the text. Wohlgemuth is cited as for, and Young as against, the possibility of mixture and of the localization of feeling. The appendix at the end of this division, on the

elementary sensory process in Desire (*Begehren*), has been reduced in length from two pages to a few lines; part of the old content will appear in the second volume under Will and Movement.

In the chapter on Ideas the author has added a general summary of the differences between sensation and simple idea. The results of Henning on smell and taste imagery, of Sullivan on kinaesthetic imagery, and of Braddock on cutaneous imagery are incorporated in a new paragraph entitled "Different Kinds of Ideas", and there is a new section of 3 pages devoted to Jaensch's eidetic images. There is also a new section on Ideational Types, part of the subject-matter of which has been transferred from one of the chapters on learning. The novel material in this section is taken from Baerwald's book; there is no mention of the American literature on the determination of types. The sub-section on the Localization of Images is also extended into a section; the article is based principally on Müller, but with citations from Martin, Stumpf, and Segal. The introduction to Hallucination, and the concluding paragraph on the nature of hallucination, did not appear in the first edition.

Aside from condensations, a couple of references to Pratt, and a single reference to Watt, the section on Auditory Perception (*gleichzeitige Tonverbindung*) stands without change. Visual Space Perception, is however, a different story. The subject-matter of *Augenmass* is largely rearranged and rewritten. In the section on the law of identical direction a new sub-section on the irregularities of absolute and relative localization resulting from certain visual disorders has been substituted for "differences in monocular localization." Jaensch's 'color transformation' is described in a 3-page addition to Katz' phenomenology of color, in a section entitled "the influence of localization upon the phenomenology of color." *Die Zuordnung der Netzhautpunkte* is enriched by Jaensch and Reich on the horopter; Dawson and Kirschmann on binocular mixture; and Kaila on the localization of double images. There are new contributions to binocular parallax by Bourdon, and references to Hillebrand on absolute depth localization, and to Henning, Kaila and Prandtl on the Panum-phenomenon. Studies of Pozdena, Poppelreuter, and A. Müller on the form of the heavens and size of the heavenly bodies are introduced in the section on *Sehgrösse*. The title of section 12, *Die Mikropsie*, is changed to *Die Sehgrössentäuschungen*, and new subject-matter added by Kaila, Jacobssohn, and Marzynski. Except for the report of additional cases of restored sight (by operation for congenital blindness), there is nothing new of importance in the section on theory of depth perception; the summary at the end of the section is dropped.

In Cutaneous Space Perception Friedline and Piéron on the *Raumschwelle*, Goudge on the Weber illusion, Goldstein and Gelb on stereognosis, and Friedländer on the perception of weight are the principal accessions. The sections on the tactual space of the blind, and on the general psychology of the blind are, however, largely rewritten, with many new facts. Wundt's theory of cutaneous space has, unhappily, been dropped. The appendix to this chapter, *Die Raumwahrnehmung anderer Sinne*, reports the more recent papers of Klemm, and the study of Hornbostel and Wertheimer on the localization of sound.

Klemm's monograph on the psychopathology and psychology of Temporal Perception furnishes the chief addition in its field. In the section on the Perception of Movement, values of the various limens of movement as found by Exner and Bourdon are given, and an abstract of a paper by Hillebrand furnishes the basis of a new paragraph on the relation of eye-movement and the direction of attention to the perception of movement. The sub-sections on the after-image of movement and on apparent movement are both recast with the incorporation of new results, in the former chiefly by Wohlgemuth, and in the latter by Benussi, Linke, Wittmann, and Cermak and Koffka. *Gestaltwahrnehmung* is entirely rewritten;

Fröbes treats the *Gestalttheorie* simply as one proposed solution of the unitariness of perceptual experience; although impressed by Wertheimer and Köhler, he is not convinced.

The changes in the fourth division (Psychophysics) are principally slight modifications and corrections of the text. Theophil Lehmann, Brunswig, Herrmann, and Katz and Benussi are, however, reported on the nature of the absolute impression; R. Pauli on the recent physiological evidence for the physiological interpretation of Weber's law; Thomson, Fernberger, George, and Boring on the 'doubtful' judgment; and Spearman, Wirth and Lipmann on rank-order correlation.

The first chapter of the fifth division (Association of Ideas) shows slight modifications; a brief reference to Williams on the memory limen, and to K. Lewin on the nature of certain associative tendencies, are the only accessions. In the two chapters on Learning the chief source of new material is Thorndike's *Educational Psychology*, and the names of American and British investigators who have made experimental contributions to the learning curve (Bryan and Harter, Swift, and Book), and to the problem of formal discipline (Dearborn, Winch, Sleight, Ruger, Carey, Smith and McDougall), are introduced: Thorndike himself appears in many connections, and Foster's results on the perseverative tendency are cited. Excerpts from the studies of Moers, Winzen, and A. Prandtl are also made. The section on the significance of ideational types for learning is rewritten, and much of the subject-matter is new; the sub-section on the nature of logical memory is revised throughout. The term *Tatbestandsdiagnostik* as a section-heading is dropped, and *Die Komplexforschung* is substituted.

The indexes which were attached to the second volume are now divided, and each volume has its own. The subject index of the first volume is, however, not complete. For instance, the terms *Akt, Funktion, Inhalt, Erscheinung, Behavior-Psychologie, Objective Psychologie* do not occur. *Reizirrtum* and *Qualitätenreihe* have each a single reference, whereas the former is mentioned at least twice, and relevant material for the latter is reported in four other places. We have discovered few errors: a cross reference on p. 597 reads 413 and should read 429; the short paragraph at the bottom of p. 22 should follow and not precede that marked (*b*); Bishop's initials (p. 104) are inverted.

On the whole the book is considerably improved. As it stands it is probably the best compendium of the results of experimental psychology that we now possess. The weakest part of the book is still the Introduction; the discussion of the fundamental concepts of psychology (excepting 'method') is half-hearted, and inadequate to the remainder of the book. If the author would give as Introduction a classification and exposition of the outstanding definitions and statements of problem and method, and would then furnish cross-references from corresponding definitions of sensation, perception, feeling, thought and the like, not only would he himself report facts and opinions with clearer insight, but the facts and opinions would also fall into better perspective. Furthermore, the value of the book would be enhanced if the source from which a paraphrase is made were more definitely cited, and, in case a secondary source has been relied upon, that fact were made precisely clear. We hope that in another five years both author and publisher will undertake a second revision as thoroughgoing as the present one. The book is now useful; it can be made indispensable.

H. P. W.

Readings in General Psychology. By Edward S. Robinson and Florence Richardson-Robinson. University of Chicago Press, 1923, pp. xvi + 674. Price $4.50.

Teachers of psychology who wish to broaden their elementary courses by the assignment of collateral reading will find themselves under lasting

obligations to Drs. Robinson and Richardson-Robinson for making so conveniently accessible so large and so well selected an anthology of psychological literature. The work may well find use also as a text in more advanced classes where the aim is to bring divergent views under class discussion.

The material is divided into twenty-two chapters following pretty much the usual text-book order. The selections, 239 in number and all of course in English, are drawn from the work of more than 100 authors, among whom the writers of successful text-books naturally predominate. The most frequently cited are Titchener and Watson, seven times each; after whom follow Angell, Dunlap, Ebbinghaus, James, Judd, Ladd, McKendrick and Snodgrass, Pillsbury, Stiles, Sully, Thorndike and Woodworth with five or six citations each; but other authors not neglected, as witness: Darwin twice, Galton three times, Lewes three times, Ribot five times, Rivers four times, Tylor three times. The temptation to scrappiness, strong in any such undertaking, has been resisted. The average length of the selections is above two pages and a half, and those running to five pages or more are not uncommon. An analytical table of contents and full subject and author indexes place the material under ready control.

E. C. S.

The Psychology of Social Life, by Charles Platt. Dodd, Mead and Co., New York, 1922. Pp. 284.

The purpose of the author is set forth as an attempt to prove the position of psychology with relation to society—that psychology explains society as a derivative from man's psychic need. His attempt at proof develops as follows.

The individual composes and makes society, but society colors all that the individual does. Thus we have three phases of behavior: one, hypothetical and theoretical, the possible action of an absolutely isolated man; the other two practical and real, the action of social man when alone, and his action as part of a group. The first is not to be considered; the second is properly the subject of individual psychology; and the third is the real consideration of the volume, with necessary and frequent reference to the second. That there may be a biological basis for the discussion, the author then explains the cerebro-spinal and sympathetic systems. He then points out that the flow of nerve force follows certain pathways of choice, here being the origin of habit, and of both the similarities and dissimilarities of man. Further, the belief is stated that these acquired nerve pathways, like those of the deep-lying instincts, are in a degree inheritable. Thus man's strongest tendencies antedate civilization, and so he is now compelled to play a part in life to which he is by no means completely adapted. The more recently acquired pathways are correspondingly easily altered. Of course, the nerve and brain patterns thus described do not long remain simple. Experience and memory soon enrich and modify all man's perceptions. But these patterns are the determinants of all that we think and do. The patterns themselves are modifiable, and here lies the interest of psychology; for we are to consider man as a real and workable, though difficult, problem.

Some of these stronger patterns are manifested in the gregarious tendency, the sex instinct, the emotion of fear, the tendency to war; in habit, tradition and custom; in convention, imitation and fashion; these are all treated in Chapters II to VII. The content of these chapters is familiar to readers of sociology, but the various social phenomena are treated as arising from psychological foundations. The chapter on sympathy and suggestion deals with these phenomena as controlling and guiding our lives, not so much through the limited field of consciousness, as

in the "secret operations of the subconscious." This predicates the treatment of mass action and hysteria, in the chapter given to these subjects; the emotional element in the individual is paralleled by like emotional outbursts in society, "the fact being that man through mental inter-action develops a tendency to emotionalism that far transcends any like tendency in him as an individual." A similar situation is presented in the chapter on superstition and error.

Having thus laid down a basis for consideration, the author presents, in Part II of the volume, a psychological theory of society, and carries out some implications of this theory with reference to current ideas of socialism, democracy, and the ethical foundations of society. His theory is based upon an evolutionary process which has produced "an animal with a brain so sufficient to itself that it seems to be something apart from the rest of the body—a relation which gives 'consciousness'—and with a complexity which has brought in the seeming possibility of a choice—a choice which I believe to be 'will'." This animal, man, being at first but a primitive being, had yet to undergo a further process of evolution whereby his intellectual reactions were at first limited to a reinforcement of old bodily tendencies; but, with new discoveries and new conditions, groups began to be formed and man became a social being. His old individual tendencies now passed to the group and there, becoming group tendencies, laid the foundation for group customs, conventions, traditions, and finally, for group institutions. All of these are but so many responses to man's psychological impulses and needs, for institutions do not arise from man's thought, but from the demands of the psychological nature. The psychological idea of society, then, needs only the addition of the two antagonizing tendencies, the primitively egoistic and the new social, and the definition of society follows: "a group of individuals, acting under laws inherent in the individual, gradually evolving, by continuous adaptation, in the general direction of an ever increasing unity of action and purpose." This definition leads to a discussion of various antagonisms found in society today, as labor and capital, the problems developed through over-population, introduction of automatic machinery, poverty, crime. The cure for the evils of society is to come through education of the child, with due consideration of the psychological bases of proper education. There must be much importance attached to an ethical basis as well, in this education. Religion is the mainstay of all people, of men and nations. And in our education, since there are certain brain patterns laid down in man by the repeated experiences of all civilized peoples, let these patterns be early aroused into being from their potential existence in all normal children. If these social concepts are impressed upon the children as facts, as measures of value, with especial emphasis upon the ethical concepts of Truth, Honor, and Duty, the world will soon mend.

Dr. Platt's volume is by no means a comprehensive treatment of either psychology or sociology. In fact, as a treatise on either, it is elementary. Its value lies in the suggestion that there should be a more authoritative study made than is now at hand of the relation of individual psychology to group action. Those who like a "popular" treatment of psychology will find the volume interesting for vacation reading.

Cornell University R. H. Jordan

Anger: Its Moral and Religious Significance, by G. M. Stratton. New York, The Macmillan Company, 1923. Pp. xx., 277.

This volume contains an elaboration of the Taylor Lectures delivered by the author at the Divinity School of Yale University.

The method which he employs in this case is the same as that used in his *Psychology of the Religious Life*,—although not as searching,—

"whereby the spirit of the great religions is drawn from their sacred writings" (vii.). Of course every one recognizes, as does Professor Stratton, the limitations and advantages of the use of such a method of investigation.

Professor Stratton hopes in the present contribution to carry "the explanation of conscience, and of the origin of religion, and particularly of monotheism a firm step farther than hitherto, and [also], that interest will be found in the novel grouping of the great faiths with respect to wrath" (vii.). To the psychologist, I take it, the most interesting part of the book would center about the author's notion of anger, its nature and function. For Professor Stratton anger is a "highly unspecialized impulsion; it has no immediate interest of its own, but arises to protect and to further any interest that you feel" (254). In the sense in which instincts are usually considered—as borrowed from McDougall—"anger and pugnacity are not instincts at all" (252). Anger is not to be found in the various stages of animal life: here the author differs from the customary accounts of such psychologists as James and McDougall. "Each of these [James and McDougall] in his own way would have instinct and emotion closely riveted together, regarding them indeed as but different aspects of a unit fact. According to these writers, anger cannot well be without the physical expression of attack and defense, nor any instinctive attack and defense without its counterpart emotion. But the evidence seems to point to a looser connection between anger and struggle, struggle having at its early stage the unpsychic, almost mechanical character found in a reflex act, although in other respects the two may differ, since struggle calls the whole body into action and is not so local as are most of the reflexes. Struggle, then, as I see it, begins without the powerful reinforcement of anger; and when anger enters into it, the reaction is elevated from the plane of the sensory-motor responses, and becomes a more clear expression of mind because a more clear expression of purpose. There have entered into it vague suggestions of desire, organizing the entire mind and body, simple as these may be, into an active attitude toward the object, in a way not found in reflexes like winking or knee-jerk. Even as struggle marks a distinct advance over plant-like absorption and rejection, so pugnacity in which there is anger marks a distinct advance beyond mere struggle. Only by a metaphor can plants be said to struggle; only by a metaphor can the lowest minds be said to fight" (33f.). "Anger, we may say, is an achievement in mental progress. Its coming is preceded by an angerless existence, but when once it comes it is never permitted to disappear. The better kinds of animal life depend upon its powerful aid" (35).

That anger is of value socially is to be seen from the function it has played in the history of the human race, especially in religious and like social institutions. When anger is manifested in the higher forms of mind it soon becomes a "servant of the moral life, lending its ardor against the enemies of the family and of justice and government. It lends strength to the union between husband and wife, it protects the child, it defends home and possessions, it maintains the rights of the citizen, it jealously favors justice, it wards off the foreign foe. . . . But it is also seen to be an enemy of morals, destroying government, disrupting the family, supporting injustice, coming to the help of greed and lust and selfish ambition. We have found anger unproductive save only when it became subject to some positive creative impulse" (71). The function of wrath in the great sacred writings differs, at times, considerably with each religious institution. "The Jew, the Arab, the Persian saw a God whose heart is for contention, driving his true subjects to wrath and fierce strife. In the group of faiths farther to the East—Taoism, Vishnuism, Jainism, Buddhism—such a thought is far away: instead of anger and conflict they study tranquility undisturbed by hatred or by love. Finally, in a strange meeting of extremes, the great religions that have prevailed in China and in Europe

together with America, agree against both these attitudes; they favor both anger and good-will, the one as servant, the other as the master-passion directed to all men and flowing eternally to and from God" (134f.). "In comparing lower with higher levels of religion, there have been three periods. Even at low levels the open anger of men toward eminent spirits and gods soon ceases to be acceptable. The hostility of spirits and gods toward one another, so widespread in backward religion, becomes in the advanced more measured, more subject to moral control; or it wholly disappears. And upon the upper levels a bound is set to anger between men: deadly hostility, early accepted, falls under suspicion, and is justified only by the vital interests of family or civil government or of religion itself; neither private advantages nor the minor advantages of the community bring it praise" (176). "There seem to exist, then, obscure racial and geographic differences in regard to anger and pugnacity. And these have left their deep mark upon religion" (208). The disfavour of the indiscriminate use of violent anger, especially in the occident,—in individual and *not* in national conflict,—is self-evident; and that is due, doubtless, to the teaching of Christianity, the development of democracy, the teaching of natural science, the influence of woman and philosophy (239-242).

From such conclusions as the above, and a critical insight into other possible alternatives, Professor Stratton points out that the present function of anger should be instrumental only, and that it should be used as a servant of "good will" (258). Anger "should be permitted, even encouraged; but not required. Let those be calm before evil who can hurl mountains against it and be calm; and let those feel outraged who cannot but feel outraged. Each should be left free to be valiant according to his genius" (256). The influence of Aristotelian ethics upon these suggestions is evident. As Professor Stratton writes again, anger can "become fused and tempered with other emotions, and subject to their bidding. The discipline of the boy, the man, the woman, requires, then, not the complete suppression of anger, but the knowledge and habit of being angry with the right person and at the right time and in due measure. As to its goal and object, chivalrous anger is the passion at its best" (259). Even religious institutions can profit by the use of disciplined anger,—as, for example, when "good-will finds the way blocked, finds patience no longer a virtue, the expense of toleration now mounting too high. A man will have need of all his intelligence, of all his wisdom, to make a sound decision, to do more good than harm. But this responsibility comes in the use of any power, of any engine; no steam is raised in a boiler that does not threaten to scald and rend the whole neighborhood" (263f.). Finally, for our guidance in the use of anger, Professor Stratton lays down fifteen concrete rules (260-262).

Professor Stratton's book is interesting reading. Whether his theory of anger—especially that phase of it which claims that it is of a rather late development in the history of organic evolution, and stresses its pure psychic bases, and particularly its moral aspect—is sound, only a very careful study will reveal: a more profound study than the author has allowed himself. Professor Stratton's rule of conduct—the intelligent control of anger—while not new, however, never grows old, and to-day, perhaps more than ever, its constant presentation is valuable. The refreshing style of the book is a genuine pleasure to one who must read much that is poorly written; it reminds one of the contention that serious thinking can still be written as it was by Plato. Professor Stratton, like James and Santayana, has learned the art of writing. We wish other serious authors would be not only instructive, but also pleasant.

Northwestern University A. J. Snow

Intelligence Measurement: A Psychological and Statistical Study Based upon the Block-design Tests, by S. C. Kohs. With preface by L. M. Terman. New York, The Macmillan Co., 1923. Pp. xii., 312.

Professor Kohs sets forth in detail the design and experience of a new performance test relatively independent of the language factor. That such an experimental effort will prove itself of great value in the future there is no doubt; every new intelligence scale empirically determined must be welcome.

Professor Kohs' test material consists of two parts: (1) sixteen color cubes; these "cubes of one inch dimension are all painted as follows: one side red, one side blue, one side white, one side yellow, one side blue and yellow (divided diagonally), one side red and white (divided diagonally)" (64); (2) seventeen designs "graded in difficulty which increases by modifying the designs at various stages" (65). The method depends upon the subject's ability to carry out in practice the skein of color combination that the specific design placed before him contains. The score value for any subject is the functional relation of the actual time utilized by the subject and the number of moves necessary for a *successful* completion.

In all 367 cases were used by Professor Kohs for experimental purposes as well as for the standardization of the new test. The "time spent in the examination of each case varied from one hour to three hours, the average being about two hours" (41). "Each subject was given the block designs, the two Trabue tests (B and C), the Binet test, and the dissected sentences test" (41). Professor Kohs has carefully worked out "tables giving the chronological ages of the subjects, arranged by source; the Binet mental ages of the subjects, arranged by source; the Binet mental ages and the intelligence quotients arranged by sex and source; the correspondence of chronological and mental ages, totaled separately for sex and source; the age-grade distribution, classified by sex; an age-progress table, classified by sex; nationality; place of birth; social class; home conditons; and teachers' estimates of intelligence" (39).

The author's efforts to standardize his test included a year scale such as the Binet, and a point scale derived by the P. E. scale method; while the percentile method and the point-scale method were found by him to be defective as a technique for standardization of his test.

The validity of Professor Kohs' test was determined, by him, by the use of ten criteria which were as follows: "(1) the mental processes employed; (2) increases in score from year to year; (3) correspondence of median mental ages; (4) correlations between mental ages and intelligence quotients; (5) correlations with teachers' estimates of intelligence, and with school standing; (6) conformance of intelligence-quotient distribution with normal probability; (7) correlations with vocabulary, Trabue B and C and Military Test; (8) the probable error of a block-design mental age; (9) the segregation of the feeble-minded; and (10) percentage ratios of agreement and disagreement with established standards in intelligence" (173).

It is interesting to note in this connection Professor Kohs' attempt to prove that his test measures intelligence. He does not agree as to the nature of intelligence either with the group of psychologists represented by Ebbinghaus, Ziehen, and Meumann, or with that represented by Binet, Stern, and Terman. He thinks that the definition that intelligence is the ability to analyse is only partially true, while to "define intelligence as 'adaptation to a new situation' is but little superior to that of an immature youngster who defines 'soldier', 'to fight'. We wish to know what intelligence *is*, not what it *does*" (169f.). We wonder, at this point, whether Professor Kohs wants Kant's "thing-in-itself". Finally, we discover that, after all, his definition of intelligence is functional: that intelligence is the ability to analyse any "situation which confronts one, a critical inquiry into

methods of solving the problem, and a final synthesizing of detail into a consistent whole" (171). In other words, to him intelligence would be a 'gift' possessed only by the human animal, and then only by the one who acts as a result of complete self-conscious ratiocination. That this new test measures intelligence "there seems to be no reasonable doubt, substantiation having been obtained through introspections, that these tests require first, the breaking up of each design presented into logical units, and second, a reasoned manipulation of the blocks to reconstruct the original design from these separate parts. The results of this activity, it is presumed, yield a fair index of this analytic-synthetic power which we have termed 'intelligence'" (172). Professor Kohs' practice, however, controverts his own definition of intelligence; for he uses children as subjects for his investigation, though "a *critical* inquiry into the methods of solving problems, and a final synthesizing of detail into a consistent whole" is hardly to be expected from children, and, indeed, is not called for by his test. Would it not have been better if he had been content to define the function of his tests as modestly as did Professor Boring in the *New Republic* (35, No. 444)?

Professor Kohs also takes the opportunity to present a sketch of the history of philosophical, psychological and neurological development for the sake of orientation and also a discussion of a variety of other topics of general interest. These efforts, we are afraid, are entirely too irrelevant to the main thesis of the book and necessarily too brief to be of value.

Northwestern University — A. J. Snow

Problems in Psychology. By A. J. Snow. New York, Holt and Co., 1923. pp. 115.

There is a growing tendency among teachers of psychology to use problems as a basis for class discussion, criticism, and written work; but good problems are not always easy to find. Snow's *Problems in Psychology* thus meets a definite need. The problems are not intended to replace the usual questioning in psychology, but rather to supplement it. They are so arranged that they may be used with any standard text-book; no single point of view predominates, and they are numerous enough to permit selection. The collection is divided into sections in such a way that a single section may conveniently be assigned along with a given chapter in a text-book. The problems are practically all in the form of statements, many of them quotations. The pupil is asked to explain, justify, illustrate or criticise the statement. The collection shows judicious selection and painstaking care in compilation. It is easy to predict that the book will find a ready welcome among instructors and will be widely used.

Clark University — Miles A. Tinker

NOTES

THE SEVENTH INTERNATIONAL CONGRESS OF PSYCHOLOGY

Fourteen years elapsed before it was again possible to arrange an International Congress of Psychology. The Sixth Congress took place in Geneva in 1909; the Seventh Congress was held at Oxford from the 26th of July to the 2nd of August 1923. Considerable progress has been made in the science during these years. It has been above all else a period of intensive experimentation; but those members who expected the programme to reflect this development were disappointed. There were several excellent reports of experimental work, and the representatives of the *Gestalt*-School described this latest trend in empirical investigation and psychological theory; but the papers were for the most part so very general in nature that the experimentalists frequently felt themselves out of touch with the meeting. An International Congress, however, may not be the place for a large number of reports on specific subjects.

A comparison of the programmes of the last two Congresses reveals three outstanding changes. The programme of the Seventh Congress shows a falling off of papers on animal psychology, an increased interest in applied psychology, and the appearance of papers based on the Freudian theory. In the Sixth Congress there was a section devoted to animal psychology, and there were seven papers on this topic. At that time there was practically no psychotechnical work being done by psychologists, and the only papers that foreshadowed this development were those dealing with education and testimony. The Freudian theory was just emerging from the unconscious. At the Seventh Congress there was only one paper devoted entirely to animal psychology. Köhler merely referred to his work with chimpanzees in order to illustrate his general theory of perception. On the other hand the session which inspired the most discussion was the one devoted to the subject of vocational guidance; and there were also several separate reports of applied work. Five papers had direct bearing on the Freudian hypothesis.

There were 584 names listed as members of the Sixth Congress, as against 219 for the Seventh Congress. Of this latter list of members somewhat over half were present at this meeting. The names of twenty-two members (twelve of whom attended the Seventh Congress) appeared on the lists of both Congresses.

It was thought that the political and economical conditions of Europe would prevent most of the foreign psychologists from coming to Oxford. It was therefore a gratifying surprise to find so many different countries represented, in most instances by their most distinguished psychologists. The following is a selected list of those present: England—S. Alexander, P. B. Ballard, F. C. Bartlett, R. J. Bartlett, H. Binns, W. Brown, C. Burt, J. Drever, J. C. Flügel, D. K. Fraser, R. G. Gordon, Bernard Hart, Henry Head, M. W. Keatinge, J. T. MacCurdy, R. J. Mackay, C. S. Myers, C. K. Ogden, T. H. Pear, F. C. S. Schiller, A. F. Shand, C. Spearman, H. Sturt, M. Sturt, R. H. Thouless; America—E. G. Boring, C. McF. Campbell, Ivy Campbell, R. Dodge, L. Hoesch-Ernst, H. S. Langfeld, J. H. Leuba, Adolf Meyer, H. T. Moore, W. B. Pillsbury, Morton Prince, B. Rand, L. L. Thurstone, E. C. Tolman, H. C. Warren, J. V. Yarbrough; Austria—A. Adler; Belgium—O. Decroly, A. Fauville, F. Fransen, A. Michotte; France—P. Bovet, Pierre Janet, H. Piéron; Germany—K.

Abraham, W. Köhler, K. Koffka, O. Lipmann, W. Moede; Holland—G. van Wayenburg, H. Zwaardemaker; Hungary—G. Révész; Norway—M. L. Rymert; Spain—G. Dwelshauvers, E. Mira; Sweden—S. Alrutz, S. E. Henschen, G. A. Jaederholm, E. Sjöbring, T. Thunberg; Switzerland—E. Claparède, H. Flournoy, and A. Reymond.

On the evening of the first day there was a reception on the lawn of New College and the following afternoon Dr. and Mrs. William Brown gave a garden party in the beautiful grounds of Worcester College. The members were thus given an opportunity to become acquainted with one another at the beginning of the Congress. The meetings were held in the lecture rooms of the University Museum and were always attended by two hundred or more members and guests. There were discussions after every paper, in which Janet, Piéron, Claparède and Decroly were most prominent. C. S. Myers, the President of the Congress, presided, but he invited senior representatives from the different countries to act as honorary chairmen of the several sessions. H. C. Warren and Adolf Meyer were selected from the American group.

The first of the five symposia was on "The Nature of General Intelligence and Ability." G. H. Thomson criticised Spearman's theory on the ground that mind is much too complex to warrant the assumption of one factor underlying general ability. He discussed the inheritance of unitary factors and the influence of the environment. Intelligence was defined by him as the ability to meet new situations with old responses and to discard those responses which prove unsuccessful. There may be nothing "general," however, in this form of adaptation. Claparède described the following problems which are related to the question of general intelligence: mental age, individual differences, opposition between special aptitudes and general intelligence, correlation of aptitudes, vocational tests, integrative action of intelligence, solution of new problems. He concluded that (1) intelligence should be defined as the ability to solve new problems; (2) the term "general intelligence" should be used to denote the average mental age of an individual; (3) we should cease to refer to Spearman's G factor as general intelligence, thus avoiding confusion; and (4) we should investigate individual difference, vocational tests, and the integrative action of intelligence. Thurstone seemed to accept the existence of general intelligence. He defined intelligence in more specific terms than did his predecessors. He believed that intelligent conduct is a trial and error process at a level at which our actions are incomplete and approximate. On the basis of this definition he criticised non-language intelligence tests which require overt action rather than abstract thinking for their solution.

In the symposium on "Does Progress in Educational and Social Science depend on progress in Psychology?" M. W. Keatinge explained that psychologists would answer the question in the affirmative, but that some educators were still doubtful. Both he and P. B. Ballard enumerated some of the influences which psychological investigations have had upon education. These facts are too well known to psychologists to be enumerated. The speakers confined their remarks chiefly to the field of education.

E. D. Adrian was the first speaker in the symposium on "The Conception of Mental and Nervous Energy." He prefaced his remarks by stating that from a physiological point of view the less one said about nervous and mental energy the better for a clear understanding of the problem. He emphasized the fact that the term "energy" has been used in two different senses, the biological and the physical. The psychologist may have a right to speak of mental energy, but it must be defined in different terms from physical energy. The use of the term "energy" in some of the Freudian literature has caused much confusion. McDougall's hypothesis of "neurin" is more useful to the psychologist and the physician than it is to the

physiologist. Adrian then discussed the present status of the problem of nerve conduction, and concluded that if we use the term "energy" in the physical sense then the term "nervous energy" is unnecessary and "mental energy" impossible. Head's principal endeavor was to show how the problem of the relation of mind and body can be solved by an examination of experimental data. He described several cases of lesion and referred to the work of Sherrington on spinal and decerebrated animals. For effective action, both conscious and reflex, there is necessary a high degree of physiological efficiency, which depends not only upon the structure but also upon the condition of the nervous system. For this state of high grade physiological efficiency he used the term "vigilance," which he seems to have chosen for its mental connotation. Conscious processes bear the same relation to the higher centers of the nervous system as do purposive reflexes to the vitality of those centers which are lower in the neural hierarchy. Both are the expression of physiological vigilance. Myers did not agree with Adrian that the concept of a special kind of nervous energy, which does not obey the laws of thermodynamics, is unnecessary. He asserted that there is such a form of energy which appears as the constructive and directive force of the highest levels of mental activity, and that it cannot be neglected on the physiological side if we are to have a consistent psychophysiological parallelism. Sjöbring, in his paper on "The General Forms of Mental Activity," differed radically from the speakers in the symposium. The character of mental processes is activity, the form of which is found in feelings. The problem is to investigate the variety of feelings. The different qualities of feeling determine different aspects of mental activity.

Drever and Ernest Jones took part in the symposium on "The Classification of the Instincts." Drever agreed with the use of the term "instinct" as covering behavior forms, or reaction patterns, which have a drive or urge as their psychical aspect. He criticised existing classifications, especially the biological one, which is best exemplified by Freud's "altogether too alluring" threefold division into herd, sex and ego instincts. He proposed a physiological classification as follows: (1) general and specific; (2) under each head, appetitive and reactive; (3) under reactive, simple and emotional. Jones did not believe that Drever's differentiation between appetitive and reactive instincts is a fundamental one. In his outline of the contribution which psychoanalysis has made to the subject he defended Freud's theories. The most important positive contribution of the Freudian psychology is probably the demonstration that the normal sexual instinct is more complicated and extensive than was previously supposed. Jones proposed to divide the instinctual manifestations into "attractive" and "repulsive." The former group comprises hunger and sexualities, from which are derived curiosity and possessiveness; the latter, aversion, flight, and aggressiveness. In the discussion which followed E. C. Tolman insisted that too much attention has been given to classifications and too little to the manifestations of instincts.

The last symposium was on "The Principles of Vocational Guidance." Otto Lipmann, in his discussion of this problem, was strongly influenced by the theory of *Gestalt*. He criticised the analytical method on the ground that an individual's ability is not a sum of his separate traits. Therefore he emphasized the importance of the method of observation, which permits a qualitative study of the individual's personality. The analytical method should be used to ascertain ability for special work, the observational method to discover the fitness of the applicant for an occupation. Cyril Burt considered the following points: the age at which vocational guidance can best be applied, the organizations which should undertake vocational guidance, the analysis of qualifications, and the traits which should be considered in giving advice. Under the last head he stressed the importance of temperamental qualities. He agreed with Lipmann that the

recommendation must be based on a psychological profile rather than on a special vocational aptitude. Thurstone gave a list of thirty-three principles of vocational testing. Several papers in vocational psychology followed the symposium. Moede described by means of lantern slides the vocational tests which he is giving in Berlin, and van Wayenburg showed some tests which he has been offering. In the discussion Langfeld emphasized the importance of the interview and of the consideration of the mental hygiene of adult applicants.

There were special addresses at several of the evening sessions. Morton Prince offered a thesis that consciousness is not synonymous with self-awareness. Conscious processes may be segregated from the dominant consciousness and function co-consciously without any self-awareness. A study of these dissociated processes gives us a clue to the interpretation of infra-human minds. His arguments for co-conscious states are familiar to those who have read his numerous publications on this theme.

F. C. Bartlett spoke on "Symbolism in Folk Lore." Ernest Jones objected to Bartlett's treatment of this subject, in that he dealt only with the manifest content.

William Brown read a paper on "Mental Conflict" before a very large evening audience, and T. H. Pear described the work which he, R. H. Thouless and Miss Ikin did upon the "Psycho-Galvanic Reflex in Dream Analysis."

On Sunday evening in Wadham College Canon Streeter discussed the question "Is Religion a Psychoneurosis?" If it is, he remarked, then the more religious a man is, the more severe should be the neurosis, which is an absurdity in the face of the facts. Henry Head protested vigorously against the title of the address. On the same evening B. H. Thouless, under the title of "The Psychology of the Contemplative Life," described the mental attitude of some of the famous mystics.

Beside the symposia, allied papers, and lectures there were fifteen individual papers. The Congress was fortunate in the presence of Köhler and Koffka, the most representative members of the new school of German psychology. The addresses of these men stood out in relief from the background of the more traditional doctrines which dominated the meetings. The introduction of Köhler, who was the first member to use the German language, was made dramatic by the fact that Piéron, a Frenchman, was the acting chairman at the time. Shortly after the meeting, five or six psychologists interested in the problem of *Gestalt* gathered in a private room for further discussion of this problem, and in the intimacy of this small group Köhler of Berlin and Michotte of Louvain engaged in friendly debate over the principles of the new school. These are two of the many examples of the delightfully broad-minded and tolerant attitude of the members of the Congress.

Köhler described the cardinal points of his theory, which he has already explained in his publications on anthropoid apes and in his book "Die physischen Gestalten in Ruhe und im stationären Zustand." He also illustrated by means of lantern slides the subordinate rôle which association plays in our perception of form.

Koffka, who graciously assented to the request of the Chairman by speaking in English, described his latest experiments upon apparent movement through the region of the blind spot. It transpired in the course of the discussion that Michotte had performed similar experiments at Louvain and had obtained the same results without previous knowledge of the German work.

Boring described the results on the two-point limen, which he obtained in his experiments on nerve division. These results were the outstanding incompatibles between his experiment and that of Head and Rivers. He presented data to show that his findings were not influenced by the small-

ness of the tactual area on which he worked. In his conclusions he held to his original statement that the perception of duality is mediated by deep sensibility. There followed an interesting exchange of views on method between him and Head. More discussions such as this one upon concrete differences between scientists would greatly add to the value of Congresses.

MacCurdy sketched the development of human behavior from simple reflexes, through reaction patterns, chain instincts, imaginal instinctive reaction, to intelligent thinking. Intelligent planning consists in the comparison of images with perceptions. In original thinking there is the fabrication of new images. When pattern reactions operate with images there is unconscious instinctive motivation, which corresponds to Holt's Freudian "wish."

Révész's paper was divided into two parts. On the basis of numerous experiments he asserted that hens peck by aiming at the grain; and as they require visual stimuli for this act, they do not peck in the dark even when left for several days, unless they are especially trained. He also offered proof that hens perceive optical illusions. After they are trained to peck at grain on the smaller of two geometrical figures, they will peck the grain only on the apparently smaller of two figures which are objectively equal in size.

Piéron described the various problems connected with perception of time, and offered suggestions for further experimentation. He discussed the laws of the temporal attribute of sensations and the perception of simultaneity, succession, duration, change and motion. In his treatment of the last two phenomena he referred to the work of Wertheimer, Korte, Koffka, Benussi, Burtt and Dimmick.

Dwelshauvers explained his objective method of studying mental imagery, which consisted of registering the accompanying unconscious movements.

H. Binns and H. S. Raper reported the results of experiments which were made to determine the accuracy of tactual and visual judgments of minute differences in the physical properties of wool. Those individuals who have had practice in the arts and crafts can judge well by either sense. Other subjects depend more on visual judgments.

The experiments on "Indirect Measures of Fatigue," by C. H. Griffiths and W. B. Pillsbury, were reported by the latter. Fatigue was induced by typewriting and by solving problems. Blood pressure steadiness and attention were measured. There was a decrease of systolic pressure after work in the majority of cases. Steadiness increased. The results for attention were ambiguous.

Miss M. Sturt reported the results of her work on "The Judgment of Time in Sleep." E. Mira described his method of recording "The Cardiovascular Changes in Mental Work." Pierre Janet spoke on "Psychic Asthenia and Atony." S. Alrutz emphasized "The Psychological Importance of Hypnotism." Karl Abraham gave his "Psychoanalytic Views on some Characters of early infantile Thinking." Alfred Adler, in his paper entitled "Advances in Individual Psychology," laid special emphasis upon his well-known theory of organic inferiority.

There was an exhibition of psychological books which have recently appeared in England, and of a few new instruments. Michotte's tachistoscope and apparatus for registering movements aroused the most interest.

The banquet was held in the historic hall of Christ Church. Myers, Prince, Janet and Köhler were among those who made short speeches. The last day of the Congress was devoted to an excursion to Cambridge. After a luncheon at Caius College, at which Dr. Myers was host, the members visited the well-equipped psychological laboratory under the guidance of the director, Mr. Bartlett.

A report of the Congress is hardly complete without mention of the eloquent sermon preached at Christ Church by Canon Barnes of Westminister from the text "Great is truth and stronger than all things." He expressed the kindest appreciation of the work of the psychologists by reminding them that there was a moral as well as a scientific goal, a spiritual as well as an empirical world. He paid tribute to the memory of Rivers by ending his sermon with the following quotation from the writings of that lamented scientist: "Concerning the energy derived from mental conflict, we do not know how high the goal to which it may reach." It was voted at the business meeting that this sermon should be included in the Proceedings.

Harvard University H. S. LANGFELD.

THE FUNCTIONS OF THE SYMPATHETIC SYSTEM IN CURRENT PSYCHOLOGICAL TEXTS

Recent investigations of bodily changes in emotions have directed the interest of psychologists toward the operation of the sympathetic or autonomic nervous system (the two names seem to be used interchangeably). Teachers of elementary psychology frequently describe the sympathetic system as if it were capable of acting independently of the cerebrospinal system. They speak of various activities of the viscera as being 'controlled' or 'governed' by the autonomic system, and give to the student the impression that the sympathetic ganglia act as centers for reflexes affecting the viscera, that is, as centers for reflexes which do not involve any neurones in the cord.

A study of current textbooks in introductory psychology reveals that many of the authors hold to the same mistaken concept of the functions of the autonomic system. A few quotations will serve to illustrate this tendency. "While the autonomic system is connected with the cerebrospinal nervous system, it is a relatively independent and self-directing system. It is due to its action that the vital and automatic processes of respiration, circulation, digestion, and the glandular processes are carried on without burdening the central nervous system."[1] "The latter [the autonomic system] is so-called because it acts automatically....This system controls the smooth muscles and glands, particularly the endocrine system of which it is an integral part."[2] "The latter [smooth muscles], including the glands of internal secretion, are in large part governed by the autonomic system."[3] "Its [the sympathetic system's] function is the control of the action of glands and smooth muscles.....In addition afferent impulses come from all of the viscera and often go on to the spinal cord."[4] "A stimulus coming in over a sensory neurone, passing to various parts of the cord or up to the cortex, also sends some of the energy by one of its branches into the sympathetic system."[5] "They [the sympathetic ganglia] act to a large extent independently, controlling movements within the body. In this way, for instance, they regulate the supply of blood to the various parts of the body."[6] "The striking peculiarity about the autonomic system, as its name indicates, is its relative self-directing or autonomic activity."[7] "It is through the operations of the sympathetic system that all the vital processes of digestion, respiration, and circulation are carried

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[1]B. B. Breese, *Psychology*, 1917, 31f.
[2]C. E. Seashore, *Introduction to Psychology*, 1923, 324.
[3]*Op. cit.*, 338.
[4]W. S. Hunter, *General Psychology*, 1919, 144.
[5]H. H. Goddard, *Psychology of the Normal and Subnormal*, 1919, 127.
[6]B. Dumville, *The Fundamentals of Psychology*, 1912, 28.
[7]J. R. Angell, *Psychology*, 1908, 56.

forward."[8] "The activity of the autonomic system governs the organic or biological life processes, so that usually they operate without conscious control."[9]

Reference to recent authorities in physiology and physiological psychology makes it evident that a view of the sympathetic system as a center of independent activity is incorrect. Starling states the case very clearly. "Since the main nervous system is characterized by the possession of nerve cells, it was formerly thought that any collection of nerve cells must partake of the coordinating and reflex functions of the central nervous system, *i.e.*, must act as local nervous centers. All efforts have failed however to prove the existence of such a function, and we must conclude that the sole use of these [the sympathetic] ganglia is to serve as distributing centers.... Indeed, the essential part of a nerve center is not the nerve cells at all, but the presence of a complex tangle of fibres, rendering possible the passage of impulses in all directions.....All such mechanism is wanting in the sympathetic ganglia, which contain neither association fibres between different cells of a ganglion nor commissural fibres between cells of adjacent ganglia."[10] Luciani and Martin make similar statements. "We have learned that the fundamental property of the central nervous system lies in its capacity for subserving reflex acts, so in order to discuss this question we must ascertain whether the ganglia of the sympathetic system are capable of subserving reflexes.....Any such possibility must *a priori* be excluded .....The excitations which they transmit must therefore reach the centers of the cerebrospinal axis before they can be reflected again to the periphery."[11] "The special physiological function of the sympathetic system may be stated in a sentence: it forms the efferent connection between the central nervous system and all the smooth muscles and glands of the body, and the heart."[12] From the point of view of the physiological psychologist, both Dunlap and Ladd and Woodworth state the same facts. "These ganglia are sometimes called 'nerve centers,' and properly come under one of the several meanings of that highly confusing term. But they are not (and this is important) structures in which afferent currents are converted into efferent currents. No reflex, in other words, can take place through these ganglia alone; the afferent current must go into the cord, even if it does not go up to the brain, before it can be redirected outward to any of the effectors. The ganglia of the visceral nerves have merely a distributive function."[13] "It is evident, accordingly, that the older conception of the sympathetic as a relatively independent reflex center, charged with the coordination of the more 'vegetative' functions of the organism, must be abandoned. The center for such coordination is to be sought in the central nervous system, of which the sympathetic is but an adjunct. It is even doubtful whether limited local reflexes occur by way of the sympathetic ganglia. The latter are, more probably, not reflex centers at all, but simply relay stations in the path of the outgoing fibres to the organs mentioned."[14]

It would be unfair to conclude this arraignment of the physiological conceptions of psychologists without pointing out that a few elementary texts give a correct account of the functions of the sympathetic system, as the following quotations show. "The autonomic system as a whole con-

[8] J. R. Angell, *Introduction to Psychology*, 1920, 46.
[9] H. C. Warren, *Elements of Human Psychology*, 1922, 36.
[10] E. H. Starling, *Principles of Human Physiology*, 1920, 474.
[11] L. Luciani, *Human Physiology* (trans. by F. A. Welby), iii., 1915, 373.
[12] E. G. Martin, *The Human Body*, 1912, 165.
[13] K. Dunlap, *An Outline of Psychobiology*, 1917, 100.
[14] G. T. Ladd and R. S. Woodworth, *Elements of Physiological Psychology*, 1911, 150f.

sists of a number of ganglia, whose function is to receive motor impulses from the cord and distribute them to the viscera and glands that they excite."[15] "Afferent neurones are distributed to the tissue which the sympathetic controls, but these afferent neurones belong to the afferent peripheral cerebrospinal system.....There is no substantial evidence to show that the sympathetic system has an afferent supply of its own."[16] "The autonomic is not separate from the main nervous system, but consists of outgoing axons from centers in the cord and medulla."[17] It is indeed unfortunate that these texts are in the minority!

University of Pittsburgh GILBERT J. RICH

RECURRENT IMAGES

I recently had occasion to drive an automobile 400 miles within the span of a day. The excessive visual and kinaesthetic stimulation produced, when I went to bed,—as I did immediately upon arrival home,—two phenomena which I think take their place with the recurrent images.[1]

The movement of the car was retained in kinaesthetic imagery and was transferred in perception to the bed upon which I lay. My fellow travellers reported a like experience.

I alone, however, experienced visual after-images of movement, which were projected upon the field of my closed eyes or, with open eyes, within the darkness of the bed-room. The movement was toward me, a positive duplication of the perceptions aroused from watching the roadway. During the entire time of driving I kept my eyes fixedly upon the road directly ahead of the car. My companions, on the other hand, directed their eyes to whatever attracted them, and looked at the in-rushing road only intermittently. In my case, stimulation was constant and of long duration; it was also intense, particularly during the last half of the trip which was made under the illumination of strong head-lights.

The kinaesthetic images here reported are probably of common occurrence. The experience is at least similar to that often reported by voyagers after landing. It may, of course, not be wholly imaginal. The recurrence of visual images aroused under these conditions has not, however, to my knowledge, been before reported. K. M. D.

A SIMPLE APPARATUS FOR THE DETERMINATION OF THE *RL* OR *DL* OF COLOR BY THE METHOD OF CONSTANT STIMULI

This apparatus is made to suit the needs of the small laboratory, or to render possible the inexpensive multiplication of pieces in a large laboratory. The critical and difficult aspect of the apparatus is the preparation of the color stimuli. For an *RL* a color is mixed with gray to give a series of 9 members. When the steps are very small for a given *O*, a proper distribution of judgments may be obtained by using numbers 1, 3, 5, 7 and 9 of the series. If these steps are too large for another *O*, an adequate distribution of judgments may be obtained by using the smaller steps, 3, 4, 5, 6 and 7.

The 9 stimuli are painted in kalsomine on a cardboard wheel, which is sufficiently large to carry another set of 9 stimuli, varying in degree of

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[15]W. B. Pillsbury, *Fundamentals of Psychology*, 1922, 85.

[16]J. B. Watson, *Psychology from the Standpoint of a Behaviorist*, 1919, 154.

[17]R. S. Woodworth, *Psychology, A Study of Mental Life*, 1921, 124.

[1]In the sense in which the term is used by E. B. Titchener, *Beginner's Psychology*, 1915, 75.

chroma by the right amounts to give a set of judgments for a *DL*. A smaller wheel carries a duplicate set of *DL*-stimuli, and has an extra space to match the color of the screen through which the stimuli are observed.

Both wheels are mounted on a slide with windows for the exposure of the stimuli, one window for each wheel. The slide lies flat against the exposure screen, where there are two more windows. In one position of the slide both windows in both screens coincide, and the colors are exposed for *O*'s judgment. In the other position the slide closes the windows in the screen, and *E* secretly adjusts the colors for the next judgment. This arrangement of stimuli and screen brings the colors only a few mm. from the front of the exposure screen.

By means of the duplicate sets of *DL*-stimuli one may take account of the space-error in a simple way, and by numbers on the back of the wheels *E* can easily tell which color is being exposed. An arrow or other mark on the slide, to which *E* always brings a line or arrow drawn on the back of the wheel, guarantees that portions of two stimuli will not be exposed inadvertently.

In the models built for our own use, the windows in the screen are square, with 1 in. sides, and the width of the strip of grey screen between the windows is also 1 in. These dimensions have proved convenient.

Cornell University H. G. Bishop

The Term 'Attensity'

In systematic lectures of the last half-dozen years I have started out, not from the 'mental elements,' but from the ultimate 'dimensions' of psychological subject-matter: quality, intensity, protensity, extensity and attensity. Protensity is, of course, the pre-temporal welling-forth noticed, *e.g.*, by Curtis' *O*s (this Journal, 27, 1916, 43 f.); and extensity is the pre-spatial spread exemplified by Katz' color film (*cf.* M. F. Martin, *ibid.*, 33, 1922, 459) and by tonal volume. I have coined the word 'attensity,' to replace what is ordinarily called sensory or attributive clearness or vividness, in the hope that its formal likeness to the other dimensional terms and its freedom from associations may help to clear up the confusion of cognitive and attributive clearness. The word will appear, in this sense, in our Cornell Studies.

E. B. T.

Publication Ceases in Germany

Professor William Stern of the University of Hamburg informs me that German publishers have indefinitely postponed the printing of all scientific books and periodicals on account of the rising wages of printers.

Harvard University H. S. Langfeld

Errata

A slip in the handling of paged proofs led to the appearance, in the last number of the Journal, of an unusual number of printer's errors. The Publisher joins me in offering an apology to our contributors and readers for this annoyance. I think that none of the misprints—except perhaps the date 1873 for 1872 in the Footnote to page 473—can be misleading.

E. B. T.

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AN EXPERIMENTAL STUDY OF THE VISUAL NEGATIVE AFTER-IMAGE[1]

By CATHERINE C. BRADDOCK[2]

Katz[3] tells us that the visual negative after-image is a film. The purpose of the present Study was to test this statement, to obtain descriptive reports of the image, to learn the effect of certain perceptive motives upon it, and to compare the images from surface and film, and from various sorts of tridimensional objects.

All work was done in a dark room. In every experiment the *O* sat 2.5 m. from the presented stimulus, and viewed it binocularly through a black tube, 1.25 m. long and 25 cm. in diam.

The *Os* in Experiment I, completed during the summer session of 1921, were Dr. L. B. Hoisington (H), assistant professor of psychology; Dr. J. Gleason (G), assistant professor of psychology at Vassar College; Dr. H. G. Bishop (Bi), instructor in psychology; Miss C. C. Braddock (Br), Miss Amen (Am), Mr. Andrews (An), and Mr. Talman (T), graduate students in psychology. Bi, Br, G, and H were trained *Os*; Am, An, and T were relatively untrained. In Experiments II, III, and IV, carried on during the following winter, the *Os* were Bi, Br, and H; and Mr. J. P. Nafe (N) and Mr. A. C. Reid (R), graduate students in psychology, comparatively untrained in observing.

In every series the *O* was instructed to fixate the stimulus and to describe, in terms as nearly attributive as possible, the after-image which it occasioned. He was asked to note especially how the texture of the after-image compared with that of the stimulus. The exposure periods for the stimuli varied from 30 to 60 sec.

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[1]From the Psychological Laboratory of Cornell University.

[2]Miss Braddock carried out all the experiments here reported; her untimely death prevented her from working up the results for publication. Dr. Grace Adams, who had followed the course of the investigation, very kindly offered to undertake this difficult and responsible task, and the paper now printed (including the Table) is due to her friendly interest.—*Editor.*

[3]D. Katz, *Die Erscheinungsweisen der Farben*, 1911, 56ff.

Experiment I. After-images from Surfaces and Films

Series i: *Surfaces.* The stimuli were round pieces of Milton Bradley paper, 8 cm. in diam. (numbered 1 red, 2 yellow, 3 green, 4 blue, 5 black, 6 white), attached to a screen of neutral gray cardboard, illuminated from both sides. The *Os* projected the after-images aroused by these stimuli (*a*) upon the lids of the closed eyes, or (*b*) upon a gray screen identical with (and drawn immediately in front of) the screen against which they had seen the stimulus. In projection (*a*) *E* turned off the lights as *O* closed his eyes.

Series ii: *Films.* The colors of the films were those of the surfaces. Our means of reduction was that which Martin describes. The openings in the neutral gray screens, 40 cm. apart, were 10 cm. in diam. The methods of projection of the after-image were the same as in Series i.

Here, as in the subsequent experiments, we left the choice of the colors to chance; but while later we completed each series within an experiment before beginning the next, here we presented first a surface color, and then the corresponding film.

Experiment II. Flat Objects

Series i. The stimuli were flat figures cut from Milton Bradley papers, mounted on cardboard, and representing twelve familiar objects (1 rabbit, 2 soldier, 3 leaf, 4 teapot, 5 cat, 6 hen, 7 flag, 8 house, 9 bottle, 10 yacht, 11 boot, 12 seascape in picture frame). The size of these objects varied from 3 by 2 to 5 by 4 in. Their colors were so chosen that the colors of the after-images should be "natural." The first six stimuli had only one color; the last six, two or three.

Series ii. The objects were the same as in Series i, but the dark figures had a narrow white border, and the light figures a narrow black border. In both series the objects were presented against a vertical gray background, and the after-images were projected by methods (*a*) and (*b*).

Experiment III. Bulky (Transparent) Objects

Series i. The seven bulky objects (1 a small bottle filled with red liquid, 2 a small bottle filled with yellow liquid, 3 a blue glass cube, 4 a green glass oval, 5 a blue glass oval, 6 a clear glass oval with colored flowers, 7 a clear glass cylinder with green stripes) stood upon a narrow glass shelf, projecting from the vertical screen. Behind them, to enhance their color, were white cardboards, cut to their shape.

Series ii. Stimuli 1, 2, 3 and 4 stood upon a horizontal ground glass screen, illuminated from below.

Series iii. Black-lined tissue paper was placed between the ground glass and the four objects of Series ii. The black lines showed through the transparent objects.

The after-images were still projected (*a*) upon the closed eyes or (*b*) upon a gray screen.

Experiment IV. Opaque Tridimensional Objects

Series i. The place of the five stimuli (1 a pasteboard cube with white outlines and faces of light and dark red, 2 the frame of a cube, made from black wood, 3 a small china cup painted yellow, 4 a red plaster apple, 5 a gray toy horse) was on the glass shelf of Experiment III. A fine thread, with a knot for fixation point, hung in front of every object. To the projection methods already described three others were added: (*c*) the object was snatched away, and the *O* was instructed to continue to fixate the knot; (*d*) the object was left visible, but the lights were dimmed so that its color could not be seen, and the *O* was instructed to project his image upon this darkened object; (*e*) the *O* was instructed to project his image on the black wall of the dark room. Methods (*a*) and (*c*) were employed for all five objects, (*e*) for only 1 and 5, and (*b*) and (*d*) for 1 alone.

Series ii. The five objects, with fixation thread in front, stood on the horizontal glass of Experiment III. We used projections (*a*), (*b*) and (*c*), as in the first series.

Results

The negative after-image, critically regarded in its own right, is always a film. This statement anticipates what is to come, and apparently contradicts many of our reports; but to avoid ambiguity, we make it here. Martin has familiarized us with the true pretextural, predimensional, unlocalized film, and its changes with localization[4]. Our after-images, while affected by localization, were subject to other perceptive motives also. Hence our *O*s gave reports which, while they in no wise contradict the Martin findings, do, owing to the different trends of the problems, vary from them. By careful analysis and after many re-cataloguings, we have been able to sum up these reports in the following table. The table includes all descriptions given throughout the entire experiment, except those of hue and chroma. These we have purposely omitted: hue, since it was determined by the stimulus; and chroma, because, although in their general remarks the *O*s spoke of the goodness of the chroma of most of the after-images, they characterised it specifically only when it was unusually and startlingly vivid, or when it was so poor as to make other description almost impossible. If then we should give the frequencies of the reports "brilliant," "good," "poor," the chromas of the images in general would seem much poorer than in fact they were. We note only that somewhere in the first experiment every *O* described chromas which were merely "good;" but that after this experiment H alone characterised chroma when it was neither "brilliant" nor very "washed out."

We did not fractionate our *O*s' reports into descriptions of Texture and Dimensionality, Localization, Shape, and Objectivity, until the experimental work was entirely over. We then fractionated them in many different ways before we decided on these four headings for our chart. At the end we found that, when we had given a report its proper place in one or more of these groups, we had, except for hue and chroma, completely analysed the report. From the general instructions it was not expected (and indeed, because of the complexity of the experience, it was impossible) that every report should mention all five characters. What is surprising is that—despite the general nature of the instructions, the complex experiences, and the disparity in the training of the *O*s—every *O* at one time or another noted every character, and gave descriptions which

[4] M. F. Martin, Film, Surface, and Bulky Colors, and their Intermediates, this JOURNAL, xxxiii, 1922, 451 ff.

exemplified every sub-group of that character; and that these descriptions are so consistent from O to O and from experiment to experiment that we can record them all in a single chart. For our problem this simplicity is fortunate and important; it allows us by mere inspection to note how slightly the images of one experiment differ from those of another. This is our significant gross result. The detailed results are the sub-groups. The rest of the paper we shall devote to exemplifying these sub-groups, and to showing their relations to one another.

No. Reports	Exp.	Series	Proj.	Localization				Texture and Dimensionality					Shape					Objectivity		
				1	2	3	4	1	2	3	4	5	1	2	3	4	5	1	2	3
105	I	i	*a*	28	9	0	0	72	14	2			22	16						
105			*b*	0	54	6	34	54	12	34			35	20						
105		ii	*a*	45	15	0	0	75	12	6			24	15	1					
105			*b*	3	54	3	25	57	10	27			17	11						
120	II	i	*a*	14	4	0	0	92	17	2	1	0	29	30	2	19	7	22	34	10
120			*b*	9	29	5	23	83	7	20	3	3	18	36	3	29	11	26	30	16
180		ii	*a*	2	2	0	0	113	34	7	12	3	15	42	1	34	20	31	45	18
180			*b*	6	14	4	27	96	1	40	3	5	18	43	1	40	26	28	49	24
70	III	i	*a*	7	2	0	0	56	8	1	1	2	10	17	1	10	1	38	16	3
70			*b*	4	4	2	8	49	4	9	1	1	16	19	2	12	1	36	20	2
20		ii	*a*	0	1	0	0	12	2	0	0	3	10	4	1	0	1	5	6	[illegible]
20			*b*	0	0	0	4	19	2	1	1	2	7	10	0	4	1	10	7	0
20		iii	*a*	5	0	0	0	6	3	0	0	2	6	4	0	5	2	4	2	1
20			*b*	0	8	0	3	10	3	4	0	2	6	5	0	7	2	6	5	2
20	IV	i	*a*	0	0	0	0	24	1	1	0	2	10	16	1	11	3	15	24	2
50			*b*	0	5	0	2	4	1	1	0	2	3	9	0	1	3	4	14	0
100			*c*	0	22	13	6	37	0	13	0	20	15	26	0	31	8	25	51	2
20			*d*	0	8	0	8	3	4	0	0	0						2	[illegible]	[illegible]
20			*e*	1	4	0	0	8	0	0	0	3	2	6	0	5	0	7	11	0
20		ii	*a*	2	3	0	0	20	3	0	0	4	10	6	1	5	3	10	16	4
50			*b*	0	9	1	1	5	1	3	0	0	2	4	0	3	5	3	7	0
50			*c*	0	23	8	9	18	2	3	0	4	18	12	1	14	0	10	4	2

Key to Numbers

Localisation
1 No loc.
2 Loc. in neighborhood of screen
3 Loc. at screen
4 Loc. before screen

Texture and Dimensionality
1 Typical film
2 Thick film
3 Thin film
4 Almost surface
5 Almost bulk

Shape
1 No shape
2 Indefinite shape
3 Shape divergent from stimulus
4 Definite shape: contours blurred
5 Definite shape: contours sharp

Objectivity
1 No obj.
2 Partial obj.
3 Full obj.

Texture and Dimensionality

We treat texture and dimensionality together because the *O*s invariably reported them together; as one *O* (H) has it, "Texture and dimensionality seem bound up together." That is to say, the textureless film was dimensionless, the textured surface was bidimensional, and the tridimensional

bulk was "glassy." The surfaces and bulks, always merely "flashes" of surfaces and bulks, depending on a perceptive attitude and disappearing with any shift of this attitude, fall into only two general classes. The films, on the other hand, remaining films when critically regarded, show three more or less distinct varieties. Their differences were apparently not of a perceptive, but of an attributive kind, and the *Os* likened them to the differences in volume of tones.

(1) *Typical Film.* First, we have the typical pretextural, predimensional film which is neither thick nor thin, which 'invites' one to look into it, which is luminous and soft, and which suggests both tridimensionality and bidimensionality.

Bi (I ii *b* 1)[5] "Hints strongly at tridimensionality, yet not tridimensional; verges on surface, yet not surface. No substance in the color itself." Bi (I ii *a* 2) "After-image all of a piece with the visual field;. . . whole field lively, kinetic." H (I i *b* 1) "A bit of bright glowing color, not surface, nor solid, nor liquid, more nearly gaseous." H (I i *b* 2) "Luminous cloud effect, uniform medium that suggests depth, translucent. Light penetrates through from behind." H (I ii *a* 1) "No grain or texture to it. Like a film of translucent color; just like a self-luminous patch of color with hint of depth." Br (I i *a* 6) "Frontness vague and indefinite, yet looks softish, and suggests flatness in a way, and also depth." An (I i *a* 2) "Cloudy, with a sheen of brilliancy to it. Tridimensional, not as a body, but as a cloud." Am (I ii *b* 4) "Misty, as if you could look into it." Gl (I i *a* 6) "Soft and yet compact, not surface, not deep; . . . a sort of cloud that suggests depth." Gl (I ii *a* 3) "Think of it as having depth because it is a haze." T (I i *a* 5) "Filmy, downy, soft, cloud-like, more or less density." N (II i *b* 4) "Filmy, as if made of colored gas, and you can see into it; cloudy, as if the figure extended beyond itself." R (II i *b* 10) "Very soft, cloudy, semi-transparent." R (II i *b* 11) "Cloud-like, soft effect."

(2) *Thick Film.* Sometimes, most frequently with projection (*a*), the films, while definitely not tridimensional, were definitely thick and massy.

H (I ii *a* 3) "Had a strong suggestion of volume to it. Slightly less translucent." H (I i *a* 4) "Seems massive somehow." Bi (I i *a* 3) "Thick and bulky, simple attributive character of volume, without dimensionality." Br (I i *a* 5) "Seems more massy and solid." Am (I i *b* 4) "Appeared thick." Gl (I ii *b* 1) "Green, duller than red, and not so luminous, but seems to have more thickness." T (I i *b* 6) "Density was quite heavy." H (II ii *a* 5) "Greater amount of density to it." N (II i *a* 1) "Had considerable depth." R (II i *a* 10) "Heavy, having more body." Bi (II ii *a* 7) "Thick and bulky film. When bulky less transparent, but more massive, this one not glassy." Bi (III iii *a* 4) "Dense film, pseudo-bulk." Br (III iii *a* 2) "Thick, dense, soft, loose; . . . thick, but not bulky." R (III i *a* 5) "Heavy, more compact."

(3) *Thin Film.* Often, especially with projection (*b*), films were or became very thin. This type was rarer with closed eyes, because the *Os* had difficulty in abstracting the images from the visual field itself, which is a thicker film.

H (I ii *b* 3) "Very thin, very little body to it, like colored light on cardboard." H (I i *b* 2) "Was thin, yet a little cloudy, misty with a kind of depth and translucency." Bi (I ii *a* 4) "As yellow gets less in chroma it gets thinner." Am (I i *a* 4) "Don't think it had depth, but looked like auto-light in distance, yet suggested depth." Br (I i *b* 3) "Thin." T (I ii *a* 5) "Flat, but can imagine depth to it; . . . a trifle filmy." Br (I i *b* 2) "Thin and filmy, but not filmy in the sense of thick." Gl (I i *b* 6) "Would not suggest tridimensionality, but it is soft." H (I ii *a* 1) "Don't find that

[5] I refers to the experiment, ii to the series, 1 to the stimulus in the series, and *b* to the method of projecting the image.

when it grows thinner it seems nearer to surface." Bi (II i *b* 6) "Thin and dilute." Bi (II i *b* 12) "Soft, fuzzy, flat, which is really only the suggestion of flat." Br (II ii *b* 6) "Fairly flat, smooth, yet filmy." Br (II ii *b* 7) "Fairly thin, though loose." R (II i *b* 12) "Thin, transparent, like light thrown on screen." R (II i *b* 3) "Thin, flat, cloud-like."

The interpreting of a thick or thin film as surface or bulk was arbitrary, and depended upon its localization and the meaning of its stimulus.

(4) *Almost Surface.* Types (4) and (5) always accompanied an attitude which was more or less completely the attitude of the object-consciousness. We name them "almost surface" and "almost bulk" because, critically regarded, they never remained real surfaces or real bulks. They either appeared both surfacy and filmy, or both bulky and filmy, at the same time; or else suddenly, with growing criticalness on the part of *O*, changed from surface to film, or from bulk to film.

H (II ii *a* 1) "Seemed to be more nearly a surface; but I was in a perceptive attitude, trying to see how objective it could be." N (II ii *b* 3) "As long as it means 'maple leaf' it is a surface and flat; as it loses its outlines and gets into a mass of color, it takes on penetrableness and filminess." Br (III i *b* 1) "Soft surface, almost meant book." H (II ii *b* 4) "For the moment it seemed opaque, dull, flat." N (II i *b* 2) "As it fades it loses meaning and becomes less surfacy."

(5) *Almost Bulk.* H (II ii *b* 7) "At first very suggestive of a solid mass; somewhat glassy, and seemed to have real tridimensionality." H (II ii *b* 9) "Took on some characters of bulkiness; seemed more glassy and transparent." R (II ii *b* 9) "Tendency to glassiness." R (II ii *b* 8) "Green at first like a shadow; then when it meant 'house' was solider." H (II i *b* 2) "When detached from the screen a little harder, glassier, a little less internally active and slightly more translucent. Suggested depth, though still not so soft and loose. Has a tendency toward bulk." R (III i *b* 2) "When it becomes objective, it becomes more glassy and a little transparent." N (III ii *b* 3) "Difficult to say if there is a difference in texture when it becomes a cube. Have an impression of seeing through it; yet it is different from looking through glass." Bi (IV i *c* 1) "Hint of glassy." Br (IV i c 1) "Foggy and filmy, but more transparent. Don't know whether really a thinner film or actually of a more glassy and harder texture." H (IV i *c* 1) "Hint of bulkiness, glassiness." H (IV i *a* 1) "Fleeting impression of dense, bulky, glassy thing, floating."

Localization

(1) *No Localization.* As a film is predimensional and pretextural, so it has no position. With projections (*a*) and (*c*) indefinite localization was the common report; even with projection (*b*) it was not infrequent.

Bi (I i *a* 4) "Localization very uncertain." H (I ii *a* 1) "Localized out somewhere." H (I i *a* 4) "Not definitely localized." Gl (I i *a* 4) "Can't place it." Gl (I ii *b* 1) "Might have been anywhere." An (I i *a* 1) "Suspended off in space." Am (I ii *a* 1) "Seemed sunk in the dark." Bi (I i *a* 5) "Localization indefinite, near or far wherever I fixate it." Bi (II ii *a* 4) "On eye-lids colors are as indefinitely localized as the sky is." Br (II i *a* 4) "Out there." N (II i *b* 2) "Not localized." Bi (II i *b* 9) "No relation to screen; screen not seen." Bi (III i *a* 3) "Nowhere."

(2) *Localization in Neighborhood of Screen.* Because the *O* has seen the stimulus against the screen out in front, he sometimes, even with closed eyes, places the image with reference to this screen. With projection (*b*) this position is customary. In neither case can the *O* state the exact relation of image to screen; it 'was near' it or 'referred' to it.

H (I i *a* 2) "Localized out somewhere in front, somewhere near place of stimulus." H (I ii *a* 2) "Localized out in front at about the region of the screen, and then became more indefinite." T (I i *a* 4) "Localized about the same place as the stimulus." Br (I i *a* 3) "Located about the same distance as the screen." H (I i *b* 2) "At best definitely not lying on the screen, but in the neighborhood of the screen, with a tendency to go beyond and through it." Bi (I ii *b* 2) "Spot was referred to the cardboard, but not really there." Br (I i *b* 3) "Localization indefinite, but it hovered near the cardboard." T (I i *b* 4) "Localization was uncertain, but I think it was back on the screen." An (I i *b* 3) "Location appeared to be about where the object is on the screen." Gl (I ii *b* 4) "Located on screen or between me and screen." N (II i *b* 11) "Near the screen, yet really does not belong to it." R (II i *b* 4) "Within or further away than screen." Br (IV i *d* 1) "It is most difficult to say whether it is really out there in the air or not. It tends to be identified with the screen or near the screen; yet when I realize I am still fixating the point, I see it there and feel that it is transparent."

(3) *Localization at Screen.* The very thin films (Texture and Dimensionality 3) could actually lie on the screen (*b*) or darkened object (*d*) upon which the *O* projected them. As films they had no texture of their own; so that in many cases they seemed merely to lend their color to the cardboard, affecting it in no other way.

Bi (I ii *b* 4) "Appeared enmeshed in grain of screen as if it belonged to it and the screen had been tinted that way there." H (I ii *b* 4) "Like a pencil of light hitting the cardboard as if it washed over it." Br (I i *b* 2) "Tinted the gray screen; seemed that it might be just a bluish part of the cardboard." An (I i *b* 3) "Located on front screen. Appears as if projected on the screen, as if an integral part of the coloring of the screen." T (I i *b* 1) "Localized on the cardboard as a part of it." Gl (I i *b* 2) "Localized on the screen, not filmy, but like a part of the screen." Bi (II ii *b* 4) "Alters the gray screen in color and tint. Has no texture of its own, but takes on that of the screen."

Even, here, if the *O* shifted his attitude ever so slightly, he noticed that the background itself was soft. With projection (*d*), because the object was so weakly illuminated that it did not present absolute surface, this softness did not detract from the reality of the object.

H (I ii *b* 4) "Makes a difference to the screen in some way, a little dulling, obscuring, softening of the surface; it is not so distinctly seen." H (I i *b* 1) "The image steals more from the screen than the screen does from the image. The gray loses its surface-character. The image got thinner, but was still slightly lively."

(4) *Localization before Screen.* At times with projection (*b*) and always, if instructions were followed, with projection (*c*), the image stood distinctly in front of the background. Here we find an approximation to bulk, but not a true bulk,—because the image did not appear "glassy," and the tri-dimensionality showed between the color and the screen, not in the color itself.

Bi (I i *b* 3) "Located this side of gray." Gl (I i *b* 6) "Located this side of the screen." Am (I i *b* 4) "Located in front of the screen." R (II ii *b* 7) "This side of the screen." Br (II i *b* 7) "Out in front of the screen." H (II ii *b* 7) "Out in front." N (IV i *c* 1) "Stood out on shelf." B (IV ii *c* 1) "Stands out in front." Br (IV i *c* 1) "At first clearly out on shelf."

Shape[6]

(1) *No Shape.* The film which is without texture, dimensionality and localization, is also in its own right without shape. Many of the reports bear out this fact.

H (II i *b* 1) "Just a mass." Bi (II ii *b* 5) "Round, irregular shape." Am (I ii *a* 2) "Very little definite outline." An (I i *b* 4) "Became indefinite in form." Bi (I i *b* 3) "No sharp outline." N (II i *b* 5) "Irregular shaped light spot." Br (II i *a* 3) "Soft and fuzzy outline and shape indefinite." H (III i *b* 4) "Blur of purple." R (III i *b* 3) "Just a blur."

(2) *Indefinite Shape.* We purposely saw to it, by the forms of the stimuli, that some of our after-images should have contours. Frequently, though, when the *O*'s attitude was not highly perceptive, these contours were blurred and showed no detail. In Experiment I, detail was of course impossible.

Bi (I i *b* 1) "Outline indefinite, but shape circular." An (I i *b* 4) "Circular in shape, but indefinite." Gl (I i *a* 5) "Fuzzy in outline." H (II ii *a* 2) "Poor outlines, very fuzzy, not very good reproduction of details." Br (II ii *b* 2) "Outlines good, though the details of the picture are lacking." N (II i *b* 4) "A dark square." R (II ii *b* 12) "Fuzzy, no detail." Bi (II i *b* 3) "All the big points." Bi (II *b* 8) "Windows and doors, daubs of yellow, elliptical, no corners." R (II ii *a* 6) "Crescent shape with ragged outlines."

(3) *Shape Divergent from Stimulus.* Occasionally, despite our control, the shape of the image differed radically from that of its stimulus.

N (II ii *b* 4) "Looked more like a duck." H (II i *a* 3) "Rather an oak-leaf than a maple-leaf." H (II ii *a* 4) "Not quite so sharp in outline, and distorted in shape." Bi (II i *a* 4) "Shape roundish like a little camel."

(4) *Definite Shape: Contours Blurred.* Even with a more or less complete object-consciousness, if the details of a figure were clear, its boundaries were almost invariably obscured, and its outlines ragged.

Br (II i *b* 2) "Outlines fuzzy, but shape clear." Br (II *a* 12) "Very, very clear, but fuzzy." H (II i *b* 4) "Outlines good, though not as sharp as in the stimulus, in form accurate reproduction of stimulus." Br (III i *a* 1) "General bottle-shape, but fuzzy edges." Bi (II i *a* 8) "Outlines sharp, but shadowy at edges, rounding it off. The after-image never looks like an object, because it is rounded off."

(5) *Definite Shape: Contours Sharp.* Seldom, and always under a perceptive attitude, these boundaries for an instant appeared clear and then, as the image faded, became ragged.

Br (II i *b* 3) "Shape very clear, outlines at first distinct, then became more and more indistinct." R (II i *b* 8) "At first rounded and fuzzy, and then lost this and became smooth; edges became smooth and whole thing more flat. Then became fuzzy again." N (III i *b* 3) "Lines at sides were well defined, got less well defined."

Objectivity

Experiment I offers no reports of objectivity; subsequently, because of the object-character of the stimuli, these reports became customary.

[6]The observations of this paragraph are in conflict with such results as those of W. McDougall, Some New Observations in Support of Thomas Young's Theory of Light- and Color-Vision, *Mind*, N.S. 10, 1901, 52 ff. Perhaps a difference in the set or attitude of the *O*s may be responsible; it seems, as we point out later in this paper, that the after-image is subject to contamination by other modes of imagery. [Cf. W. Fuchs, Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten ("Angleichungserscheinungen"), *Zts. f. Psych.*, 92, 1923, 249 ff.,—an article which we received after the above Note was written.]

(1) *No Objectivity.* Throughout the last three experiments when an image, though it might have some position or shape, was still taken more or less observationally, we get reports of "no object-reference."

H (II i *b* 12) "No object-reference." Bi (II i *b* 12) "No objectivity." R (IV i *c* 1) "Like block of air, no texture at all, no framework, an indefinite substance." N (III i *b* 3) "No objectivity, light and dark." Br (III i *a* 7) "Dark streak, pale streak, no objectivity." N (II i *a* 4) "Took up a non-perceptive attitude." H (II i *b* 8) "Does not mean house, but then I was not in attitude for meaning."

(2) *Partial Objectivity.* Partial objectivity enters into all descriptions of surfaces and bulks which were not wholly surfaces and bulks, or which would not remain surfaces and bulks, and into the infrequent reports of sharp outlines that soon blurred. This incomplete objectivity is obviously a part of that attitude, or is one of those attitudes, which Martin did not catch and Adams[7] could not classify. We can do no better. Our partial objectivity usually manifested itself as an incompatibility between true objectivity and the filmy nature of the image. The *O*s tried to lessen the conflict or to reconcile the incompatibles by such phrases as "ghost of an object," "shadow of an object," and "object, but never a perception," but in the end had to admit that the after-image had a "personality of its own."

Bi (II ii *b* 6) "No objectivity, except that it is a little like a shadow of an object, except that it has a personality of its own." Bi (II i *b* 6) "Like the shadowy extent of an object, but too filmy to be taken for an object." H (II ii *a* 1) "It represents an object as a shadow represents an object. It seems to stand out from the background and be more nearly surface, but I was in a perceptive attitude to see how objective it could be." H (II ii *a* 7) "Conflict of apparent objectivity as against the softness and filminess of the color." H (II ii *a* 8) "Indefinite localization, filmy thing, nothing beyond it, seems incompatible with objectivity." R (II i *b* 8) "When all the colors assumed their proper place it meant 'flag.' At first it appeared rounded and fuzzy, and then lost this and became smooth; the edges became sharp, and whole thing more flat." Br (II ii *b* 3) "I look at the black line and the perceptive motive comes in, then I look into the middle and see that it is a film." N (II ii *b* 3) "As long as it means 'maple-leaf' it is a surface and flat; as it loses its outlines and gets into a mass of color it takes on penetrableness and filminess." H (II ii *b* 4) "Objectivity, but never the perception of a kettle, always image of kettle." H (II ii *b* 10) "Almost carried object-meaning, though still filmy, hazy and not tridimensional." Bi (III i *a* 1) "No more a bottle than the ghost of a man is a man." Bi (III iii *b* 2) "Like the shadow of a bottle; meaning of bottle is not the same as seeing a bottle." Br (III i *a* 2) "The objectivity of a silhouetted object." R (III i *b* 1) "Think of bottle and it seems tridimensional, otherwise flat." Br (III iii *b* 3) "Was filmy and then flashed into something cube-like. When I looked for it, it disappeared." H (III i *b* 4) "Two attitudes kept fluctuating, perceptive and descriptive; when perceptive, carried meaning of having tridimensional aspect; when as before, a fuzzy, filmy thing." N (IV ii *a* 3) "Unorganized, flat mass of yellow; then suddenly snaps into a cube-shape and becomes tridimensional and an object; it stands right up in front." R (IV ii *b* 5) "Flash of objectivity; seems to stand out at right angles; then objectivity becomes poor, means 'horse' somewhat, but only trace of tridimensionality in it."

(3) *Full Objectivity.* An after-image was a satisfactory 'object' only when the *O* 'forgot' to become critical towards its substance and outlines, or when it represented an object which could, under certain conditions (*e.g.*, lowered illumination), have ragged outlines and filmy texture.

[7]G. K. Adams, An Experimental Study of Memory Color and Related Phenomena, this JOURNAL, xxxiv, 1923, 359ff.

Bi (II ii a 10) "I let the perceptive motive have its way, and saw it as a ship on the sea." N (II ii b 8) "A house with lighted windows and doors. The object was so good that I forgot to look at the texture." H (II ii a 7) "Meant British flag against sky." H (IV i d 1) "A very good cube, the most complete cube, and solid." Br (IV i d 1) "Slipped inside and was a green cube." R (IV i d 1) "Dark cube with white frame, same as if actual cube was there." Br (II i a 12) "Like a picture seen in the twilight, with the fuzziness and softness of twilight."

Two questions remain. (1) Are the two variants of film, the thick and the thin, due simply to the mode of projection of the after-image, or does the 'density' of the film vary intrinsically, *e.g.*, with hue? There is no doubt that mode of projection has its effect (see p. 161); and sporadic cases not referable to mode of projection may result from special conditions. We prefer, nevertheless, to leave the question open.[8] (2) What are the 'almost' surfaces and bulks and the sharp outlines which quickly disappear? Do they represent intrinsic changes of the film, or are they perceptive additions? We reply that they fulfill perfectly the three conditions of the imaginal overlay; and we have evidence that overlays sometimes formed part of the original experiences. For example:

Br (II i a 12) "A flash of green hills, but this went out and left only the yellow rectangle." Br (II i a 12) "The sky seemed streaked with clouds."

Conclusions

Katz' claim for the negative visual after-image is just. No matter what its stimulus, it is, when critically regarded, a film,—textureless, dimensionless, in a strict sense shapeless, and without locality. Under the conditions of our experiments, it shows differences in 'density.' If the *O*'s attitude tends toward objectivity, other, perceptive, differences appear; the images are localized; they have, for an instant, sharp outlines and definite texture. As soon as this 'objective' attitude becomes ever so slightly critical, the localization ceases to be definite, the outlines are blurred, and the image is again essentially soft and insubstantial.

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[8] As there are preformal differences that can be communicated only in terms that suggest form (K. Goldstein u. A. Gelb, Psychologische Analysen hirnpathologischer Fälle auf Grund von Untersuchungen Hirnverletzter, *Zts. f. d. g. Neurol u. Psychiatr.*, 41, 1918, 73), so may there be pretextural differences that can be characterised only in terms that suggest texture.

THE RÔLE OF KINAESTHESIS IN MEANING[1]

By Myra Shimberg

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I. Historical Introduction

The rôle of imagery in the higher mental processes has long been a matter of contention among psychologists. Marbe's experiments in 1901 and Bühler's investigations in 1907 provided the impetus for further experimental work in this direction.[2] Marbe conducted experiments in an attempt to ascertain the conscious processes involved in judgment.[3] Bühler's investigations were entitled "Ueber Gedanken". He gave his subjects certain questions to answer and insured their thinking by asking questions that could not be answered without reflection. Then he asked for full introspections.[4] He concluded that "the most essential part of the experience is something which in content is different from anything that can, as a last resort, convey its impression in sensations;...as the essential components of our thought experiences, *thoughts alone* can be considered."[5]

Among American psychologists, the work of Woodworth is most clearly allied to that of the Würzburg school. Woodworth set some problem which

[1]From the Psychological Laboratory of Wellesley College. Communicated by C. A. Ruckmick.

[2]From the theoretical side, Mach, Ehrenfels, Meinong, Watt, Ach and Messer in Germany, and Spencer and James in England, had already attacked this problem.

[3]E. B. Titchener, *Experimental Psychology of the Thought Processes*, 1909, 81.

[4]K. Bühler, Tatsachen und Probleme zu einer Psychologie der Denkvorgänge: 1. Ueber Gedanken, *Archiv f. d. ges. Psychol.*, 9, 1907, 312.

[5]*Ibid.*, 315, 317.

[6]R. S. Woodworth, Imageless Thought, *J. Philos. Psychol.*, 3, 1906, 703 ff.

the subject was to solve promptly; and as soon as the solution was reached, or even before, he interrupted the course of the subject's thought and called for a description of the process of seeking and finding a solution. Introspection was made more reliable by calling for answers to very definite questions as "any visual picture?" "any words heard?" "any feelings of bodily movement?"[6] An example from Woodworth's voluminous reports follows: To the question "What substances are more costly than gold?" the answer came promptly "Diamonds." The subject reported that she "had no visual image of the diamond; the thought of diamonds was there before the sound of the word. . . . It is the same way in conversation; you know what you want to say, but the words come so quickly that you don't have a chance to think before you say them."[7]

Woodworth's own introspections also revealed examples of imageless thought. One of the many instances he quotes occurred while "reading a psychological book in a foreign language." In a certain sentence occurred an unfamiliar word. On rereading it he saw the meaning from the context, "yet no English or other equivalent suggested itself until distinctly later . . . It seems impossible to describe these facts without admitting the existence of other than sensorial contents of consciousness." Woodworth believes that, in addition to "sensorial elements, thought contains elements which are wholly irreducible to sensory terms."[8]

The "imageless thought" doctrine has been effectively challenged by many psychologists. Colvin vigorously opposed it: "There is . . . . a type of motor imagery which has up to date passed unnoticed. It is a type which expresses itself in more general motor attitudes and includes all sorts of kinaesthetic images that suggest the individual's relation to a situation and which are the mind stuff of meaning. . . *Thinking is not imageless*, but does not have the rich ideation of concrete visual or auditory imagery."[9] In his introspection of motor ideation, Colvin found himself using imagery that may be termed mimetic. It consisted of *motor* images and sensations that were symbols of the situation which they represented just as truly as words represent concrete reality. Whenever he introspected his *meaningful* thinking, he invariably found this tendency.[10] "May it not be, that those who believe that they have imageless thought possess this mimetic imagery but in so slight a degree that it has never been analyzed out in their consciousness?"[11]

Titchener also stands opposed to the doctrine of imageless thought. Referring to Bühler's thesis—which would also be Woodworth's—that there are thoughts without the least trace of any sort of imaginal groundwork, Titchener criticizes Bühler's experiments as leading the subjects into the pitfalls of the stimulus-error: "Bühler's observers were men of wide psychological experience, of long technical training, of undisputed ability; but they were given an immensely difficult task, in terms of a very poor method. . . . I submit that the observer was not describing his thought, but reporting what his thought was about; not photographing consciousness, but formulating the reference of consciousness to things: in a word, that he had fallen, in the case of thought, into the error which we should term the stimulus error in the case of sensation."[12]

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[7]*Op cit.*, 703 ff.

[8]*Ibid.*, 706.

[9]S. S. Colvin, Method of Determining Ideational Types, *Psychol. Bull.*, 6, 1909, 236.

[10]S. S. Colvin, *Psychol. Bull.*, 7, 1910, 58.

[11]*Ibid.*, 59.

[12]E. B. Titchener, *op. cit.*, 146.

Recently Moore[13] has summarized the investigations concerning the place of the sensation and image in perception, memory, and thought. He points out the inadequacy of the doctrine that meaning is equivalent to some form of sensory or imaginal content, but does not feel inclined to regard meaning as extrapsychological. He lays down his own thesis in the words: "Meaning itself is a mental structure, the process by which it is obtained is a function."[14] In the course of this exposition he discusses his version of "mental structure" and "mental function."

It is not unlikely that the discussion of imageless *vs.* imaged thought has significance in connection with the question of imagery aroused with other higher mental processes. There has been considerable investigation of the rôle of imagery in voluntary movement. But recently the function of the motor processes, looming so large in the behavioristic type of psychology, has also received attention at the hands of the introspectionists. The motor attitudes connected with the cognitive and conative processes have a full share of space in our present literature. More especially the part played by the kinaesthetic sensations is being examined in its bearing on the problem of meaning and in the formation of meaningful perceptions.

The rôle of kinaesthesis in psychological phenomena has been the subject of many experimental papers. Woodworth first investigated the cause of voluntary movements. "What is the cause of a voluntary movement?" is his starting point. "A purely schematic psychology" would answer that "voluntary movement" is "movement foreseen and intended. . . There must, therefore, be in the mind an idea of the movement, and. . . . this idea will consist of reproduced sensations of sensations originally produced by the movement. . . . Therefore, the cue of a voluntary movement consists of a sensorial image of the movement." "The kinaesthetic image is the distinguishing mark, the determinant, of the coming movement." The experimental observations of Woodworth, however, led him to "deny that any form of sensorial image of the movement or of its outcome need be present in consciousness in the moment just preceding the innervation."[15]

Woodworth's experiments required the subject "to make a given movement with some preliminary hesitation and to note the condition of mind that preceded the movement" with particular emphasis on what imagery appeared, *etc.* Subjects of considerable psychological training were chosen. Results, of course, showed great individual differences in the sort or amount of imagery experienced. Out of many reports, I quote the following: "Imagery of any kind scarcely occurred at all except as the result of effort to get it. Kinaesthetic imagery so got was not like the sensations of the actual movement." In a rough summary of his results, Woodworth estimates that some sort of imagery occurred in 55% of the total cases, *one-half thus showing no imagery.*[16]

Ruckmick's experiment on the Rôle of Kinaesthesis in the Perception of Rhythm investigated the contents of consciousness during the period of the presentation of rhythm. The general conclusion reached is that "whatever the material presented for rhythmization. . . . kinaesthesis was essential for the establishment of a rhythmical perception. That perception once established, however, rhythm might be consciously carried, in the absence of any sort of kinaesthesis, by auditory or visual processes."[17]

[13] T. V. Moore, Image and Meaning in Memory and Perception, *Psychol. Monog.*, 27, 1919, no. 119.

[14] *Ibid.*, 186.

[15] R. S. Woodworth, Cause of a Voluntary Movement, *Studies in Philos. and Psychol.* (Garman Commemorative Vol.), 1906, 351ff.

[16] *Ibid.*, 361.

[17] C. A. Ruckmick, Rôle of Kinaesthesis in the Perception of Rhythm, this JOURNAL, 24, 1913, 359.

Of the experiments on *meaning* and *kinaesthesis*, those of Jacobson, Ogden and Rogers seem most significant. In reply to a criticism that Cornell observers have been predisposed against the "discovery of meanings in experience and have therefore confined their introspections to the 'known mental categories of sensation, image, and feeling in which [they] have been schooled'," Jacobson submitted a series of experiments on the meaning and understanding of letters, words, and sentences. He concluded that meaning is carried by visual and kinaesthetic processes. "So far from having a predisposition against meanings," said he, "we have . . . made a systematic attempt to cultivate reports about them. . . . We find that *wherever there is meaning there are also processes*, and that the correlated meanings and processes are *two renderings, from different points of view, of one and the same experience*."[18]

In contradistinction to this experiment we have the investigation of Ogden on the consciousness of meaning. He presented each of his subjects with four series of words, the first containing concrete words, the second abstract, the third abstract and concrete, and the fourth nouns containing thirteen different classes of objects. He reported that in some of his series, especially the last, *imagery was a negligible factor*.[19] "Summarizing briefly, we may say that the instruction leads to the formation of a mental state upon the presentation of the stimulus word. This mental state, the nucleus, is the primary dispositional reaction to the word. It may evidence itself in consciousness as a generalized meaning, as more precise meaning contents of an imageless nature, or as a mere attitude of familiarity. It may also be quite unconscious, so far as introspection shows, and yet evidence itself in its control over the associated imagery and notions of meaning that subsequently appear. It is this relational control which constitutes the meaning of the word."[20] Ogden concludes by saying that, because of the complexity and variability of his stimuli, the experiences "were difficult to analyze. . . . A similar investigation under conditions of brief exposure with words or pictured objects would perhaps be conducive to a more precise analysis."[21]

Such an investigation was started, at any rate, in Rogers' Analytic Study of Visual Perceptions. Rogers used as stimuli twelve series of ten irregular inkblots presented visually. After the exposure period the *Os* were asked for a full introspective report of all mental processes occurring between the beginning of the exposure and the distraction-stimulus.[22] The following results are pertinent to our discussion. (1) Kinaesthetic sensations represented the largest percentage (36) of any particular class of mental phenomena reported. (2) All imaginal materials considered to-

[18]E. Jacobson, Meaning and Understanding of Letters, etc., *ibid.*, 22, 1911, 574-575.

[19]R. M. Ogden, Some Experiments on the Consciousness of Meaning, *Titchener Commemorative Vol.*, 1917, 80ff.

[20]*Op. cit.*, 89f.

[21]*Ibid.*, 120.

[22]A. S. Rogers, Analytic Study of Visual Perceptions, this JOURNAL, 28, 1917, 538. V. also: Auditory and Tactual Perception: the Rôle of the Image, *ibid.*, 34, 1923, 250ff. Some of her results are directly comparable to those we have just discussed. She states (258) that "the more complex the stimulus, the greater is the tendency to reproduce the direct sensation in imagery." On the other hand (263), "the images which served to elaborate the meaning of the perception lie without exception for the greater part in the background of attention." On the whole "these simple images render very definite services to the auditory and tactual perceptions; namely, those of reproduction, interpretation, orientation and elaboration" (265).

gether formed about one third (35%) of all processes, of which visual (27) and kinaesthetic (7) were most predominant.[23] (3) The kinaesthetic sensations and the auditory verbal images performed the function either of *bearing meaning* or of self-instruction *to seek meaning* in the figure.[24] In conclusion Rogers states that the results agree in general "with the view maintained by several writers—James, Bagley, Wallaschek, Lehmann, and Titchener—that the background, sensational or imaginal, is of fundamental importance to the meaning of perception. The kinaesthetic and organic sensations and the imaginal processes under our conditions have consistently formed a setting for the central visual complex and have supplied more of the derived significance than the 'directly' initiated processes themselves."[25]

Thus Rogers, contending for an intimate relation between meaning and process, is at variance with Ogden, already cited, who concluded that the meaning of a word is given in a "mental act of relating."

II. *Experimental Procedure*

This experiment on *meaning* was not intended to add material to either side of the discussion on imageless thought, though indirectly the subjects are inevitably linked. The special problem of the paper, as indicated in the title, is to ascertain the part that *kinaesthesis* plays in the search for *meaning*. One of Moore's criticisms, if valid, would have some bearing on our study. He claims that there are certain disadvantages in the use of nonsense material for the solution of questions regarding meaning. "Recall—especially the recall of nonsense syllables—is a mechanical something, and, though it does involve the higher processes, the main factor that comes out is imagery more or less obscure to which one can not attach any meaning whatsoever."[26] While we should be inclined to disagree with him in the extreme position of the latter statement, we recognized the desirability of selecting material which should be progressively more meaningful, in order to get a genetic survey of the meaning process. In our experience we have found very few *O*s who could not "attach any meaning whatsoever" to the most forbidding nonsense syllable. But we selected our material in such a way that the syllables would become progressively more complex and therefore furnish additional anchorage for meaning. The investigation followed an earlier experiment which attempted to ascertain the ease of learning Esperanto as compared with that of Danish words. The results were distinctly in favor of Esperanto. This was undoubtedly due to the fact that Esperanto abounds in words of Romance origin which were not entirely unfamiliar to the *O*s. Esperanto words were, therefore, selected as the medium in this experiment, because it seemed that they would

[23] *Op. cit.*, 540.
[24] *Ibid.*, 546.
[25] *Ibid.*, 575.
[26] *Op. cit.*, 277.

supply the link between the Ogden and Rogers experiments, and would also meet the criticism of Moore: *i.e.*, they would be unfamiliar, and yet not entirely devoid of possible intrinsic significance.

A. Problem. In the first part of this experiment, the object was to ascertain how the meaning of certain Esperanto words was carried.

B. Material. For this purpose five series of words were used. These were constructed as follows.[27]

(1) The first series (I) consisted of 20 words[28] of three syllables each. All these words were nouns. This insured the Esperanto noun-ending in "o" and the Esperanto antepenultimate accentuation, thus giving a certain uniformity and rhythm to the list. *E*, moreover, carefully excluded (*a*) all words containing a harsh combination of vowels and consonants, and (*b*) all words which had any obvious affinity to English or French words, as, for example, *agrabla* = agreeable; *aceter* = buy.

(2) Series II was intended to be more difficult, and so longer, more nonsensical-sounding words were chosen. These were all of four syllables, and were chosen in the manner described above, except that a few verbs were added so that the series was not as uniform as the first.

(3) Series III consisted of words of 2 syllables. In an attempt to make this series still more difficult, noun-stems, adjective-stems, and verbs were inserted, thus securing dissimilar endings. We found that the disyllabic words in this series presented more difficulty than words of 3 or 4 syllables in the former lists. As each short word was presented *O* was forced to take it as a whole, whereas the longer words offered the opportunity of picking out a syllable already meaningful.

(4) Series IV was easier than any of the series heretofore presented. It contained trisyllabic words, each selected with regard to its approximation to some English or equally familiar word.

(5) Series V contained short, easy words, characterized by their close approximation to familiar words. The rhythm was carefully noted, all the words being (*a*) nouns, thus ending in "o"; (*b*) of two syllables; (*c*) accented on the first syllable; and (*d*) smooth and pleasant to the ear. It is obvious, from the preceding enumeration of precautions taken, that this list was intended to be the very easiest of all.

C. Observers. The *Os* who participated in this experiment were all connected with the Department of Psychology. They were Dr. E. A. McC. Gamble (G),[29] Professor of Psychology; Miss A. Shuey (S) and Miss M. Amig (A), Graduate Assistants; and Miss D. Macomber (M), an undergraduate student in the Department.

D. Method. The *Os* worked individually with *E* in a room kept as free from distraction as possible. Reactions were recorded by requiring the *Os* to press down a finger reaction key, and the time was measured in σ by a Dunlap chronoscope in conjunction with a stop-watch, which aided in checking up the longer intervals.

The *instructions*, adapted from those of Ogden, were: "Close your eyes and hold yourself as passive as possible, both mentally and physically. After saying 'ready', 'now', I shall pronounce a word. Give it your immediate, full attention with the definite purpose of understanding the meaning

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[27]For specimen lists see *Appendix*.

[28]The number of words in the different series varied as *E* discovered *O*'s learning capacity in the allotted time.

[29]G is a highly trained *O*. She has attained considerable facility in learning nonsense syllables, with which she has been experimenting for many years.

it assumes. As soon as you are satisfied that the meaning has been grasped, react by pressing the key in front of you. Be in no hurry to react, and let your occupation with the word be as natural as possible."[30] Then *E* pronounced each Esperanto word in a quiet monotone. No hint of the English meaning of the Esperanto word was given, and care was taken that *O* should not have had previous acquaintance with it. Furthermore, no definition of *meaning* was provided. One *O* (G) demanded a further explanation, and was finally told to react when the associations, which arose, should lend their meaning to the word. None of the other *O*s raised any question, and each was allowed to solve the problem in her own way.

III. Results

A. Reaction Time. The average reaction time (in sec.) of each *O* in all five series is recorded in the following Table. The series are given in order of increasing difficulty. (Series I of G and M and Series II of M are omitted because of inaccurate timing on those occasions.)

Series	A	G	M	S
V	2.57	2.23	2.26	1.05
IV	3.01	3.13	3.09	1.05
I	9.50	——	——	1.76
II	5.96	5.90	——	.90
III	4.54	3.52	3.18	.85

A on the whole took the longest time to react. This is perhaps explained by the fact that it was one of her first attempts at introspection and that she was extremely anxious to do just as she was told.

The type of word seemed to have some effect on the reaction time. For A, G and M the reaction time decreased from Series III to V. They thus found less difficulty in reacting to the simpler, shorter words, especially when uniformly presented. The variation of S may be due to the fact that in Series II and III she almost invariably reacted to the first syllable of the word instead of to the whole, thus securing a shorter reaction time in those two series than the other *O*s.

We shall refer in more detail to these reaction time results in the analysis of content.

B. Statistical Analysis of Content. The appended Tables are intended to embody a summary of the processes occurring between the stimulus and the reaction as reported by *O*.[31] An explanation and analysis follows.

(i) In Table I the contents, reported in all five series of the experiment, are given in percentage values for each *O*. It is obvious from this Table that for every *O* kinaesthesis, especially in the form of internal speech, played a very important part.

[30] Cf. Ogden, *op. cit.*, 79.

[31] Form partially adapted from Ogden, *op. cit.*, 84.

Kinaesthesis, in fact, represented 57.06% of all images reported. Kinaesthetic and visual imagery together formed at least 85% of every *O*'s total imagery. For G and M visual imagery was predominant; for A and S kinaesthesis seemed to play the most important rôle.

Percent. Total Images Reported

	Kin.	*Vis.*
A	68.33	15.83
G	39.89	60.7
M	30.92	58.76
S	84.23	11.61

The approximate distribution of kinaesthesis in the different series, given in increasing order of difficulty, was as follows.

SERIES	A	G	M	S
V	59	24	17	82
IV	58	39	29	83
I	78	39	53	82
II	66	53	34	77
III	87	51	39	91

The *O*s do not agree at every point, but some significant generalizations can be made. With one exception (M) Series II or III or both seemed to evoke most kinaesthesis. *Without exception* Series III contained a larger percentage of kinaesthesis than Series V. Since the former was the most difficult, and the latter undoubtedly the easiest, it seems obvious that kinaesthesis must play its most important part in the striving after an elusive, difficult meaning.

It may be noted, moreover, that the *O*s who reported the greatest amount of kinaesthetic imagery agree in practically every respect: the peak of kinaesthetic imagery is reached in III, a decided waning occurs in IV and V.

(ii) Table II was compiled in the following way. The percentages were determined by the number of words in which the various contents appeared, divided by the total number of words given, thus obtaining the percent. of reactions which the *O* reported in terms of kinaesthesis.[32] This was done to ascertain whether kinaesthesis was distributed through the series or concentrated in one place.

The results show that internal speech occurred with 71 per cent. of all the stimulus words: A reported 75%, G 61%, M 57%, S 96%. Without taking other items into consideration, we

[32]Form partially adapted from Ogden, *op. cit.*, 84.

seem here to be at variance with Ogden, who states that in 67% of his experiments "imageless contents were reported."[33]

(iii) The time reactions of a certain group of words were also studied. From the series in which, according to Table I, *O* reported least kinaesthesis, and from the series where she reported most, some words were taken. These were not intentionally selected, but the first or last few words, in order, were utilized. The results of S were omitted because in her case kinaesthesis was so predominant that the contrast between kinaesthetic and non-kinaesthetic introspections could not be made.

We observed that, with a very few exceptions, in the cases where meaning had been attained without any accompanying kinaesthetic processes, the interval between the stimulus and the response was *less* than 3 sec. In those cases, on the contrary, where introspective reports revealed kinaesthesis, the reaction time was seldom less than 3 sec., and sometimes as much as 29 sec. It seems fair to conclude, therefore, that when the reaction comes quickly kinaesthesis is usually not present, but in longer, *i.e.*, more difficult reactions it is an almost invariable concomitant.

(iv) While internal speech was undoubtedly the most frequent type of kinaesthetic process reported, it was by no means the only form of kinaesthesis present. The following table demonstrates this fact.

Percent. of Kinaesthesis Excluding Internal Speech

O=M

V	IV	I	II	III
3.8	2.7	0	5.8	22

Here again it may be noted that the kinaesthetic processes were most predominant in the most *difficult* series, III.

(v) Affective processes were sometimes reported, especially by M, but were of minor importance, since they usually accompanied the reaction, and were merely expressions of satisfaction or disgust at the response.

(vi) Sensations other than these kinaesthetic processes were seldom noted in the case of A, M and S. G, however, reported a suspension of respiration in the attempt to secure meaning. This was most marked in Series V (30%), where few kinaesthetic processes were reported. For example:

[33]*Ibid.*, 85.

G IV 15 *Sufokis*[34]
6 *Suffocates*
Repeated *sufokis* IS, and had Vv image of it. Then there was a period of questioning, during which I took two short inspirations. Then came both IS and Vv (in colors)[35] *suffocate, suffocates.*

G V 4 *Onklo*
1.2 *Uncle*
Vv image of uncle accompanied by an expiration.

G VI 8 *Arko*
5.4 *Ark* or *Arch*
Hesitation. Vv arko, then doubt as to whether *E* had said *ako* or *arko.* Vv *ark.* V image of blackness. Pushed breath out in a sort of expiratory pant, and said *ark.*

(vii) Several times *O* reported "conscious attitudes" or *Bewusstseinslagen.* This is the only approximation to Ogden's relational control which in itself "constituted the meaning of the word." But these "conscious attitudes" were usually analyzed out by the *O*s themselves, as for example:

S I 8 *Lerteco*
.8 *Lateral*
Conscious attitude: "Think of something." Feeling of anxiety. Strain in eyes and throat. Awareness kinaesthetically of position of body. IS *lateral,* simultaneously muscular relaxation, and a feeling of pleasantness.

G IV 8 *Kaskado*
1.4 *Cascade*
Vv image (in colors) *cascade.* VC image of *cascade* accompanied by a feeling of assertion. The feeling of assertion is symbolized by a catch of the breath and a contraction of the stomach.

C. General Analysis of Introspective Results. Most of the results have been elaborated in the preceding sections. A few of the introspective reports themselves, however, follow:

A III 7 *Kruta*
1.4 *Crouton*
A image of *crouton.*

M V 6 *Boato*
.97 *Boat*
Vv *boato.* VC image of rowboat.

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[34]In the introspective reports the following form is employed:
1st line—Observer; No. of Series; No. of Stimulus word; Stimulus word.
2nd line—Reaction time in sec.; English word = reaction word.
3rd line—Introspection.
A = Auditory; C = Concrete; IS = Internal Speech (auditory, verbal kinaesthetic imagery); K = Kinaesthetic; V = Visual; ss = sensations; v = verbal.

[35]G has synaesthesia, and sees all words in varied colors. This perhaps accounts for the predominance of visual-verbal images in her reports.

A V 2 *Dio*
God
V image *Deo*, and A image of *God*.

M V 19 *Kampo*
1.31 *Tent*
Vv *campo*, and Vv image of *tent*.

The above introspections are selected because they are a fair sample of cases in which no kinaesthesis at all was reported; *i.e.*, short reaction time, little imagery, suggestion of incompleteness. It will be noticed that three of the four samples were chosen perforce from Series V.

Representative reports from all four *O*s are:

S I 7 *Ferdeko*
1.2 *Dago*
After *E* said *fer* (1st syllable of word), I felt anxiety to know what was coming. Doubt, hesitation, expressed in muscular tenseness. "Would second syllable bring meaning?" Repeated *ferdeko* IS. Then IS *dago*. As word came, had V image of an Italian.

S IV 3 *Cevalo*
1.8 *Chase*
An image of *E*'s word. Then IS *chase*. Until meaning came, there was tension in muscles and K ss. of strain in arms. With meaning came relaxation and feeling of pleasantness.

S I 1 *Skatalo*
3.4 *Scot*
K. ss; strain around eyes. VC image of hand on key. Anxiety symbolized by strain in eyes. Then IS *scot*. After the K image of half-formed word in throat, said *scot*.

S V 5 *Floro*
.88 *Floral*
IS *floro* and *flordalia*. VC image of room in own home, and attempt to make *flordalia* sensible. The elusive familiarity of this word was accompanied by a feeling of unpleasantness and K ss of strain in eyes. Then IS *floral*. After reaction came VC image of a *floral* shop.

The predominance of kinaesthetic processes in the above introspections is obvious. S almost invariably reported strain and tenseness in the period between presentation of the word by *E* and her reaction. Meaning, when it came, was expressed usually in internal speech and was accompanied by a muscular relaxation and some affective feeling.

A V 20 *Vazo*
1.8 *Vase*
K image of position. IS *vazo*. IS *vase*, with vague realization that some people pronounce the *a* broad.

A III 3 *Skurgis*
4.87 *Scourge*
Repeated *skurgis* IS. Then IS *scourge*. This was accompanied by K ss in neck, and K image of position of body, and thrusting forth of head.

A IV 2 *Babili*
1.8 *Baboon*
IS *baboon.*

A II 7 *Atvalora*
2.2 *Ad Valorem*
IS *ad valorem.* Feeling of satisfaction.

A usually reported IS. Her introspections were usually very short, as in examples 3 and 4 above.

M III 13 *Pakas*
3.24 *Package*
Vv *pakas*, repeated IS 1st syllable of word and added *age*. Pressure in arm. With reaction came VC image of brown paper *package*.

M V 1 *Akto*
1.46 *Act*
Vv akto, latin word. Then IS *act*.

M V 8 *Nego*
1.79 *Snow*
Vv image *nego*. Vv image *neige*. Then IS *snow*. With reaction came VC image of white *snow*.

M IV 8 *Kaskado*
2.77 *Cascara*
Vv image *kascado*. Then Av image of *cascara*, and VC image of a pill. After reaction, came VC image of a cascade.

M had a great deal of visual concrete imagery which often accompanied the meaning of the word, especially if kinaesthesis was entirely absent, as in example 4 above.

G III 12 *Sultro*
2.17 *Shoulder*
V perception of apparatus. Inspiration. Tension in abdominal muscles. Felt as if I were sticking my stomach out. Then Vv (in colors) *shoulder*, with fragmentary IS.

G V I *Akto*
Eight
IS and Vv imagery of *ako*, *eight*, *octave*, *akto*. IS esperanto. Probably was word for *eight* in Esperanto. Reaction, accompanied by decided expiration.

G I 1 *Skatalo*
Darkness
Vv image of *skatalo* (blue and green colors). IS *scot*. No sense. "Shall I say it?" Then came Greek word for *darkness*, *skotos*. Settled down feeling, with muscular relaxation.

G II 1 *Kokcinelo*
14.4 *Cock*
Regarded word as would nonsense syllable. Vv (in colors) *cock*, and *punchinello*. Dubious feeling as to which was correct meaning. IS and fragmentary Vv "probably little *cock* is real meaning." Reacted. Before stimulus word came, had a kind of tickly or breathless sort of feeling located on left side.

G had very vivid visual-verbal imagery, visualizing in color practically every word in every series. The visual-verbal images were often accompanied by internal speech. G also often reported, as above, abdominal, respiratory sensations, etc. When meaning came, *G* usually reported expiratory pant, as has already been noted.

IV. Supplementary Experiment

A. Introduction. This experiment is supplementary to the original experiment already described. The fact that Series III was often more difficult than were I and II was probably due in part to the circumstance that, in the very long words of the latter series, *O* usually seized the first syllable, and gave a meaning to it, instead of to the word as a whole. In an effort, therefore, to make *O* grasp and give meaning to the word in its entirety, the following experiment was tried with the same *O*s under the same conditions enumerated above.

B. Material. E prepared three series[36] containing two lists of 10 words each with their English equivalents. The first list in each series consisted of very easy trisyllabic Esperanto words, the English equivalents in one case being disyllabic and in the other two series monosyllabic. The second list in each series consisted of difficult trisyllabic words and their English monosyllabic equivalents.

C. Procedure. The following instructions were read to the *O*s:

After saying "Ready", "Now", I shall read you a list of ten pairs of words, the second of the pair in each case being the meaning of the first, as: *domus-house*; *chat-cat*. Listen attentively to the series with the definite purpose of grasping and remembering the meaning of each word. Five minutes after the first reading, I shall repeat the series, and allow you to supply the given meaning of each word.

Then to the beats of a metronome (one syllable every sec., with 3 sec. between the words) the list was read to the *O*. After 5 min. rest, during which *O* was instructed not to think of the words just learned, the Esperanto words were again presented in a different order, to prevent serial associations, and *O* was allowed to supply the English equivalent, if she could, and in any case to give her complete introspection. After this was accomplished, the second list was presented in the same manner.

D. Results. In such a short experiment, a statistical analysis of content would of course be insignificant. The following results, however, as far as they go, seem to bear out what has already been noted in the previous experiment.

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[36]See *Appendix*.

(i) Internal speech was very prominent, both in fixing the word when heard the first time, and in bearing meaning the second time. The following introspections bring this out:

S I A 8 *Salono* [*parlor*][37]
1.41 *parlor*
First time I repeated IS *salono*, then IS *parlor*. Tried to connect them. Effort to find synonym. Tension of muscles.
2nd time. IS salono, salon, *parlor*. Reaction.
S I B 1 *Pokalo* [*cup*]
1.4 *cup*
1st time repeated IS *pokalo*. Strain in arms and elbows and forehead. Tension in muscles of back. Said to myself (IS) *k*al-*cup*."
2nd time IS *k*al-*cup* with emphasis on the first letters.
M II B 9 *Vegeti* [*grow*]
3.78 *grow*
Vv image *vegeti*. Repeated twice IS. Feeling of familiarity. Then Vv and IS *vege*. Av image *vegetable*. Then IS *vegetable*, and immediately said *grow*.
S III A 10 *Nebula* [*mist*]
1.21 *mist*
1st time, when *E* presented *mist* as meaning, I said IS "*mist*, not cloud."
2nd time IS *nebulous*. Then VC image of *mist* or grey fog. This came perhaps simultaneously with reaction.
A III A 1 *Esperi* [*hope*]
1.4 *hope*
1st time associated IS *esperi* with Esperanto.
2nd time repeated IS *esperi*. Then, still in IS, came *hope*. Reacted.
M III A 9 *Promeni* [*walk*]
1.2 *walk*
Vv image *promeni*. VC image of lady dressed in old fashioned clothes, on sunny *walk*. Then IS *walk*.
M I A 10 *Fabelo* [*story*]
1.4 *story*
Vv image *fabelo*. Then IS and Vv image *fable*. Immediately said *story* without imaging it beforehand.
M III B 8 *Klinigi* [*bow*]
polite
1st time before second word was presented, said IS *clinic*. VC image of clinic. After second word, thought "what a funny connection." Then visualized people in clinic bowing to each other and being very polite.
2nd time IS *clinic*. Then came memory image of people in clinic bowing. IS *polite*, and reacted.

(ii) Here, however, as in the other experiment, meaning, it seems, can be carried non-kinaesthetically, especially when easily secured. This is well brought out in the case of G, who because of her long practice in associating words had little difficulty in remembering the meaning of 20 words. Some of her introspections follow:

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[37]The form of these introspective reports is similar to that of the main experiment, *except* that each Esperanto stimulus word is followed in brackets by its English equivalent.

I B 9 *Amelo* [*starch*]
.95 *starch*
Vv image of *amelo*. Reacted, saying *starch*, and afterwards or simultaneously had Vv image of *starch*.

II A 6 *Kompoto* [*jam*]
1.2 *jam*
Reacted immediately and simultaneously visualized (VC) dish of compotes.

III A *Batali* [*fight*]
1.2 *fight*
Vv image of *batali*. Vv battle. Then said and saw (VC) *fight*, simultaneously.

III B 9 *Elporti* [*stand*]
1.4 *stand*
Vv image of *elporti*. Then said *stand*, and saw simultaneously a figure standing. (VC imagery.)

It will be noticed that the above reaction times are very short, as has already been concluded is usually the case when kinaesthesis is absent.

(iii) As the words were more familiar in the second experiment than in the first, it is not surprising that kinaesthesis did not play so important a part, the answering word being often on the tip of *O*'s tongue, so to speak. Though bringing no new facts to light, this experiment, however, seems to strengthen the conclusion already reached. Even under conditions just enumerated, imagery still seemed to play an important part in securing meaning. Out of 180 cases in this supplementary experiment, there was only one instance where *O* did not specifically report imagery.

V. Conclusions and Summary

The dissimilarity, then, between our results and those of Ogden is, of course, obvious. Ogden reports that "in 88 experiments (67.6%) imageless contents were reported. Deducting 6 cases in which concrete visual imagery also appeared, seven in which the meaning of the word was associated with an object present to the senses, and two in which there was definite reference to the direction of an object and therefore a possible kinaesthesis, we have remaining 73 experiments (56.1%) in which no imagery was detected by the observer."[38] In our experiments the number of such cases was found to be practically negligible. In fact, in the supplementary experiment, there was only 1 case out of 180 in which the *O* did not specifically report imagery.

This finding is similar to that of Rogers, who reports that the results of her experiments on perception "agree in general with the view maintained by several writers—James, Bagley, Wallaschek, Lehmann, and Titchener—that the background, sensational or imaginal, is of fundamental importance to the

[38] *Op. cit.*, 85.

meaning of perception. The kinaesthetic and organic sensations and the imaginal processes under our conditions have consistently formed a setting for the central visual complex and have supplied more of the derived significance than the 'directly' initiated processes themselves."[39]

The conclusions of Jacobson, while doubtless more far-reaching than ours, at least agree with our main contention. He found that "the meanings of the stimulus-words were thus carried by visual, auditory, and kinaesthetic processes; or to speak more precisely, the meanings which these processes bore were the meanings of the stimulus-words, in so far as the latter were consciously realized."[40].

As to the nature of the images which play so great a part in the search for meaning, we notice that kinaesthetic images are most prominent. Kinaesthesis, in fact, represented 57% of all images reported. While many forms of kinaesthesis were represented, internal speech occurred most frequently, with 71% of all stimulus words. We did not find, however, that kinaesthesis was necessary to meaning. On the contrary, meaning was sometimes carried by means of both auditory and visual imagery. This is quite in accord with the findings of Ruckmick who stated that "there may be a perception of rhythm without accompanying kinaesthesis, in terms of (*a*) visual imagery, or (*b*) auditory imagery, or sensation."[41]

However, wherever reaction times were long, and the search for meaning was labored and difficult, kinaesthesis was an almost invariable concomitant. It is significant to note here that, in the conclusion to her paper on the analysis of perception, Rogers states that while "we should not go so far as to contend that this 'context' is the meaning, since the process itself is always to be considered as distinct from its function, . . . nevertheless, as meaning becomes more elaborate, the accessory processes acquire increasing importance."[42]

Summarizing briefly, the following tentative conclusions may be stated.

(1) Images play an extremely important part in the search for meaning.

(2) There are individual variations as to the amount and kind of kinaesthesis present, but in some form, and especially as internal speech, it is a noticeable constituent.

(3) Meaning, however, may be carried without any kinaesthesis. In this experiment it was reported as carried in both auditory and visual images.

(4) Kinaesthesis almost invariably accompanies the longer reactions, and a more labored search for difficult and complex meanings.

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[39]*Op. cit.*, 575. [40]*Op. cit.*, 564. [41]*Op. cit.*, 359. [42]*Op. cit.*, 575

APPENDIX

Sample Words from Series Used in Part I

Series	I	II	III	IV	V
(1)	Skatalo	Kokcinelo	Nagos	Agrabla	Akto
(2)	Monaho	Bestkusejo	Fremda	Babili	Dio
(3)	Franajo	Glicirizo	Skurgis	Cevalo	Ruso
(4)	Faceno	Enmiksigi	Glavo	Depeso	Onklo
(5)	Saliko	Proscripcii	Varbi	Festeno	Floro
(6)	Hirundo	Krutegajo	Saumo	Identa	Boato
(7)	Ferdeko	Atvalora	Kruta	Cagrenas	Vino
(8)	Lerteco	Punktokomo	Banas	Kaskado	Arko
(9)	Agado	Tritikajo	Peza	Oferis	Silko
(10)	Frakseno	Malkunigi	Senco	Biciklo	Pluvo

Part II

Series I

	List A		List B	
(1)	Tapiso —	Carpet	Pokalo —	Cup
(2)	Angulo —	Corner	Balai —	Sweep
(3)	Bileto —	Ticket	Aleo —	Walk
(4)	Pasero —	Sparrow	Makulo —	Spot
(5)	Citrono —	Lemon	Sitelo —	Pail
(6)	Mateno —	Morning	Oscedi —	Yawn
(7)	Insulo —	Island	Forniko —	Ant
(8)	Salono —	Parlor	Degelo —	Thaw
(9)	Argento —	Silver	Amelo —	Starch
(10)	Fabelo —	Fable	Baleno —	Whale

Series II

	List A		List B	
(1)	Esperi —	Hope	Elspezi —	Spend
(2)	Batali —	Fight	Kuraci —	Treat
(3)	Aceti —	Buy	Lacigi —	Tire
(4)	Elekti —	Choose	Remuri	Pad
(5)	Konspiri	Plot	Maltrafi —	Miss
(6)	Ripozi —	Rest	Aldoni —	Add
(7)	Dividi —	Share	Dorloti —	Pet
(8)	Cagreni —	Grieve	Klinigi —	Bow
(9)	Promeni —	Walk	Elporti —	Stand
(10)	Deziri —	Wish	Trastreki	Cross

Series III

	List A		List B	
(1)	Amiko —	Friend	Maniko —	Sleeve
(2)	Glacio —	Ice	Kolombo—	Dove
(3)	Infano —	Child	Abio —	Fir
(4)	Cevalo —	Horse	Languo	Down
(5)	Almozo —	Alms	Mentono	Chin
(6)	Kompoto	Jam	Kulero —	Spoon
(7)	Linio —	Line	Leporo —	Hare
(8)	Biero —	Beer	Paliso —	Stake
(9)	Objecto —	Thing	Vegeti —	Grow
(10)	Nebulo —	Mist	Timono —	Pole

Table I

Observer	A					G					M			
Series	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV
Class	%	%	%	%	%	%	%	%	%	%	%	%	%	%
Concrete														
Visual	14.2	20.8	4.1	4.1	5.8	6	5.8		8.6	6.1	11.1	23.5	24.4	18.9
Auditory														
Kinaesthetic Processes														
General		4.1	8.3		2.9	6	2.9	5.4	8.6	4			4.4	
Eyes													4.4	
Ears													4.4	
Throat												2.9		
Lips		8.3		8.3										
Tongue														
Mouth	7.1			8.3	5.8		5.8							
Neck			4.1										4.4	
Head			4.1									2.9		2.7
Arm				4.1		3	5.8	2.7		2			4.4	
Verbal														
Internal Speech	71.4	54.1	70.8	37.5	50.0	27.2	38.2	43.2	28.2	18.3	53.8	28.4	17.3	21.6
Visual				8.3	17.4	54.4	41.1	48.1	52.1	69.3	19.2	38.2	30.4	43.2
Auditory	7.1	12.5	8.3	29.1	17.4						15.3	5.5	4.4	13.5

Table II

Observer	A					G					M			
Series	I	II	III	IV	V	I	II	III	IV	V	I	II	III	IV
Class	%	%	%	%	%	%	%	%	%	%	%	%	%	%
Concrete														
Visual	18.1	33.3	1.9	9.	5.0	14.2	18.1		26.6	8.6	21.4	40.0	58.3	58.3
Auditory														
Kinaesthetic Processes														
General		8.3	18.0		5.0	14.2	9	14.2	26.6	8.6			18.3	
Eyes													18.3	
Ears													18.3	
Throat												13.3		
Lips		18.8		25.0										
Tongue														
Mouth	9.0			8.0	10.0		18.1							
Neck			9.0										18.3	
Head			9.0									6.6		9.1
Arm				9.0		7.1	18.1	7.1		4.3			18.3	
Verbal														
Internal Speech	90.0	83.3	100	55.5	65.0	57.1	100	92.8	66.6	21.7	85.7	60.0	58.3	66.6
Visual			1	18	25	71.4	90.0	92.8	93.3	100	42.8	36.6	91.6	100.0
Auditory	9.0	16.6	18	41.8	25						28.5	13.3	83.3	41.6

COLOR TESTING AND THE PSYCHOLOGY OF COLOR

By George Henry Taylor, Railway Medical Officer, Sydney, N. S. W.

Color testing is a factor of importance in the elimination of the unfit in a Railway Service. As my experience of color testing is practically confined to my service as a Railway Medical Officer, a period of nearly sixteen years, my remarks will be founded upon that experience, and as far as possible upon personal observation.

The evolution in the method of color testing in this State is instructive. In 1886 a test for color vision consisting of colored wools was introduced. I have, however, been unable to obtain particulars of the test. When the Railway Commissioners took office in 1888, the then Chief Commissioner (the late Mr. E. M. G. Eddy), who had been an officer of the London and North Western Railway, introduced as a test for color and form vision a card on which were printed four colors, red, green, yellow and blue. Each colored portion measured about $2\frac{1}{4}$ by $1\frac{3}{4}$ in. In the centre of the card were a number of black dots to be used for testing form vision. The examinee was placed at a distance of 15 ft. from the card and was asked to name two colors, the remaining colors being covered by a piece of cardboard. In 1892 the British Board of Trade forwarded for the information of the directors of the various British Railways a copy of the report of the Committee appointed by the Council of the Royal Society (Lord Rayleigh's Committee) and asked for their (the directors') criticism. The report presented to Parliament by the Board of Trade in 1894 shows that the Secretary of the Lancashire and Yorkshire Railway forwarded to the Board of Trade a "copy of a card which is used in the testing of this Company's servants in the matter of color vision." The card, which was reproduced in the report, was identical with that introduced in New South Wales in 1888. In the description and instructions printed in the Board of Trade report appears the following: "Modified after the regulations issued from the Horse Guards by Professor Longmore in 1868." Incidentally, the Secretary of the Lancashire and Yorkshire Railway, in his letter to the Board of Trade, stated: "We find that there are very few cases indeed of color blindness, in fact during the last ten years only one or two men have been found deficient in this respect."

In 1902 my predecessor (the late Dr. G. P. M. Woodward) recommended that the test for color vision should be by means of colored glasses as follows: red, green and purple, as used in

signals, and also a yellow glass. These were placed in a small frame having apertures about $2\frac{1}{2}$in. in diam., made in such a manner as to admit of the glasses being changed by hand, in order to prevent the possibility of their order being learnt. Before approving this recommendation the Commissioners sought the advice of a Sydney ophthalmic surgeon, who endorsed Dr. Woodward's recommendations, and advised that the cards previously mentioned should be used as well as the colored glasses, but that only one color on the card should be shown at a time. These two methods remained in operation until 1904. In that year the question of uniform standards for vision, color sense and hearing was considered by the Interstate Conference of Railway Commissioners, and Holmgren's Wool Test and Dr. Williams' Lantern Test were adopted as the tests for color sense. These tests were brought forward by the then Chairman of the Victorian Railway Commissioners (Mr. Thomas Tait), and were similar to those in use on the Canadian Pacific Railway, of which Mr. Tait had been an officer prior to his appointment in Victoria. The introduction of these tests caused a considerable amount of resentment amongst the employees in the Railway Service, as they resulted in the discovery of several men with a defective color sense. The confusion colors of Holmgren were looked on with suspicion, and eventually the Commissioners decided to replace the Holmgren test for men already in the Service by a test of "ability to select correctly from six shades each of red, green, purple and yellow wools." An examination of a large number of color-blind and color-normal persons by this wool method failed to disclose to the examiner any distinction between the color-efficient and color-inefficient, and it has long since been discontinued in this Service. The Williams Lantern Test was retained.

Some years of experience with the Williams Lantern showed that, owing to the colors being exhibited in the one sequence, it was possible for a person with a defective color sense who had been trained to an exact knowledge of his defect to pass an experienced examiner as having normal color sense, when the lantern was used alone. As a result of consultations between the Signal Engineer of the Department and myself, certain alterations were made in the Williams Lantern, which permitted the examiner to vary the colors exposed, both as regards sequence and combination, as desired.

The Holmgren Wool Test was retained as the test for applicants for employment until 1915. During that time about 58,000 persons were examined, and I was convinced that in a sensible percentage of cases its findings were not correct. Its application was slow and tedious, and it had an exhausting effect on the officers who conducted the examinations.

In 1915, on my recommendation, Stilling's Plates were adopted and used in conjunction with the Modified Williams Lantern as a test for color sense for persons entering the Service.

In 1922 the Interstate Conference of Commissioners adopted Stilling's Plates together with the Edridge Green or Modified Williams Lantern as the test for color sense on all Australian Railways.

I am satisfied that a trained and observant color-blind person may remain undetected in one examination by the Modified Williams Lantern and the Edridge Green Lantern; but I am also satisfied that a person who can pass the Modified Williams Lantern, the colors in which are restricted to red, green and white, without error, and who also satisfies a trained examiner that he is efficient in 1, 2, 3, 4, 5, and 8 of Stilling's Plates, although he may still have an undetected defect, is safe as a railway man under any conditions in which color sense is a guide.

What follows is an example of the unconscious variation through experience in the mind of an examiner in regard to efficiency in color testing by the present method. Out of many thousands of men examined for entry into the Railway Service, the percentage of color failures since 1916, when the method was the Modified Williams Lantern and Stilling's Plates, is:

Year	Failed Stilling & Lantern	Passed Stilling, Failed Lantern	Failed Stilling Only	Total
1916	6.56	2.31	.04	8.93
1917	5.26	1.15	.13	6.55
1918	5.46	1.12	.08	6.67
1919	4.95	.93	.08	5.98
1920	5.20	.16	.02	5.39
1921	4.89	.36	.04	5.30
1922	4.37	.32	.00	4.70

4.70 may be regarded as the percentage of failures of men entering the Service, through color defect.

My experience of color defect includes a personal interview with at least 4,000 red-green-blinds. To explain the inception of my present judgment it is necessary to go a little into detail. During the first seven years of my service in the New South Wales railways as Medical Officer, Holmgren's Wools were a part of the official color test. They were then replaced by Stilling's Plates. Candidates were first taken into the color testing room in batches of four, and there examined one at a time by the wools. After this, they were examined for visual acuity by Snellen's types; and finally, in a naked condition, for physical

fitness. Color blindness debarred a man from work only on the running lines. After a short experience, I detected a difference in the expression of face in a color-blind person from that of a color-efficient person, during my examination by the wools. The color-blind person suggested to me a listening expression, and the color-efficient a watching expression. The general impression that a color-blind face conveyed to me, therefore, as compared with the color-efficient face, was a want in its expression. I may here remark that quite a number of men, including intelligent and educated persons, who are color-blind, are quite unconscious of their defect until it is demonstrated to them. I also noticed that, when selecting the wools in the test, a color-normal person showed much more frequently a slight tremor of his hands, and when spoken to his voice was frequently not under the same degree of control as was that of a color-blind. Of course, a large proportion of the men examined were, from an academic standpoint, uneducated, the percentage of salaried and educated men being comparatively small. In ordinary conversation with a color-blind person his voice is wanting in emotional quality. When he smiles there is an absence of warmth in the smile, and it seems to disappear more quickly than does the smile of a color-normal person. It does not linger for a few seconds in the face. I had difficulty at first, as a type used occasionally to crop up in which the voice and emotional condition closely approximated the color-blind in persons who were color-normal. I ultimately found that this condition was associated with tone-deafness, and at first, even after recognizing this defect, I had great difficulty in separating the one from the other in my mind. However, I now know that the color-blind man has inflection in his voice without warmth; that it does not show an equal response in emotion to the bright side of life, and that therefore its general tone is monotonous as compared with the general tone of the color-efficient voice. The tone-deaf voice has practically no degree of inflection of tone, although it has a quality of modulation. The physical tremor is frequently present in the tone-deaf, quite as frequently as in the color-efficient, and there is more fire in the voice of the tone-deaf when he is roused than in that of the color-blind. By some observers, however, the one will be confused with the other, even when sufficiently educated by experience to recognize the color-blind type. I strongly suspect that Mendel's Law applies to the tone-deaf as it has been proved to apply to the color-blind, and that a similar percentage may be found in each case. There is an obvious psychological analogy between the two conditions. I have never found a man who is color-blind to be also tone-deaf, nor have I met a man who is tone-deaf and found him to be also color-blind. I have never yet examined a woman who was red-

green-blind, and my experience of women is very small as compared with my experience of men; but I have met one woman and one child who were tone-deaf, and it is a little singular that in each case there was a color-blind inheritance on the male side.

Quite a large number of men who are employed away from the lines on this railway are color-blind. I meet these men in my rooms when they are recovering from sickness or when they have been injured; and, as the medical history of each man is with me when I examine him each time, I see and examine him under conditions similar to the color-efficient man. I also know quite a large number of men outside the Service who are color-blind, a number of medical men, schoolmasters, artists, etc.

Some time ago I published in the Lancet and other papers the statement that stammering is much more common amongst color-efficient persons than it is in the color-blind, and this is still my opinion. Since publishing it, I have detected three cases of color-blind men afflicted with a marked stammer; but that nervous quaver in the voice which is frequently in evidence in persons who are a little excited and nervous is almost peculiar to the color-efficient.

I have met men who professed a love of color but who were color-blind, and a case I now quote will show a somewhat similar condition in regard to tone. While I was travelling in a railway train with an intelligent man I have known for some months, our conversation drifted into a discussion of music. My companion stated that he loved music, particularly high-class music, and instanced Mendelssohn's Spring Song as one of his favourite melodies. "However," he said, "my wife will not let me sing as she says I have no sense of tune, and I recognize myself that I have a very bad memory for tunes." I asked him "Are you certain that you have no sense of tune?" He said "My wife, who is a musician, says I have not." I said to him "I will whistle a tune and see whether you can recognize it," and I whistled Mendelssohn's Spring Song, which is also a favourite of mine. "Well," he said, "I think I have heard it before, but I do not know what the name of it is." I then tried him with the Dead March in Saul. He said he thought it must be a march, but he did not recognise having heard it before.

A color-blind person, in my experience, has frequently an intelligent and alert mind. He is stable in his official duty and is not of the type from which emotional firebrands are made. A color-blind to whom color is not interpreted in terms of sight may have a response to what is analogous to color in sight through sound.

THE CAUSE OF THE DISAGREEMENT BETWEEN FLICKER AND EQUALITY-OF-BRIGHTNESS PHOTOMETRY

By C. E. Ferree and Gertrude Rand, Bryn Mawr College

The possibility of the use of the eye in the measurement and comparison of light intensities has been a subject of great interest for more than a hundred years. The lack of proportionality in its response to intensity and the variation in this selectiveness of response with wave-length have thus far limited its use to a balancing or equalizing of light intensities. Without an undue amount and detail of calibration it could not be used to measure light intensities directly.

The Eye as a Balancing Instrument. Lights may be balanced either with regard to their energy content or to their power to arouse sensation. While only a limited use can be made of the eye for the former purpose, an important field is found for the latter,—the rating of lights by the eye for its own use, or photometry. However, the employment of the eye for rating lights for its own use is by no means free from difficulty. Here again a selectiveness of response to the different wave-lengths is, depending upon the method used, a serious source of trouble. Important points to be considered in this regard are variations in the intensity of the response with time of exposure and their differences for the different wave-lengths of light. The sensation rises to its maximum through an interval of time and then dies away because of a progressive loss of sensitivity, or adaptation of the eye. Moreover, the rate of rise and fall varies both with the wave-length and with the intensity of light. That is, there are a lag and a decay in the response of the balancing instrument, both of which are variable functions of the composition and intensity of the lights to be balanced. In short, the eye as a balancing instrument may be likened roughly to weighing scales which never quite attain to stability or constancy of balance when the objects or commodities to be balanced are not of the same kind. The result obtained depends upon how long the objects to be balanced remain on the scales, the stability increasing, however, with the increase of the time beyond a certain value, the time required for the instrument to give its maximum response. Moreover, to make the situation still more complicated, the effect on the value of the balance varies with the amount of the commodity present.

From the above considerations it is quite obvious that the length of exposure of the eye to the lights to be balanced is an important factor in the result that will be obtained when these lights differ in composition. The effect of time or length of exposure of the eye as a factor should not be ignored, therefore, in the comparative study and evaluation of the fitness of photometric methods for the purpose for which they are to be used, namely, the rating of lights for the work which the eye is called upon to do. Length of exposure is, we believe, an important, if not the chief point of difference between the two methods of arriving at a balance which are now the most widely used: the methods of flicker and of equality of brightness. In case of the former, a slow succession of exposures is given of a value much less than the time required for the eye to give its maximum response; and in the latter, a single exposure many times greater than the time required for the eye to give its maximum response. When the difference in the way in which the eye is used in the two methods is considered, it is not strange that a poor agreement in result should obtain for lights of different composition. Indeed it is somewhat surprising that agreement should ever have been expected. The reason for the disagreement will become clearer perhaps on a more detailed consideration of the effect of length of exposure on the eye's response.

For exposures less than the time required for the eye to give its maximum response, such for example as are used in the method of flicker, the following facts may be noted. (*a*) The rate of rise differs greatly for the different wave-lengths of light. Also the time required for the sensation to reach its maximum value is short, of the order of tenths or hundredths of a second; and the rate of deviation from equality of response for equal exposures as the sensations rise towards their maxima is rapid. Thus, when it is sought to establish a photometric balance between lights differing in composition by means of exposures of the small values used in the method of flicker, not only will the ratings of the exposed lights sustain a direct relation to the values which the sensations have reached in the individual exposures, and a balance be established which is not a true one for the longer exposures used in the equality-of-brightness method and for the exposures which the eye receives in the greater part of the work which it is called upon to do; but a considerable difference in result should also be expected for small differences in the length of exposure. That is, not only may the balance established by the method of flicker be considered an inapt one for the purpose for which the results are to be used, but it is also subject to no small amount of change by small changes in the speed of rotation of the flicker disc or other apparatus used to present the two lights successively to

the eye. The first of these points is, of course, of the greater importance; the second, the change in result with change of speed, is of consequence, however, because of the difficulty of standardizing the speed of rotation of the disc. The sensitivity of the method requires that the lowest speed be used that gives no flicker. In case the lights differ in color value, this lower limit of speed varies with the different wave-lengths for the same *O* and for the same wave-length for different *O*s, the net result of which is to affect both the amount of the disagreement with the equality-of-brightness method and the precision of the determination. Obviously, too, a difference in result should be expected from any inequality of length of exposure to the two lights, as happens in case of the method of flicker when the rotating sectors exposing the two lights are given unequal values.[1] (*b*) The rapidity of the rate of rise is strongly affected by the intensity of light. The effect of intensity, too, is different for the different wave-lengths. Even the order of ranking of the different wave-lengths as to rate of rise is changed with change of intensity. That is, for a given *O* the rate of rise at a low intensity may be faster for blue and green than for red and yellow; and at a higher intensity this order may be reversed. Intensity of light may thus be expected to be an important factor in any balance between lights of different composition with exposures shorter than the time required for the eye to give its maximum response. (*c*) The state of adaptation of the eye is doubtless a factor in the rate of rise, perhaps also field size, foveal and extrafoveal, although as yet the extent of these influences has not been systematically investigated. (*d*) Individuals differ apparently not only as to the rate of rise, but as to the relative rates of rise for the different wave-lengths.[2] For example, it may be expected that for some *O*s at a given intensity of light the sensations aroused by the short wave-lengths rise more rapidly than those aroused by the long wave-lengths, while for others the reverse of this is true; also that there are individual differences in the effect of intensity of light both on the rate of rise and on the relative rates of rise for the different wave-lengths.

[1]For a demonstration of this point see A Preliminary Study of the Deficiencies of the Method of Flicker for the Photometry of Lights of Different Color, *Psychol. Rev.*, 22, 1915, 110-162.

[2]This comment is not based upon a direct or systematic investigation of individual and group differences in the rise of sensation, but seems to be a reasonable inference from the collective results of several investigators of this and related functions. It is strongly indicated also by the individual and group results in flicker photometry, interpreted in the light of the results we have obtained with regard to the relation of the disagreement of the two photometric ratings, and the changes and reversals of this disagreement, to the rise-of-sensation curves.

For exposures longer than the time required for the sensation to reach its maximum value, such for example as are used in the equality-of-brightness method, the effect of length of exposure is of comparatively little consequence in the use of the eye as a balancing instrument. (*a*) The rate of decay of sensation is slow. The drop is comparatively rapid just after the maximum is reached, but the curve soon straightens out, becoming less and less steep as the exposure is continued. For exposures of the order of value used in the equality-of-brightness method, the course of sensation is quite stable as compared with the rapidity of change that comes before the maximum is reached. Moreover, the order of magnitude of exposure is that which the eye ordinarily receives in viewing its objects: thus the balance obtained is better suited to the purpose for which it is intended. Also the rate of deviation is slow; therefore small differences in length of exposure have comparatively little effect on the value and precision of the balance obtained. (*b*) Intensity of light has an effect on the rate of decay of sensation, but there is, so far as we can discover, no difference of any very great practical significance in the relative effect in case of different wave-lengths. The reversal, for example, in the relative ranking with reference to rate of change for the short and long wave-lengths, which occurs as an effect of intensity in the rate of rise, never takes place, so far as we know, in the rate of decay. (*c*) There are some individual differences in the relative rates of decay for the different wave-lengths; but again these differences may be expected to be small as compared with differences in the rate of rise.

If the choice were between single exposures of the length ordinarily used in the method of flicker and those of the length used in the equality-of-brightness method, there is, aside from the question of ease of making the determination, little doubt that it would be uniformly in favor of the longer exposures; for there would be just as little reason for rating lights for working purposes with an underexposed eye as there would be for using a false balance as a weighing instrument. There seems to be a belief among some, however, that in the succession of exposures occurring in the use of flicker the impressions summate to such an extent or in such a way as to overcome the unequal effect of the underexposure. To obliterate the unequal effect of the individual short exposures either the sensations must rise to their full value in the succession of exposures or the sensation which lags the most must also summate the faster by an amount which just compensates for its slower rate of rise in the single impression. The former position is out of the question; the latter, as we have shown in a former paper, can not be assumed without violating well-known laws relating to the factors which

influence the persistence of vision. It could not be held, for example, unless it were assumed that the sensation which shows the greater lag and is therefore of lesser intensity should also show uniformly the greater tendency to persist or to carry over to the succeeding impression; which is contrary to fact.

It may still be considered a point for discussion, however, to what degree it should be held that the difference in lag between the sensations aroused by the single exposures used in the method of flicker is obliterated in the succession of exposures. Broadly considered, three positions are possible with regard to this point for the rates of succession employed in the method of flicker. (1) The difference is not obliterated at all. In this case the flicker balance should deviate from the equality-of-brightness balance in direct proportion to the difference in lag for the single exposures. (2) The difference is in part obliterated, but is still present to a degree which renders the method questionable in principle for the rating of lights for the use of the eye when there is a considerable difference in color or composition. And (3) the difference is entirely obliterated, or so nearly so as to be of no practical consequence for the validity of the method. The first is the position indicated by the results to be given in this and a following paper. If there is a summating action due to the succession of exposures, and there probably is, there is no detectable difference in the effect of this action on the sensations aroused by the lights compared. That is, the difference in level to which the two sensations rise in a single exposure is not decreased by a detectable amount by the succession of exposures; for the results given by the method of flicker have been found by us to agree within the sensitivity of the judgment with the results given by the equality-of-brightness method (0.4 to 0.5 per cent for the method of flicker, and 1.3 to 2.7 per cent for the equality-of-brightness method) when the time of exposure by the latter method is made equal in value to the individual exposures in the method of flicker. In other words, the equality-of-brightness method within the limits mentioned gives the same results as the method of flicker when the eye is given the same length of exposure by both methods. This would not be the case (*a*) were the difference in the length of exposure not the cause of their disagreement in result; and (*b*) were the effect of the underexposure on the results obtained by the method of flicker obliterated by any detectable amount in the succession of exposures.

The Behavior of the Eye in Flicker Photometry. The utilization of the absence of flicker as a sensitive means for detecting equality of brightness in two differently colored impressions is based on the following principles. (1) Brightness flicker is due to a succession, at certain rates of speed, of impressions differing

in luminosity or brightness. No flicker occurs, then, when the impressions are of the same brightness. (2) The phenomenon is very sensitive to changes in the luminosity of the successive impressions. (3) Color fusion takes place at a much lower rate of succession than brightness fusion. This leaves the brightness flicker outstanding in a field uniform as to color quality, also of very low saturation owing to the degree of underexposure of the eye.[3]

All of these principles are sound as applied to detecting small differences in the luminosity or brightness of the successive impressions. For this purpose the phenomenon of flicker is excellent; but trouble arises when we try to say that lights differing in composition which arouse sensations of equal brightness for one length of exposure to the eye will arouse sensations of equal brightness for any length of exposure. For change the length of exposure for either the equality-of-brightness or the flicker method, and a different rating is given to the luminosity or sensation-arousing power of the two lights,—inconsiderable, it is true, for the equality-of-brightness method, unless an exposure is used shorter than is required for the sensation to reach its maximum. That is, change the rate of speed of the flicker disc, and a different rating is made of the two lights. Make an equal and corresponding change for the equality-of-brightness method, starting from the same length of exposure as was given by the flicker disc, and a corresponding and approximately equal change in the result is obtained. For example, select a certain rate of speed for the flicker disc, the rate at which the method is most sensitive. Determine the value of the individual exposures at this rate of speed. Set a rotary tachistoscope or other short-exposure apparatus to give an exposure equal to this value, and judge the two lights by the equality-of-brightness method. An agreement of result is obtained within the limits of sensitivity of the method. Increase or decrease the rate of speed of the flicker disc and again determine the value of the individual exposures. Set the rotary tachistoscope to give this value of exposure, and again make the rating by the equality-of-brightness method. Again, an agreement in result is obtained, but the rating by both methods is different from the previous rating. The equality-of-brightness and flicker methods agree in result if the eye is given the same length of exposure to the lights compared in the two methods; they do not agree if the eye is given one length of exposure by the flicker method and another by the equality-of-brightness method. Nor will the equality-of-brightness method agree with itself in any two

[3]As might be expected at the lower rates of succession for which the degree of underexposure is not so great, the colors show a higher saturation.

determinations if in one the eye is underexposed to the lights compared and in the other it is fully exposed as it is in the accustomed use of this method. Moreover, there is no discoverable reason why agreement should be expected in either of the above cases, once the facts about the difference in the variation of the intensity of sensation with the time of exposure of the eye are known for lights of different composition.

The method of flicker would present no difficulty as a means of rating lights with regard to their power to arouse the brightness sensation if the intensity of the brightness sensation were a regular function of the intensity of light. However, the intensity of sensation is an irregular function of three interacting variables: wave-length and intensity of light, and time of action on the eye. It is safe to predict that when the relation of the intensity of sensation to intensity of stimulus is finally and correctly written for vision, intensity of sensation will not be expressed as a regular function of one variable (intensity of stimulus) as it is in the Fechnerian formulation, but as an *irregular function of three variables: wave-length, intensity, and time of exposure.*[4] No one would think of attempting to express the responses of the selenium cell, the photoelectric cell, or the photographic plate, to the different wave-lengths of light as a function of one variable; and yet the eye is admittedly more complex in its responses than any of these reactors.

Factors which Affect the Results in Flicker Photometry. Starting from the above premise as to the influence of length of exposure on the eye as a balancing instrument, it might be instructive to inquire into the number of factors or changes in the working conditions that might be expected to affect the results of the method of flicker. These factors, so far as we have been able to discover, are as follows. (1) Wave-length or composition of light: lights of different wave-length or composition rise to their maximum value in sensation at different rates, *i.e.*, they show different amounts of lag. Lights of different composition which arouse sensations of equal brightness at the optimum value of exposure will arouse sensations of unequal brightness for the short exposures given by the sectors of the flicker disc run at the speeds ordinarily employed. (2) Intensity of light: the lag changes with change of intensity of light and the change is different in amount for the different wave-lengths. The effect of change of intensity of light on the sensations aroused by exposures of the length used in the method of

[4]Even when the wave-length and exposure are held constant, the intensity of sensation is not a regular function of intensity of light, more particularly when short exposures are used. The deviations from regularity are quite large when the eye is exposed for a time shorter than is required for the sensation to reach its maximum.

flicker is to change the relative levels to which the sensations aroused by the lights compared are allowed to rise, and thus the value of the balance which is established by the method of flicker. (3) The rate of speed at which the flicker disc is rotated: a change in the rate of speed changes the value of the individual exposures, and this changes the relative levels to which the sensations compared are allowed to rise during the individual exposures. Also this change might be expected to have, and does have, a different effect for different intensities of the same pair of lights. (4) The relative lengths of exposure given by the lights compared, or the relative values of the open and closed sectors of the disc. In practice the open and closed sectors are given the same value. However, if these values are made unequal, the levels to which the sensations are allowed to rise change correspondingly, and the relative rating of the two lights as to luminosity should be changed. For example, if an *O* is used who underestimates red and yellow and overestimates blue and green at a given intensity of light by the flicker method, a change in the value of the sectors to give a shorter exposure to red or yellow, and a longer exposure to blue or green, should cause a greater underestimation of red or yellow and a greater overestimation of blue or green. (5) Field size may be, and probably is, a factor insofar as changes in the field size may affect the relative rates of rise of the sensations aroused by lights of different composition. The effect of field size on the rate of rise of sensation presents an interesting subject for further investigation and one which, so far as we know, has not been undertaken. (6) State of adaptation of the retina: this may also be a factor, through a possible influence on the rate of rise of the sensations aroused by lights of different compositions.

Results showing the effect of the first three of the above factors will be given in later papers. Accompanying these results, curves will also be given showing the rate of rise of sensation plotted in just noticeably different steps (the time j.n.d.) for the same *O* at each of the intensities and compositions of light employed in the photometric work.

Difference in Length of Exposure the Cause of the Disagreement between the Flicker and Equality-of-Brightness Ratings. The evidence in support of this point may be summarized as follows.

(1) Our first and more fundamental evidence is the comparison of the type and amount of the disagreement between the two methods with the brightness which the sensations have been allowed to attain for an exposure time equal to that of the individual exposures used in the method of flicker. As a basis for this comparison the brightness was measured in terms of just noticeable differences from zero. A close agreement was found.

The rise of sensation curves was determined with the same apparatus, the same *O*, the same composition and intensities of light, and the same state of adaptation as were used in the flicker and equality-of-brightness comparison.

(2) The effect of intensity of light on the type and amount of the disagreement was determined, and the result was checked up on the rise of sensation curves for three of the seven intensities used. Again the disagreement was found to sustain a close relation to the difference in height to which the sensations are allowed to rise for these intensities with the lengths of exposure used in the method of flicker.[5] Furthermore, an intensity was found for each pair of lights at which agreement occurs for the two methods of photometry with the speed of rotation of the disc ordinarily employed. Above and below this intensity came underestimation or overestimation of the colored light, depending upon the color selected for comparison with the white light. No single intensity could be found at which agreement occurred for more than one pair of lights. These most favorable intensities were quite widely separated for the four pairs of lights, and the percentages of disagreement at each of these intensities for the three remaining pairs were quite large.

(3) The effect of speed of rotation of the flicker disc on the type and amount of disagreement was determined for each of the intensities of light employed. Changes in the speed of rotation of the disc change the length of exposure and therefore the height to which the sensations compared are allowed to rise. A number of speeds was used. A very considerable effect on the disagreement was found, varying with the intensity of light employed. At the intensity at which agreement occurred for the two methods at the most sensitive flicker speed, variation in speed produced its least effect, as might be expected from the small angle of deviation of the rise of sensation curves for the lights compared at their point of intersection. In some cases the closest agreement came when the speed was low, in others when it was high. The result would obviously depend upon the relation of the rates of rise of sensation for the two lights above and below the points from which the change of speed was made. In much the greater number of cases, however, the closest agreement came with the slower speeds and the longer exposures. Considering the trend of results very broadly, it may be said

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[5]A comparison of the correspondence between the two types of photometric rating and the differences in height to which the sensations have been allowed to rise for the given exposures can be made with exactness only with regard to the type of disagreement and the order of ranking as to amount. Comparisons of actual amounts of disagreement with differences in sensation level must necessarily be rough.

that, as the speed is decreased and the length of exposure increased, agreement is approached irregularly as a limit, the limiting value of exposure being that used in the equality-of-brightness method.

(4) The effect of varying the relative values of the exposure to the lights compared was determined. The effect of this unbalancing of the flicker photometer, if a disturbance in the adjustment of an instrument already out of balance can be called unbalancing, varies both with the intensity of the light and with the speed of rotation of the disc. In general, the effect of a relative decrease of the exposure to a light already underestimated and an increase in the exposure of the light with which it is compared tends to increase the amount of the underestimation of that light by the method of flicker, and conversely. It would seem that a degree of physical unbalance or ratio of open to closed sector might be found, for any given intensity of light and speed of rotation of the disc, that would give agreement between the two methods, or would even change an underestimation to an overestimation. This latter point has not yet been verified by us.

(5) Each pair of lights at three intensities was rated by the equality-of-brightness method with a value of exposure equal to that of the individual exposures used in the method of flicker. These short exposures were made by means of a rotary tachistoscope. Agreement of result was obtained within the limits of sensitivity of the judgment. In every case the flicker result fell within the small range of values that was judged equal by the equality-of-brightness method. This might well be considered the most important step in the argument. It was added as a final confirmation, not only to show that difference in time of exposure is the cause of the disagreement between the two methods, but also to ascertain whether there is any detectable effect of summation on the disagreement, due to succession of impressions given in the method of flicker; that is, whether the balance which would be made for a single exposure is modified by the succession of exposures. If agreement in results is obtained by the two methods under this condition it would seem reasonable to conclude that no considerable differential effect could be attributed to summation. The judgment by the equality-of-brightness method for the short exposures was not as difficult as might be supposed, because the color difference was very much reduced by the brevity of the exposure; nor was the sensitivity of the method reduced to any considerable extent by the shortness of the exposure.

The lights used in these studies differed greatly in composition. The colored lights were selected from the spectrum of

a Nernst filament by a slit 0.5 mm. wide, and all trace of impurities was absorbed out by means of thin gelatines placed over the objective slit. They were red 675$\mu\mu$; yellow 579$\mu\mu$; green 515$\mu\mu$; and blue 466$\mu\mu$. Seven intensities were used, ranging from 12.5 to 50 meter-candles; the field size was 1.9°. The standard lamp was a 32 c.p., 4.85 watts per mean spherical candle, carbon lamp. When lights so different in composition as these are used, there can be no doubt that there is a disagreement in result by the two methods of photometry of a type and amount that cannot be ascribed to uncertainty of the judgment. The consistent throw of the disagreement in one direction or the other, for a given *O* for a given intensity and difference of composition of light, by an amount greatly in excess of the mean variation by either method indicates clearly that there is a physiological basis for the disagreement. For example, if the disagreement were due merely to an error or uncertainty in the judgment, the throw for a given *O* would be as likely to be in one direction as the other. Or if the disagreement were due to a difference in the pattern or concept of what is equality of brightness in the presence of hue difference, as some have suggested, there should not be a complete and consistent reversal in type of disagreement for the same *O* for the same pair of lights and hue difference with no other change of conditions than change of intensity. A change or difference in the subjective pattern could account only for such phenomena as inconsistency of rating by the equality of-brightness method for the same *O* at different times, and for differences in rating by different *O*s. A consistent throw in any direction by the same *O* at widely separated intervals, and an equally consistent reversal in the direction of the throw with change of intensity, could scarcely be accounted for on the ground of unreliability or inconsistency of the judgment due to an instable or fluctuating pattern. The disagreement is unquestionably physiological, not mental,—grounded in the nature of the response of the balancing instrument, not in the evaluation of that response by the judgment. It would seem just as plausible to ascribe the Purkinje shift and other phenomena, well recognized and established as optical, to an error of judgment as this equally consistent disagreement of the flicker and equality-of-brightness methods. The facts are that we have one type of effect of change of intensity on the eye as a balancing instrument when exposed for 0.5 sec. or more, and another when exposed from .015 to .025 sec. The two effects do not run parallel. They would run parallel, or approximately so, if for a given difference of wave-length and of time of exposure the response of the eye were a regular function of intensity. That is, even with wave-length and time of exposure held constant, the

response of the eye is an irregular function of intensity, quite strikingly so when the time of exposure is less than that required for the sensation to rise to its maximum.

Individual and Group Differences in Flicker Photometry. That there is a great deal of difference in the behavior of the flicker photometer with different *O*s is well known. The most that can be hoped is that *O*s will ultimately be found to fall into rather large groups or types. This tendency to group according to type was brought out clearly in the work of Ives and Kingsbury[6] and of Crittenden and Richtmyer.[7] It has also come out strongly in the drill work among our own students during the course of several years. For the *O* used in these experiments, red was overestimated at 50 m.c., underestimated at 25 and 12.5 m.c., and agreement with the equality-of-brightness method came at 29.39 m.c. But there is no guarantee whatever that this result will be true for another *O* picked at random. A group of *O*s, however, may be found which shows roughly the characteristics of this *O*. A fair percentage of such *O*s has been found among the students of our laboratory. As yet, however, we have not had the opportunity to compare their flicker results with those for the rise of sensation. In relation to explanation, obviously a study should be made of individual and group differences in the rise of sensation. The work on rise of sensation, so far as it has been carried, seems to indicate that great individual differences may be expected.

The Rating of Lights in Relation to their Service to the Eye. The question may be raised how much lights must differ in composition before the disagreement between the flicker and equality-of-brightness ratings comes to be a serious matter in practical work. Is the discrepancy great enough to be of considerable consequence for the range of difference of composition found among artificial illuminants? Comparisons of artificial illuminants have been made by Wilde,[8] Bell,[9] Crittenden and Richtmyer[10] and others. It has not been our purpose in this series of studies to go into that question at all. Our object has been merely to make an analytical and explanatory study of the phenomenon of flicker under various conditions pertinent to its application as an indicator in making a photometric balance.

While it may be said that the equality-of-brightness method gives a rating for exposures more nearly in accord with those

[6]H. E. Ives and E. F. Kingsbury, *Trans. I. E. S.*, 10, 1915, 203.

[7]E. C. Crittenden and F. K. Richtmyer, *Trans. I. E. S.*, 11, 1916, 331.

[8]L. W. Wilde, The Photometry of Differently Colored Lights, *The Electrician*, 43, 1909, 540-541.

[9]L. Bell, Chromatic Aberration and Visual Acuity, *Electrical World*, 57, 1911, 1163-1166.

[10]E. C. Crittenden and F. K. Richtmyer, *loc. cit.*

ordinarily used in the act of seeing than does the method of flicker, and should therefore be regarded as the logical standard of photometric methods, still, as was shown in a former paper,[11] even the equality-of-brightness method does not rate lights differing widely in composition according to the power they give the eye for clear seeing. That is, it was demonstrated in that paper that spectrum lights, made photometrically equal by the equality-of-brightness method, give rather widely different acuities, speeds of discrimination, and powers to sustain acuity. Furthermore, a continuation of that study has shown that spectrum lights made equal both as to saturation and brightness also give differences in acuity, though not nearly so great. Obviously, a method based on some aspect of acuity would rate lights differing in composition more nearly in accord with their serviceability to the working eye than any method in which the ratings are made in terms of brightness. Acuity, however, is an insensitive indicator of changes in light intensity at medium and high illuminations. Perhaps the needed sensitivity could be added by using speed of discrimination as an indicator, instead of acuity as determined in the conventional way. While acuity changes slowly with intensity at medium and high illuminations, speed of discrimination changes rapidly with increase of illumination at these intensities and very rapidly indeed at low illuminations. The rating of lights in terms of their ability to give equal speeds of discrimination of a 1′ opening or less in a broken circle, or some other suitable detail, would not be a difficult or infeasible task. Nothing more would be involved in that task than indicating which way the opening was turned; and the judgment, moreover, would present no more difficulty for lights differing in color than for lights of the same color. The method too would have the very great advantage of an objective check on the correctness of the judgment.[12] Also acuity itself

[11]C. E. Ferree and G. Rand, The Effect of Variation of Visual Angle, Intensity, and Composition of Light on Important Ocular Functions, *Trans. Ill. Eng. Soc.*, 17, 1922, 69-102.

[12]If at high intensities the speed of discrimination for the size of test-object used should be less than the time required for the sensation to reach its maximum, the objectionable effect of underexposure could be eliminated by having the surface covering the test-object of the same coefficient of reflection as the test surface. This preexposure, which could be made of any length desired, would add to the time the light acted on the eye. The eye would thus not be underexposed to the light which is being rated. That is, the short exposure would apply only to the discrimination of detail at a given level of brightness, not to the brightness which would be aroused. Again, the smaller the test-object, the longer would be the exposure needed to discriminate the required detail. This principle also could be used, if desired, to guard against underexposure. The decrease in the size of the test-object, by increasing the difficulty of the task set to the eye, would, moreover, add to the sensitivity of the method for picking up differences in intensity.

changes rapidly at low illuminations. That is, a method of rating lights based on acuity at low illuminations would have a great deal of sensitivity. If, as is stated by some writers, the Purkinje phenomenon is absent in the fovea, advantage could be taken of that fact always to make the ratings at low illumination. The detail to be discriminated would always fall well within the fovea. It might be well to revive the acuity method and to see what can be done with it in terms of facts that have been brought out in recent work. Where the task set to the eye is merely the discrimination of brightness, it is clear that the rating of the lights to be used should also be in terms of luminosity or brightness; but where the task is the discrimination of the form or detail of illuminated objects, the rating of the lights in terms of some aspect of acuity would be more appropriate to the use which they are to subserve.

Theories as to the Cause of the Disagreement between the Flicker and Equality-of-Brightness Methods. Three factors have in the main been assigned as the cause of the disagreement between the flicker and equality-of-brightness ratings: the effect of intensity of light, the size of the photometric field, and the time of exposure of the eye to the lights compared. Lauriol,[13] Dow,[14] Millar,[15] Ives,[16] and Luckiesh[17] have investigated the effect of intensity of light; Schenck,[18] Dow,[19] and Ives the effect of size of field. With regard to intensity of light as a factor, Lauriol and Dow claim that the relative shift in the brightness of the different colors at low illuminations is shown by both methods. The shift for Dow, however, is more pronounced in the equality-of-brightness than in the flicker determinations. For Lauriol the shift for the different colors varies in magnitude by the two methods and in some cases in direction. Millar, on the other hand, photometering a mercury against a carbon lamp, claims that the Purkinje phenomenon is not shown at all by the flicker method at low illuminations; while Ives and Luckiesh go

[13]Lauriol, Le photomètre à papillotement et la photométrie heterochrome, *Bull. Soc. Intern. des Électriciens*, 1904, 647-652.

[14]J. S. Dow, Color Phenomena in Photometry, *Philos. Mag.*, 12, Ser. 6, 1906, 131.

[15]P. S. Millar, The Problem of Heterochromatic Photometry, *Trans. I. E. S.*, 4, 1909, 769.

[16]H. E. Ives, Studies in the Photometry of Lights of Different Colors, *Philos. Mag.*, 24, Ser. 6, 1912, 149-188.

[17]M. Luckiesh, Purkinje Effect and Comparison of Flicker and Equality of Brightness Photometers, *Electrical World*, March 22, 1923, 620.

[18]F. Schenck, Ueber die Bestimmung der Helligkeit grauer and farbiger Pigmentpapiere mittels intermittierender Netzhautreizung, *Pflüger's Archiv*, 64, 1896, 607-628.

[19]J. S. Dow, *op. cit.*, 130-134; Physiological Principles Underlying the Flicker Photometer, *Philos. Mag.*, 19, Ser. 6, 1910, 58-77.

to the other extreme, and declare that a reverse Purkinje effect is obtained by the flicker method.

Dow, a follower of the *Duplizitätstheorie* of color vision, believes that there are two kinds of flicker, a coarse flicker (rod flicker) and a fine flicker (cone flicker). The final adjustments for the photometric balance by the method of flicker are made by the fine or cone flicker. The photometric balance, then, by the method of flicker, is always in terms of cone functioning. The equality-of-brightness balance, however, is in terms of both rod and cone functioning, unless a field size is chosen which falls within the fovea. If a field size is chosen which falls within the fovea the balance is established by both methods in terms of cone functioning, and a substantial agreement in result should be expected.[20] These explanatory principles proposed by Dow, although not completely worked out and elaborated by him, may be fairly singled out, we think, as one type of theory as to the cause of the disagreement of the results by the two methods.

Space will not be taken to develop all of the arguments against Dow's conception of rod and cone functioning as the cause of the disagreement. However, since the importance of intensity as a factor has been noted by him and other writers, it might be well to consider whether its effect can be explained in terms of the rod and cone hypothesis. If the sole cause of the disagreement is in terms of a difference in rod and cone functioning, it is not readily seen how intensity could affect the disagreement by the two methods at all when a field size is chosen which falls within the fovea; for with fields of this size the balance is established in terms of cone functioning by both methods; and intensity, if it affects the result by one method, should correspondingly affect the results by the other. The effect of intensity, however, is not only readily explained as an effect of lag on the balance established by the method of flicker, but it is just what would be expected from a study of the curves for the rise of sensation for different intensities of light. Even the disagreement as to the effect of the change from high to low intensities found by Lauriol, Dow, Millar, Ives and Luckiesh is what might be expected in terms of this explanatory principle. That is, these men probably worked with different types of *O*,—Ives and Luckiesh with a type for which it might be inferred that red and yellow rise to a higher value than blue and green for the length of exposure used in the method of flicker for the lower intensities,[21] and the converse for the higher intensities. There seems to be no explanation at all for such differences in effect in terms of the Dow adaptation of the *Duplizitätstheorie* of color vision.

With regard to size of field as a factor, Schenck found that a decrease in size lowered the mean variation for the flicker method and decreased the luminosity value for all colors. Dow found that as the size of field was decreased red and yellow lightened relatively to blue and green. Ives,[22] admitting the disagreement and accepting size of field and intensity of stimulus as important causes, sought to determine whether a field size and an intensity could be found for which the methods agree. He photometered different portions of the spectrum against carbon lamps at a number of intensities and with different field sizes, and found in general that the luminosity curves obtained by the two methods differed. The difference,

[20]Also it may be pointed out that, in terms of the theory, the proportion of cone functioning would increase with increase of intensity of light for extrafoveal field sizes.

[21]For an example of this type of *O*, see M. A. Bills, The Lag of Visual Sensation in its Relation to Wave-Length and Intensity of Light, *Psych. Mon.*, No. 127, 93.

[22]H. E. Ives, *loc. cit.*

however, was less for high intensities and small field sizes than for low intensities and larger field sizes. He concludes that high illuminations (25 m.c. and above) are the most favorable to agreement.[23] Our results seem to indicate that perhaps this generalization was based on the use of too limited a range both of the higher intensities and of wave-length. Agreement was found by us for the spectrum red ($675\mu\mu$) and the carbon lamp at 29.39 m.c.; for the spectrum yellow ($579\mu\mu$) at 26.12 m.c.; for the green ($515\mu\mu$) at 11.91 m.c.; and for the blue ($466\mu\mu$) at 14.91 m.c. Twenty-five m.c., the value which he estimated his most favorable intensity approximately to be,[24] was found by us to be comparatively favorable to agreement for red and yellow (the range of spectrum used by Ives was 653 to $517\mu\mu$); but the disagreement for these colors became large again as the intensity was increased up to 50 m.c. Moreover 25 m.c. was not at all favorable for the blue and green. The most favorable intensity is the one which allows the sensations aroused by the lights compared to rise to the same level or nearly so for the short exposures used in the method of flicker. Just what this intensity is can not be predicted in advance for any *O* even for one pair of lights.

A table is appended, Table I, in which is shown in percentages the difference in result obtained by the five *O*s used by Ives at the intensity of light (250 Illumination Units) which of those used he calls most favorable to agreement by the two methods. It will be seen from this table that the disagreement is by no means eliminated. It will be seen also that for each *O* the closest agreement came at one particular point in the spectrum and fell off on either side of this, the disagreement becoming quite large as the distance from this point increased. In this table percentage overestimation is designated by + and underestimation by −.

It seems quite clear that field size and intensity can not play a very important rôle as a foundation cause of the disagreement in the results of the two methods, and that agreement can not come for lights of all compositions through a search for one most favorable field size and intensity. We have found only one way in which agreement can be brought about for lights of all compositions, namely, by giving the eye the same length of exposure by both methods. When an exposure is selected for the equality-of-brightness method equal in value to that given by the individual exposures in the method of flicker, the desired agreement alone is obtained. When, however, agreement is wanted only for a given pair of lights, the case is different. An intensity can be found that will satisfy this condition, namely, that intensity for which the sensations aroused by the two lights have risen to the same level for the lengths of exposure given in the method of flicker. Such an intensity has been found by us for each of the four spectrum lights when matched against the carbon lamp. This intensity, however, is different for each of the spectrum lights.

That time of exposure is the cause of disagreement was, so far as we know, first suggested by Morris-Airey. It would seem *a priori* that time of exposure might affect the results of the method of flicker in one or both of two ways,—through a difference in the rise of sensation for the different

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[23] "Of considerable practical interest is the fact that the flicker method most nearly agrees with the method of equality of brightness at high illuminations" (Ives, *Philos. Mag.*, 24, (6), 1912, 182). "An illumination of 25 c.m. was chosen since by the means described below this was found to correspond closely to the high illumination at which the results of the flicker and equality of brightness methods became the same" (*op. cit.*, 854).

[24] H. E. Ives, The Spectral Luminosity Curve of the Average Eye, *Philos. Mag.*, 24, (6), 1912, 853-863.

TABLE I[25]

Showing in percentage the difference in result between the methods of flicker and equality of brightness for the five *O*s used by Ives at "250 Illumination Units", his most favorable intensity

λ	H.E.I.	M.L.	P.W.C.	C.F.L.	F.E.C.
.653	−12.0%	+29.0	−18.0	−51.0	−50.0
.643	− 3.6	+56.0	− 7.0	−31.0	−23.7
.632	− 4.3	+20.0	−15.5	−45.5	−12.9
.622	− 7.3	+12.5	− 4.2	−42.0	−15.9
.612	−10.0	+ 8.3	− 6.5	− 7.5	+ 0.3
.594	− 1.0	− 0.5	− 0.5	+ 7.5	+ 5.8
.574	+ 0.5	− 2.4	− 2.5	+27.0	− 5.9
.555	− 0.4	− 8.0	−11.9	− 4.8	+ 8.9
.545	− 3.1	− 8.4	−13.8	+ 3.0	+ 8.0
.535	− 1.9	− 4.0	−12.6	−12.0	+14.3
.526	0	− 8.7	−21.4	− 3.0	+35.5
.517	+ 0.6	−10.8	−13.8	−33.0	+30.0

colors during the individual exposure, and through a difference in their persistence between exposures. Time of exposure might affect the results by the equality-of-brightness method through a difference in the decrease or decay of sensation when the eye is exposed through a longer period than is required for the sensation to reach its maximum, although the chances of this effect being of any considerable importance are slight owing to the comparatively slow rate of decrease of sensation after the maximum has been reached. A brief discussion of how time of exposure might be expected to be effective is given by Morris-Airey, but no experimental work was done. In an article appearing in the *Electrician*, 64, Dec. 17, 1909, 401, he says: "In recent years photometers of the flicker type have obtained a certain degree of prominence, but from time to time suspicions have been raised as to whether the principle on which flicker photometers are based does not involve physiological phenomena which disturb the conditions of illumination so that the numbers obtained are not a true representation of the illuminating powers of the sources to be compared. Physiologists are at variance with regard to color theory. ....With every new change of color theory the physicist would have to change his starting point in the explanation of a phenomenon like the flicker effect and I suggest that a more stable foundation for a physical theory is to be found in the experimental examination of the growth and decay of the retinal stimulus due to differently colored lights without any attempt to connect the form of the curves obtained with physiological theories which are admittedly of uncertain stability." Morris-Airey then cites a phenomenon described by G. N. Stewart (*Proc. Roy. Soc. Edin.*, 1888) which is merely a re-discovery by Stewart of a phenomenon described by Fechner[26] many years before and now called Fechner's colors. Stewart observed that when a mirror reflecting white light was rotated at different rates of speed, the image of the source of light appeared successively in different colors. This selective unbalancing of the retina's action to give the component colors instead of white obviously could be due either to difference in rate of rise of the component color excitations or to a difference in their rate of persistence through the dark interval between the flashes, or to both. This phenomenon can be more conveniently demonstrated by the rotation of either white or black

[25]These percentages were computed by us from Table II, *Philos. Mag.*, 24, (6), 1912, 186.

[26]G. T. Fechner, Ueber eine Scheibe zur Erzeugung subjectiver Farben, *Poggen. Annal.*, 45, 1838, 227-232.

sectors (180° each), a black spiral on a white disc, or a specially constructed disc (Benham's disc) made up of sectors of 180° of white and 180° of black. On the white sector are painted narrow 45° arcs at different distances from the center of the disc and differently spaced with relation to the edges of the white sector. These black arcs are seen in different colors depending upon their spacing in relation to the edges of the white sector, and the colors change as the speed of rotation of the disc is changed.

The rate of growth and decay as an explanatory principle was considered by Ives,[27] who seemed to believe, however, that Dow's rod and cone theory offered greater possibilities of explanation. As an objection to the explanation suggested by Morris-Airey it is interesting to note that Ives says: "It is not established that at the same illumination the rates of rise of sensation are actually different for different colors........it is clear that these phenomena of rise of sensation must be studied with the same observer and apparatus by which are made the flicker measurements in which they may play a part." In Jan. 1914 the theory advanced by us in this paper was first proposed before the Philadelphia Section of the Illuminating Engineering Society, and experimental data were offered in its support. This paper was reviewed in an editorial in the *Electrical Review and Electrician*, Mar. 7, 1914, 478-479; and was published in full in the *Psychol. Rev.*, 22, 1915, 110-162. Later in 1914 Luckiesh[28] explains the discrepancy between the flicker and equality-of-brightness ratings in terms of the different rates of growth and decay of the color sensations; but he seems inclined to reject this explanation in an appendix to the same paper. Still later in the same year Ives (*Philos. Mag.*, 28, (6), Nov. 1914, 708-728) published an article in which a theory of flicker photometry is developed based on an analogy drawn between the response of the eye under successive stimulation and the action of incandescent lamp filaments under a fluctuating current. In this article an attempt is made to show mathematically that, if the eye behaves under the conditions obtaining in flicker photometry as do lamp filaments (subject to certain modifications) under a fluctuating current, the method of flicker should give with high intensities of light at the photometer screen the same results on the average as the equality-of-brightness method. The analogy of the eye to the incandescent lamp filament is not based on an experimental examination of the eye's manner of response, but is assumed. A belief in the importance of lag as a factor comes out clearly in his later writings. He says (*Philos. Mag.*, 33, (6), 1917, 19-33): "The chief experimentally established phenomena of the flicker photometer find explanation in the assumption that the stimulus is transmitted through a layer of matter having a coefficient of diffusivity which is different for the different colors and varies with the intensity of the stimulus......The interval between the reception and the perception of the stimulus [lag] is inversely as the diffusivity."

The theory proposed by us differs from that suggested by Morris-Airey and later by Luckiesh in that we attribute the cause to differences in the rate of rise of sensation, not to both rate of rise and decay. Difference in rate of decay as a factor is, we grant, an *a priori* possibility; but we feel reasonably sure that it is not a factor of any considerable consequence; because, if it were, its effect would be manifested through a differential

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[27]H. E. Ives, *Philos. Mag.*, 24, (6), 1912, 178-181.

[28]M. Luckiesh, *Phys. Rev.*, 4, 1914, 1-11.

summation effect. That there is no detectable differential summation effect seems pretty well proved by the fact that within the limits of sensitivity of the equality-of-brightness judgment the same results are given by both methods of rating when the same lengths of exposure are used. This does not mean, of course, that neither sensation has risen to a higher level in the succession of exposures than it has attained in the single exposure. It means only that, in rising, the relation of level has not changed by a detectable amount,—in other words, that the sensation which lags the more in the single exposure has not gained on the other sensation in the succession of exposures.

FLICKER PHOTOMETRY AND THE LAG OF VISUAL SENSATION

By C. E. FERREE and GERTRUDE RAND, Bryn Mawr College

The object of this paper is to show the relation of the lag of visual sensation to the disagreement of flicker and equality-of-brightness photometry for lights of different composition.

The experimental work covers two points: (1) a comparison of the rating of four spectrum lights at three intensities against a carbon lamp by the methods of flicker and equality-of-brightness; and (2) a determination of the curves of the rise of sensation for these five lights at each of the three intensities, with the same apparatus, the same *O* and the same state of adaptation of the eye, as were used for the photometric comparisons. The results obtained enable us to make a direct comparison of the type and roughly of the amount of the disagreement between the two methods of photometry with the actual luminosities or brightnesses to which the two sensations had been allowed to rise for values of exposure equal to the individual exposures used in the method of flicker. As a basis for this comparison the brightnesses were measured in terms of just noticeably different steps from zero. No experimental evidence more direct as to type than this can be offered, we believe, on the relation of lag to the disagreement in question. If it be found, for example, that for a given intensity of light, red and yellow are underestimated by the method of flicker and blue and green overestimated, then it should be shown by the curves that the sensations aroused by the red and yellow lights had risen to a lower and by the blue and green to a higher value than the sensation aroused by the light of the carbon lamp. The value which the sensation would attain can be read off directly from the curve, and comparisons can be made with the type and amount of disagreement between the two methods of photometry. In a third paper, the evidence offered by this comparison will be supplemented and rounded out by results when the eye is given the same length of exposure by both methods of photometry, and by showing the effect of varying the speed of rotation of the flicker disc and the intensity of light on the amount and type of disagreement between the results in question.

The colored light was taken from the spectrum of a Nernst filament operated at 0.6 amp.; a narrow band in the red at 675 mμ, of yellow at 579mμ, of green at 515mμ, and of blue at 466mμ. The breadth of slit used in isolating these bands was 0.5 mm.

In every case the light was examined for impurities at the objective slit by means of a small Hilger direct-vision spectrometer provided with an illuminated scale. When found, impurities were absorbed out by thin gelatine filters placed over the objective slit, so selected as to absorb as little as possible of the useful light. The spectrum lights were photometered against a 32 cp. carbon lamp, 4.85 watts per mean spherical candle. The photometric comparisons were made and the rise of sensation curves determined at 12.5, 25 and 50 m.c. as measured by the equality-of-brightness method.

The plan of apparatus used is indicated in Fig. 1 and a photograph is shown in Fig. 2. The light from the objective slit was collimated by the lens A, Fig. 1, and focussed on the eye by the lens B. The image formed fell well within the pupil. By substituting this lens system for the eyepiece of the spectroscope, the need and inconvenience of the use of an artificial pupil was eliminated and a surface of uniform color was presented to the eye. Lens B was diaphragmed by a circular opening E of such a size and such a distance from the eye as to furnish a photometric field of 1.9 degrees. Between the lenses A and B was interposed the 90° sectored disc D mounted on an electric rotator; and between lens B and the opening E, the disc F of the rotary tachistoscope used in making the exposures for determining the rise of sensation curves. Discs D and F were both of light sheet aluminum. Disc D was coated with magnesium oxide deposited from the burning metal; disc F was painted matt black.

The method of making the photometric comparisons was as follows. For the equality-of-brightness determinations, disc D, reflecting the light from the carbon lamp[1] was turned so that its edge bisected vertically the photometric field at E, and the luminosity of the colored light was increased or decreased by adjusting the collimator slit until it equalled by the equality-of-brightness method the 12.5, 25 or 50 m.c. reflected from the disc D after the correction had been made for the absorption of lens B. The sensitivity of the determination by the equality-of-brightness method was approximately 2%, varying around this value with the color and intensity of the light. For the flicker determinations the disc D was rotated and the position of the carbon lamp adjusted on the bar until no flicker was obtained at the lowest possible speed that would give no flicker. At this speed the sensitivity of the determination was around 0.4%, varying with the color and intensity of the light.

In making the determinations for the rise of sensation curves for color, the disc D was turned completely out of the beam of light so that the entire field at E was seen as colored; and for the curves for white it was turned so as to cut off the colored light completely and reflect the light from the carbon lamp so that the entire field at E was seen as white (see footnote). The exposure disc F of the rotary tachistoscope was then turned into position. This disc was compounded from two pairs of discs having radii respectively of 17 and 24 cm. These discs were mounted on the axle of the tachistoscope, the position of which was so adjusted that the circular edge of the smaller pair of discs bisected vertically the field seen at E. These halves, right and left, of the photometric field formed the comparison and standard fields for the determination of the rise-of-sensation curves.

This determination was made as follows. The smaller set of discs was closed completely and the larger was opened by a series of adjustments until, with a given speed of the single rotation of the discs, a just noticeable

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[1]For convenience this light will be designated as white in the rest of the paper and in the charts.

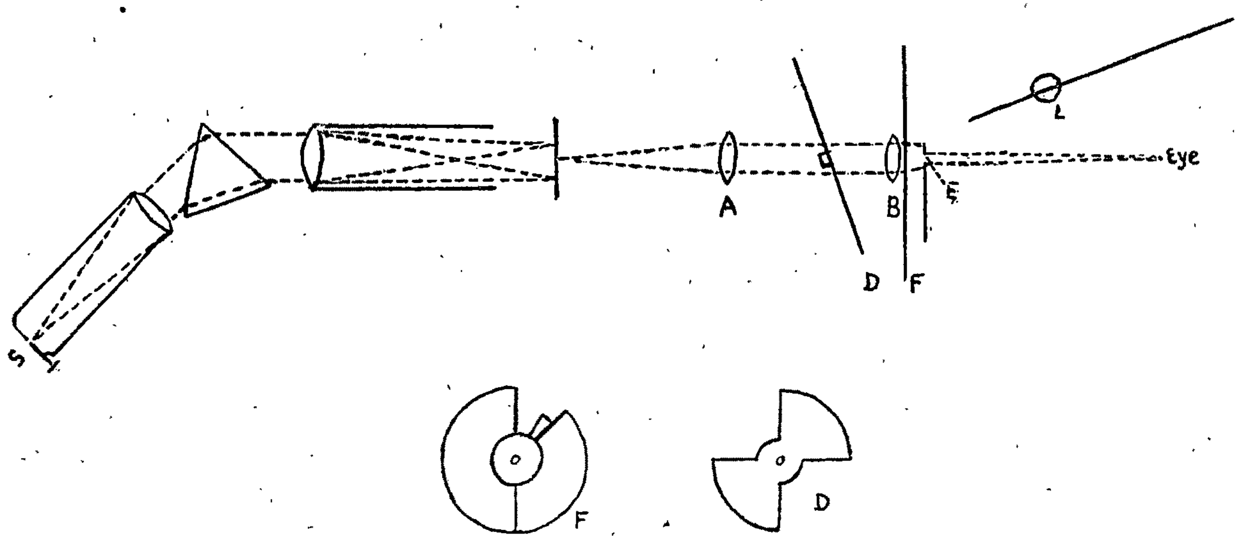


Fig. 1. Plan of Apparatus.

sensation was aroused. For the next observation the smaller pair of discs was given this value, and the opening of the larger pair was increased until with the same speed of rotation a just noticeable difference in brightness was obtained. The series was continued, the comparison opening for one observation being made the standard for the next, until a value of exposure was obtained, no further increase of which caused a noticeable increase of brightness of the comparison field. This was accepted as the optimum value of the sensation, and the sensations produced by other exposures were rated in terms of their number of just noticeable differences from zero. Further increase of length of exposure caused a decrease in the brightness of the comparison field, and a continuation of the series beyond this point gave the just noticeable difference values needed to plot the other half of the curve which represents the temporal course of sensation, rise and fall.

The exposure apparatus or rotary tachistoscope was made up of the rotation apparatus and two pairs of specially constructed sectored discs. The rotation apparatus consisted of a heavy Y-shaped stem with carefully turned bearings in which rotated a spindle fitted with an arbor and chuck to hold the exposure discs. Since only a single exposure was wanted for any observation, the discs were driven by a gravity or fall device. That is, on the other end of the spindle was the smaller of a two cog-wheel gear system. Attached at its middle to the larger cog-wheel was a rod 1 m. long designed to carry on its two arms equal weights. For most of this work, in order to give short exposures and high speeds, a heavy weight was carried on one arm alone. These weights, which slip on the rod, were of lead and were held in position by means of set-screws. The rod was graduated in mm. so that at any time the position of the weights could be read directly from the axle. Before an exposure the rod was held in position by a catch, the height of which was adjustable. When the catch was released the weight fell, rotating the discs with a velocity depending upon the height of the catch and the position of the two weights on the rod. After the fall the other end of the rod was caught automatically by a second catch.

Another factor in determining the length of the exposure is the displacement of the open sector from the path of the stimulus light or its position in the circle of rotation; for obviously upon this will depend, with a given height of the catch and a given position of the weights on the driving rod, the amount of acceleration which is given to the disc before the open sector reaches the path of the stimulus light. This value for the initial distance of the open sector from the path of the stimulus light was fixed by pinning permanently to the axle that disc of each set the edge of which made the beginning of the exposure, so that these edges were in the same radial line.

The values of the open sectors were measured with a Vernier protractor graduated to seconds of arc. These measurements after any number of sets of observations could be converted into units of time by a simple process of calibration. Smoked paper was clipped to the disc across the open sector; the pendulum was released with the weights, the starting point, etc., just as they were in the original observation; and a time line was run across the open sector by an electric tuning fork the vibration frequency of which was known. The paper was removed, shellaced, and counted at leisure.

Prior to the use of this method of determining the rate of rise of sensation, a preliminary study was conducted in our laboratory in which a comparison of results was made using all of the older methods and three new ones devised for the purpose.[2]

[2]For the details of this study, see M. A. Bills, The Lag of Visual Sensation in its Relation to Wave-length and Intensity of Light, *Psych. Monog.*, No. 127, 101 pp.

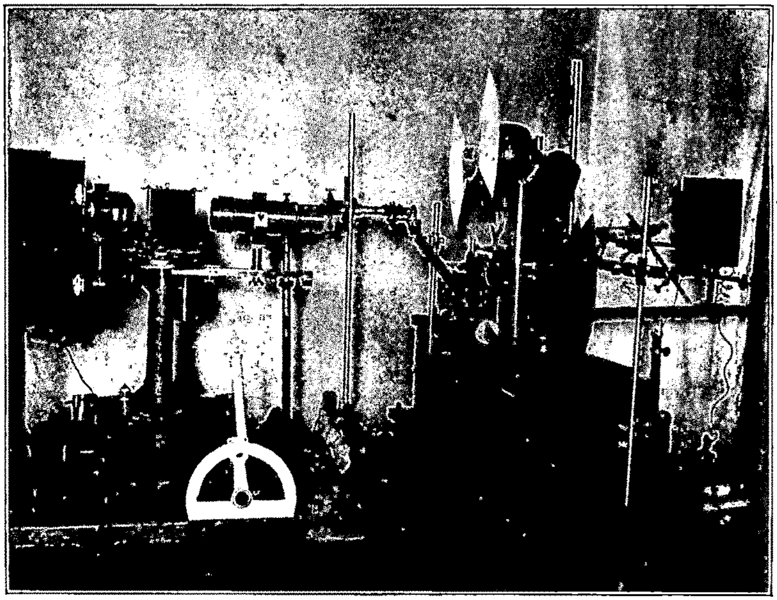


FIG. 2. Photograph of Apparatus.

The work was done with spectrum lights with the same O and state of adaptation of the eye and as nearly as possible the same apparatus for each of the methods. As the result of this intercomparison and interchecking of methods, the one used in the present study was selected as the most feasible and perhaps the surest in principle. The new methods, all of which were devised by the present writers, were designed not only to ascertain the time in which a given stimulus produces its maximum effect on sensation, but also to make possible a comparison of the sensation values at any point in the rise or fall. There are two possibilities of making accurate and reproducible judgments of sensation, namely, the judgments of equality and of just noticeable difference. Upon these judgments all quantitative work on sensory responses ultimately rests. Both of these were utilized in developing the new methods mentioned above. The development of these methods has, in fact, been just the systematic application of these two judgments to the problem in question in accord with the purpose mentioned earlier in the paragraph. The just noticeable difference obviously may be determined by holding the time of exposure constant and varying the intensity of light, or conversely by holding the intensity of light constant and varying the time of exposure. The latter principle was employed in the method used in this work. The sensation value obtained by this method may thus be called a time just noticeable difference.

In order to preserve as far as possible the same state of adaptation of the eye for the rise-of-sensation work as was used for the flicker and equality-of-brightness comparisons, the photometer lamp was always kept lighted and set to give the desired intensity. The period of adaptation to this light, the preexposure, etc., were carefully controlled.

The same O, one well trained in photometric and tachistoscopic observation, was used throughout the work reported in this and the following sections. It is to be regretted that the work of this first section is so laborious that it was impossible to repeat it on other Os. What was wanted here was a comparison of the flicker and equality-of-brightness ratings with the rise of sensation curves. We have as yet been unable to procure other Os sufficiently trained who could give the great amount of time needed for this work. In case of the supplementary data to be given in the next paper, however, the general trend of result has been checked and verified by other Os,—6 for the effect of change of intensity; 20 for the effect of speed; and one other (2 in all) for the agreement of the two photometric methods when the length of exposure is made equal. Results for more Os will be obtained on this latter point as soon as possible.

Curves representing the results obtained for the rise of sensation are shown in Charts 1-4. In these charts just noticeable

differences in luminosity are plotted along the vertical co-ordinate and time of exposure along the horizontal. Chart 1 shows the curves for the five lights at 12.5 m.c. At this intensity the curves were completed to the maximum and slightly beyond. As

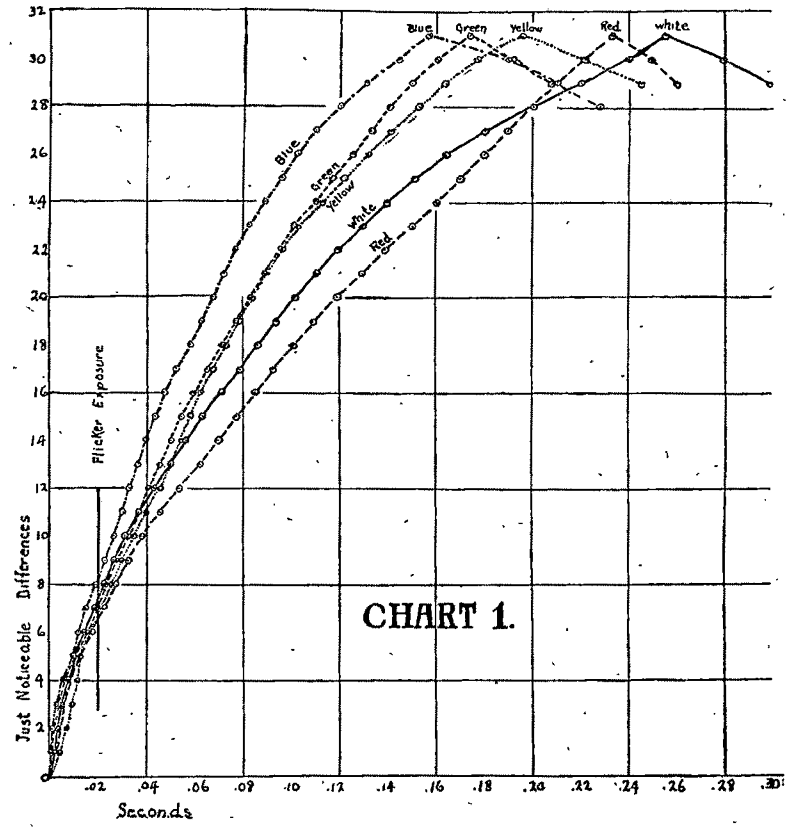
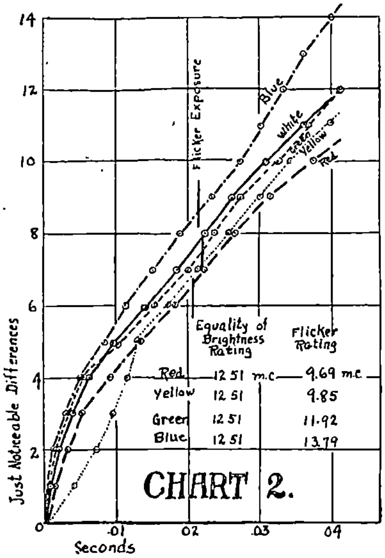
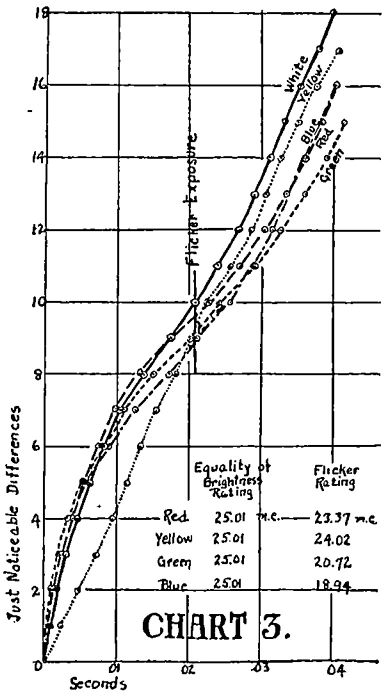


CHART 1. Curves showing the rise of sensation to the maximum for white, red, yellow, green and blue at 12.5 meter-candles

would be expected from the fact that all the lights were of the same photometric value, the same number of just noticeable differences (31) were obtained for each to the maximum. The optimum time of exposure for the blue was 0.157 sec.; for the green 0.174 sec.; for the yellow 0.195 sec.; for the red 0.233 sec.; and for the carbon lamp 0.254 sec.

In Chart 2 the lower part of the curves of Chart 1 is plotted on a larger scale. On these curves the height to which the sensation rises for a length of exposure equal to that of the





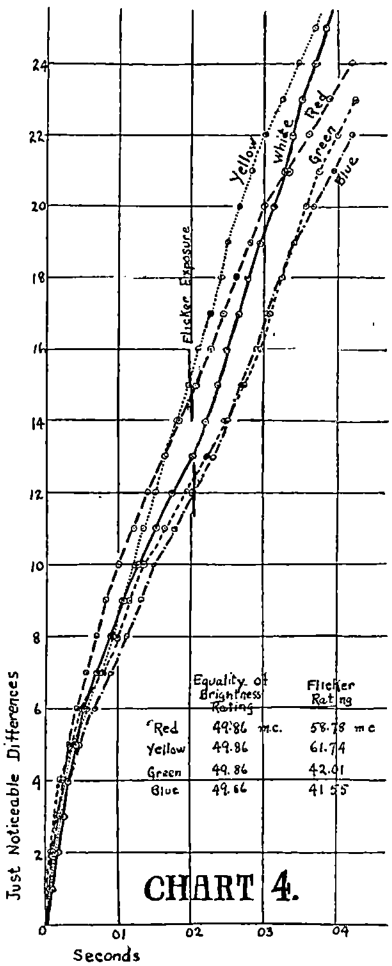


CHART 2. Curves showing the rise of sensation up to 0.04 sec. exposure for white, red, yellow, green and blue at 12.5 meter-candles

CHART 3. Curves showing the rise of sensation up to 0.04 sec. exposure for white, red, yellow, green and blue at 25 meter-candles

CHART 4. Curves showing the rise of sensation up to 0.04 sec. exposure for white, red, yellow, green and blue at 50 meter-candles

individual exposures used in the method of flicker is indicated by a short vertical line. In the use of the flicker method we worked at the lowest possible speed that would give no flicker. Since this speed is slightly different for the different colors, the values of the individual exposures were slightly different. On this chart are shown also the photometric values given to the colored lights both by the equality-of-brightness and the flicker methods. It will be noted that red, yellow and green are all underestimated by the method of flicker, red the most; and that blue is overestimated. An inspection of the curves shows that this result is as it should be according to the relative heights to which the sensations rise for the time of exposure used in method of flicker.

In Chart 3 curves are given for the rise at 25 m.c. These curves were not continued to the maximum because of the prohibitive amount of time and work required to make the complete series for lights of this intensity. The series was continued up to an exposure time of 0.04 sec. (15 to 18 just noticeable differences, depending upon the color), which is well in excess of the flicker exposure. The photometric results show that all the colors were underestimated in the order, from least to greatest, of yellow, red, green and blue. The curves for rise show that for the value of exposure used in the method of flicker, the carbon light has risen to the highest value in sensation,—then yellow, red, green and blue.

Chart 4 shows the curves at 50 m.c. These curves likewise were continued only up to 0.04 sec. (20 to 27 just noticeable differences). The photometric data show in order an overestimation of yellow and red by the method of flicker at this intensity and an underestimation of blue and green as would be expected from the curves in Chart 4. Several intersections in the curves will be noted,—one for red and yellow at an exposure slightly less than that used in the method of flicker, and one for green and blue at an exposure slightly longer than the flicker exposure.

In the next paper of the series curves will be given for each color and white at the three intensities. An exact correspondence in the type of deviation of the photometric results, and of their ranking as to amount with the respective differences in the heights to which the colored and white sensations have risen, will be noted.

It may be stated that the photometric and rise-of-sensation work was done in quite different series. The chance for bias by pre-knowledge of the results of one type of work before the other was done was negligible. For example, while the work was scattered through the years 1917-1922, the final conversion of the settings of the exposure apparatus into units of time was not made until late in the spring of 1922.

THE CONCEPT OF BACKWARD ASSOCIATION

By Hulsey Cason, Syracuse University

In discussing certain aspects of memory, various writers have referred favorably to the concept of backward association, a concept which is definitely traceable to Ebbinghaus' epoch-making work. We believe that this more or less uncritical acceptance of backward association—which is a subject of some theoretical importance—is a result of the natural difficulty which modern thinkers experience with the older laws of association, as well as of an improper interpretation of Ebbinghaus' work. In this paper, we shall attempt to point out some of the difficulties in accepting this concept. We shall not discuss the validity of the objective results themselves.

Ebbinghaus' classical study of memory showed that, when a series of nonsense syllables, 1, 2, 3, etc. is learned so that one correct reproduction is possible, associations are formed not only from syllable 1 to syllable 2, from 2 to 3, etc., but also from 1 to all of the other syllables in the series, the strength of the associations decreasing with the number of intervening syllables.[1] Other studies by Ebbinghaus also demonstrated the fact that associations are formed in the backward direction, as, for example, from 4 to 3, 4 to 2, etc.

Having obtained these reliable results, Ebbinghaus was somewhat perplexed concerning the theory of association which was current in his day. Apparently, while he was learning one association, still other associations were being formed at the same time. Why was even a weak connection formed between syllables 1 and 10, when in a single span of consciousness Ebbinghaus could grasp only seven syllables? And, most difficult of all to explain, why were *backward* associations formed when he seemed to be exercising the associations only in the *forward* direction? Ebbinghaus exclaimed: "How odd are the connections of human thoughts which jostle in their flight!" and in another place remarked that "the customary formulation, 'ideas become associated if they are experienced simultaneously or in immediate succession,' has something irrational about it."[2]

[1]The strength of the associations was measured by the familiar relearning method.

[2]H. Ebbinghaus, *Memory, a Contribution to Experimental Psychology*, 1885, 108 (trans. by H. A. Ruger and C. E. Bussenius). Throughout this discussion we shall refer only to Ebbinghaus' *Gedächtnis*, because it is in this work that his views on memory and association are most clearly stated. Neither in the *Grundzüge der Psychologie* nor in any other of Ebbinghaus' later works do we find a clear statement or a satisfactory explanation of backward association.

Ebbinghaus' own formulation of the law of association reads as follows: "Ideas which have been developed simultaneously or in immediate succession in the same mind mutually reproduce each other, and do this with greater ease in the direction of the original succession and with a certainty proportional to the frequency with which they were together."[3] This formulation agreed fairly well with Herbart's view, which seemed to Ebbinghaus to be a safe middle ground. In discussing Herbart's formulation, Ebbinghaus writes: "According to this conception, therefore, the associative threads, which hold together a remembered series, are spun not merely between each member and its immediate successor, but beyond intervening members to every member which stands to it in any close temporal relation. The strength of the threads varies with the distance of the members, but even the weaker of them must be considered as relatively of considerable significance. The acceptance or rejection of this conception is clearly of great importance for our view of the interconnection of mental events, of the richness and complexity of their groupings and organization. But it is clearly quite idle to contend about the matter if observation is limited to conscious mental life, to the registration of that which whirls around by chance on the surface of the sea of life."[4] Probably no one today would accept this general statement of the law of association. It appears that the principal difficulty is to be found in the nature of the learning process which Ebbinghaus employed, rather than in the objective results which he obtained. We may therefore discuss at this time the procedure which is commonly used in learning nonsense syllables.

No matter how expert one happens to be in concentrating on nonsense syllables, it is impossible to control the conditions in such a way that syllable 2 is thought of only after syllable 1, 3 only after 2, etc. While Ebbinghaus took practically all possible precautions to establish fairly constant experimental conditions, *some* fluctuations of attention did occur. In addition to thinking of 6 after 5, 10 after 9, etc., even a practised subject like Ebbinghaus found himself thinking occasionally of 8 after 4, of 12 after 3, etc.; and, doubtless to a lesser degree, also of 4 after 8, and of 3 after 12. The practice is not *all* from 1 to 2, 2 to 3, etc.; *some* practice takes place on other connections when these fluctuations of attention occur; and it is in these cases, where the learning process does not limit its activities to the associations which the subject is trying to form, that the additional associations, such as 4 to 8, 3 to 12, 1 to 3, etc., are formed. These considerations also apply to the learning of the "backward

[3] *Op. cit.*, 90.

[4] *Op. cit.*, 94.

associations:" practice is really forward in such cases, rather than backward. In learning the lists of syllables, there was also a certain amount of thinking about the *position* of certain syllables in the series, as, for example: "5 comes after 2 and 3," "7 and 8 come before 12 and 13," "I just repeated 8, " "here are two syllables which suggest the word so-and-so," etc. All of this constitutes *practice.*

It is significant that Ebbinghaus says very little about his *introspections*, which would have added to the value to his work. He was certainly not able to control his interest and attention *completely* for hourly periods, year after year, in such a burdensome and monotonous task as this. It will be remembered that Ebbinghaus attempted to learn each list so as to be able to repeat it just once correctly; and the relative time spent in attempting to reproduce the numerous lists was therefore considerable. During the unsuccessful attempts, especially, when he would think: "1, 2, 3, 4, 5, 6, 7, 8,. . . . . . . ?" it was only natural for him to think occasionally of 10, or 11, or 12, etc.

Certain experiments have shown that we frequently associate an item with a "whole," so that the item can later call up the whole, or *vice versa.* For example, the stimulus, town A, may cause one to think of one of a certain group of individuals whom he knew in this place. The nonsense syllables in Ebbinghaus' experiment were doubtless associated with the whole list in this same general fashion: Ebbinghaus would surely do *some* thinking about *this* list, in addition to jumping forward or backward occasionally to think about some nonsense syllable which particularly attracted his attention. When Ebbinghaus hesitated in repeating a series, pausing, for example, on syllable 10, it was only natural for him to hunt around (for the next syllable) in the *rest of the list,* recalling 12 or 13 or 14, etc., in this way forming a more definite association between 10, on the one hand, and 12 or 13 or 14, on the other.

The claim might be made that the formation of additional associations in this manner would be rather slight. This is doubtless true; but it is also true that when Ebbinghaus rearranged the lists (using 1, 3, 5, etc., or 2, 4, 6, etc.), the saving of work (in percent. of original learning time) was itself very slight, amounting to only 10.8%, 7.0%, 5.8% and 3.3% respectively when the number of intermediate syllables skipped in the formation of the derived series was 1, 2, 3 and 7.[5]

In another variation of the experiment, Ebbinghaus learned a list and then rearranged the nonsense syllables in a chance

[5]It should be added that these results, while of small magnitude, were quite reliable.

order (except that the first and last syllables remained the same as before); and he then relearned the list. There was little gain from the familiarity with the syllables: "the syllables were, therefore, in themselves, outside of their connection, so familiar to me that they did not become noticeably more familiar after being repeated 32 times."[6] With one group of tests he found a saving of only 12 sec. on a base of (roughly) 1261 sec. "In the case of the construction of a new series through a mere permutation of the syllables, there was an average saving of 12 seconds. . . . No evidence has been secured. . . . establishing the facilitation of the process of relearning a series by means of the identity of the syllables and the identity of the initial and final terms."[7] In another group of tests he found an average *increase* in expenditure of time of 5 sec. It seems therefore that the facilitating associations just about balanced the disturbing associations: so that there was little change in the efficiency of learning the two combinations. Some associations were now easier to form because of the slight practice, but other connections had to be unlearned. Ebbinghaus naturally became very familiar with the nonsense syllables which he used, and the learning process had to do principally with the formation of connections between different syllables.

It appears that Ebbinghaus' formulation of the law of association, as stated above, is entirely *too inclusive.* The actual practice of an association from 8 to 9 does not *itself* strengthen the association from 8 to 12 or from 8 to 4. During the practice, Ebbinghaus was exercising other bonds besides those from 1 to 2, 2 to 3, etc.; and some introspections would have indicated the nature of this practice much more precisely. The additional backward associations were formed in the same general fashion as the additional forward associations: the *practice* in these cases was simply "backward" instead of "forward." The later complete learning was of the same nature as the former incomplete learning had been. If practice had been confined entirely to the direction from A to B, then we feel sure that there would not have been the slightest tendency for stimulus B to call up response A.

In learning the backward associations, some practice was obtained during the regular learning of the series, as well as during the first imperfect recitation or recitations. The practice due to fluctuations of attention during the learning of the series seems to be more important in accounting for the backward associations. It is probable that highly trained subjects, by using a suitable exposure apparatus, could reduce this error

[6] *Op. cit.*, 106.
[7] *Op. cit.*, 101.

considerably. The error due to the first imperfect recitation or recitations might also be reduced to some extent, but it could probably not be eliminated completely. However, very careful introspections could throw some further light on the disturbing elements present, as well as upon the relative importance of these various factors.

There is a serious difficulty in regard to the physical mechanism involved in this so-called backward association, which is itself sufficient to discredit the assumption of this form of learning. In learning a series of nonsense syllables, the first visual stimulus is nonsense syllable 1, calling up by means of cells in the cortex a subvocal response of syllable 1. This motor response later becomes associated with nerve cells in the brain, which in turn cause the speech organs to pronounce (subvocally) syllable number 2, etc. The stimulus is visual at first, and the response is always motor. It would be impossible for such a specific chain of connections to operate backwards, partly because of the law of forward direction across the synapse. It is now generally recognized that it is misleading to speak of associating two *objects* in the mind. If we accept the view that an association is between a stimulus and a response, then it would be impossible for the association to function backwards. Although there is evidence of centrifugal sensory paths, the existence of these paths should probably not be taken as evidence favoring the possibility of backward association.

It is in general preferable not only to take account of the introspections of the subject, but also of the physical processes which are involved. The introspective evidence in this particular case would have identified the other factors at work in learning nonsense syllables, and a consideration of the physical factors involved, *i.e.*, the law of forward direction, etc., would have convinced workers in this field that the physical mechanism is highly improbable.

THE EFFECT OF GROUPING ON THE PERCEPTION OF DIGITS

By James D. Weinland

In a period of attentive vision so short as to preclude eye movements only a small number of impressions can stand out clearly, the span of attention being definitely limited. If we give but one glance at any unrelated collection of objects, as goods in a store window, items in a picture, or the like, we are able to enumerate only a few, probably but four or five of these objects. There are, however, occasions, in advertising, automobile license numbering, and many other phases of actual life, when for practical purposes as many objects or units must be presented simultaneously as can be clearly perceived. With a presentation, then, of a given number of units clear perception will depend on various factors of the exposure, highly important among which will be the spacing or arrangement of the units into groups. It is into this factor of grouping that the present investigation inquires. The experiment is limited to a study of rows of six digits presented in the different possible groupings.

For the scientific study of the area or range of visual attention it has been customary to employ some form of short exposure apparatus or tachistoscope, the principle of which is to furnish a fixation field which is supplanted for a measureable brief period by another field that contains the test material. There is thus a pre-exposure field, an exposure field, and a post-exposure field.[1]

The first gravity tachistoscope was devised by Wundt. Cattell constructed one of the early ones and used it in many of his experiments on reaction time and the attention span. It consisted of a screen which composed the pre- and post-exposure fields, the center being cut out so that as the screen fell the exposure field was presented to view. The screen was supported to slide as a window between guides at gravity speed; the exposure time was varied by changing the length of opening in the screen through which the exposure field was seen. Directly to the rear of the slide, the exposure field was mounted so that the screen would slide in front and in close proximity to it, exposing it as the cut-out portion of the screen slid past[2].

Wundt lays down the following rules for the proper use of the tachistoscope. (1) The exposure must be short enough to preclude eye movements. (2) The arrangement of the fixation mark and of the stimulus must be such that all constituents of the exposed objects can be seen with at least approximately equal distances; the exposure field must coincide with the ocular field of direct vision. (3) The exposure of all parts of the field

[1] G. M. Whipple, *Simpler Processes*, 266 ff.; R. Dodge, Psychology of Reading, *Psy. Rev.*, 8, 1901, 56; W. F. Dearborn, Perception and Reading, *Psy. Res. of J. M. Cattell*, 1914, 37.

[2] J. McK. Cattell, Ueber die Trägheit der Netzhaut und des Sehcentrums, *Phil. Stud.*, 3, 1886, 94ff.; G. M. Whipple, *Simpler Processes*, 262ff.

should be simultaneous, or so nearly so that there shall be no noticeable differences in time in the illumination of the various regions. (4) The retinal adaptation must be favorable, and sudden transition from dark to light must be avoided. (5) Persistent after-images must be avoided. (6) The duration of the retinal excitation must be limited enough to preclude roving of attention over the exposure field. (7) A ready signal must be given at an appropriate time before the exposure—Dodge adds three further suggestions. (1) The relative illumination of the pre-exposure, exposure, and post-exposure fields should be capable of experimental modification. (2) The exposure should be noiseless and free from distractions. (3) It should be possible to arrange for monocular or binocular observation.[3]

The previous work which most closely resembles the present investigation was done by Paul Ranschburg who, using the tachistoscopic method, studied the types and causes of errors in the perception of digits.[4] He had been examining a number of people on their range of perception when this problem interested him. He used 2, 3, 5 and 6 digit rows for his material, taking the digits from a handbook of statistics. He found that the perception threshold for heterogeneous rows was lower than for homogeneous rows of digits. When the rows contained identical elements the error score was the highest; it was somewhat smaller for similar elements, and least for heterogeneous rows.

Homogeneous rows:	identical elements	654536
	similar elements	926710
Heterogeneous rows		913845

Using arbitrary weighting devices he scored his results and found the percentage of error to be:

Rows with identical elements	87.7%
Rows with similar elements	76.4%
Rows with heterogeneous elements	38.5%

In the group of "similars" 3 was taken for 8, and conversely; 9 for 6; 2 for 0 or 9; and 1 for 7. In the heterogeneous rows the errors were principally transmutations, or reversals in the response of the order in which the digits were arranged in the stimulus: 90% of the total errors were made in the right half of the numbers, or from four to six inclusive in the series of six. In two-thirds of the cases errors were made in the next to the last digit, in nearly one-third of the cases the offending digit was the fourth, only seldom was it the last.

The experiment undertaken in the Psychological Laboratory at Columbia was somewhat similar to Ranschburg's. It differed, however, in seeking the effective grouping of digits to exclude errors rather than in examining the characteristic types of errors usually made.

The apparatus used consisted of the usual gravity tachistoscope modified by means of weights to control the speed of descent of the screen, and thus the exposure time. The weights operated in the following manner: a derrick was constructed to the rear of the tachistoscope and upon this derrick two small pulleys were mounted, one of the pulleys being directly over the screen, the other to the rear of the apparatus so that

[3]G. M. Whipple, *Simpler Processes*, 262ff.

[4]P. Ranschburg, Ueber Hemmung gleichzeitiger Reizwirkungen, *Zts. f. Psych.*, 30, 1902, 39ff.

a line running through it would suspend weights unobstructed by the apparatus. Through these pulleys a line was then run, to the falling screen at one end and to the weights at the other end. In this manner the falling screen was forced to pull a load as it fell, its speed being thus diminished. Weights were also attached to the screen itself, the object being to make the apparatus somewhat heavier in order that its operation might be more even, producing a more constant exposure time. By changing the weights a wide variation in speed of the falling screen and thus of the exposure time was attainable. By increasing the load the falling screen must pull, its speed was diminished; by decreasing the load it must pull, its speed was increased.

The speed finally chosen for the experiment was selected empirically. A subject attempted to read figures, similar to those used in the experiment, at different exposure times, the speed of screen-fall being varied by means of changing the attached weights as described. When the optimum speed was selected the weights were left constant throughout the entire experiment, and the exposure time was measured.

The time of exposure was measured with a Bergström chronoscope which had previously been checked against a Hipp chronoscope: 19 exposures were measured and their median time, after the probable error of the instruments was eliminated, was found to be 176 σ, with a P.E. of approximately 2 σ. This time of 176 σ was used throughout the experiment.

In regard to the time of exposure much controversy has taken place. The first arguments were for exposures as low as 1 σ, and this in spite of the fact that Dodge found the reaction time of the eye somewhere near 162-170 σ [5]. Dodge states, however, that exposures of such short periods as 1 σ are artificial, and give results removed from natural conditions, which provide no basis for general conclusions.[6] He recommends a time "well over 150 σ" for exposure where threshold values are not being specifically studied. Huey finds that the fixation pauses in reading average about 185 σ, for fast readers, but with a large variation.[7] The exposure time used here is thus "well over 150 σ", approximating the average reading fixation, and providing a normal stimulation.

The tachistoscope screen used in this experiment consisted of a black pre-exposure field, an opening for the exposure, and a post-exposure field. It was made to slide in close proximity before the figures to be exposed so that the eyes should be properly focused for the figures, during the exposure time. Both pre- and post-exposure fields were black, though upon the pre-exposure field a small white line was placed upon which the subject could fixate his eyes as the screen fell, in order that his eyes should be properly focused for the figures.

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[5] R. Dodge, The Reaction Time of the Eye, *Psy. Rev.*, 6, 1899, 477ff.

[6] R. Dodge, Study of Visual Fixation, *Psy. Rev. Mon. Sup.*, 1907, No. 35.

[7] E. B. Huey, *Psy. and Ped. of Reading*, 1908, 56ff.

The essentials laid down by Wundt for tachistoscopic work, which we mentioned above, were all complied with in the apparatus used for this experiment; though as relative and not absolute phenomena were studied the added suggestions of Dodge as to lighting and binocular *versus* monocular vision were not carried out. There was some noise in the apparatus used, principally a thud as the screen reached the end of its fall; but the sound was slight, and no subject in his introspections mentioned it as a distraction.

The digits and the order of their presentation throughout the experiment were determined by chance, as follows. All the digits from 0 to 9 were written on small paste-board cards of equal size and shape, placed in a box, and thoroughly shaken up. They were then strewn upon the table, and the six nearest to *E* were picked up in the order in which they fell upon the table and were used for a group. It will be observed that, with only one each of the 10 digits used in the shuffle, repetitions in the group of six selected digits were impossible and so did not occur,—save in one instance in the series of 100 groups when a 4 was repeated by typographical error within a single group.

The arrangement of the groups then followed. In all of the groups there were six digits; but while in some groups the six were all equally spaced (271056), in others the six digits were broken into smaller units; 3-3, 2-2-2, 4-2 and 2-4 were used, as for instance: 194 053, 17 59 84, 8572 34 and 40 9516. There were 5 of these groups, and in the experiment there were 20 repetitions of the group-form exposure. Identical group forms were not composed of identical digits; the digits, as previously explained, were selected by chance for all the 100 exposures alike.

The occurrence of the groups in the exposure series was also determined by chance in the same way that the digits were originally selected. Five digits, there being five groups, were written on paste-board cards of equal size and shape and shaken up in a box. They were then thrown upon the table and taken in the order of their fall to represent the order of exposure of the groups in the experiment. The cards with the 5 digits were then again shuffled and thrown, the resulting order determining the order of the next 5 groups of digits. This procedure was repeated till the order of grouping of the whole series of 100 was arranged.

With the digits and the order of group presentation determined, the next step taken was to type these digits upon a long roll of paper at intervals of 14 mm., the beginning of each group the width of 6 spaces (typewriter) from the edge of the roll of paper. This roll was then used as an exposure series, and was slipped in and out through two slits in the back of the exposure

field, the side presenting the numbers being turned toward the subject, so that as the screen fell the numbers should be visible for the exposure period. Before the experiment the number series was run through the tachistoscope, and with each group in the center of the exposure field a mark was placed upon the back of the paper, in order that later *E* could make sure that each number series would be in the center of the field of vision during the tachistoscopic exposure.

A short practice series of digit groups was arranged for use in the experiment, composed and presented in the same manner as the experimental series; 9 groupings were used in this series.

Procedure

The subject was seated before the tachistoscope at a distance from the screen which he selected during the practice series, and which varied with the different subjects from 2 to 4 ft. Paper and pencil were given him to write down his observations. His instructions were as follows: "Look at the fixation point before you. A series of groups of digits will be shown you which you are to repeat. In each exposure there will be six digits; be sure and write them all. If you are not sure of any digit, guess."

E, after presenting the practice series, then inserted the experimental list of digits into the apparatus as previously described. The roll was pulled to the position already marked as having the first group of digits in the center of the exposure field, the subject was given the warning signal, and the catch releasing the screen was released. While the subject was recording his observation, the screen was raised to its original position, the series of digits was advanced till the next group was in the center of the exposure field, and all was ready for a second exposure. This procedure was repeated till the entire series had been shown. No attempt was made to equate the inter-exposure times, the only caution taken being to avoid hurrying the subject.

The observations of the subject were compared with the series exposed. It was found necessary to weight the results in order to score them, and the following method was taken. The weighting was by means of penalties. For all 6 digits correct the subject was given a score of 6. For each omission or error he was penalized 2 points, for a digit added he was penalized 1 point, and for each transmutation he was penalized 1 point. If 2 numbers were transmuted together, as 3-6 for 6-3, just 1 point was taken for this mistake. As the results sought were comparative, and this system of weighting applied to all the group alike, its use could not bias the results.

Ten subjects took part in this experiment; 8 were men, and 2 were women. All were graduate students in the department

of Psychology. The two who made the highest scores are mathematicians, and it seems that their accustomed use of figures aided them. Nearly all the subjects had previously performed a laboratory experiment testing out Ranschburg's findings, and so were familiar with the general type of the experiment.

Following is a tabulation of the scores. After the actual scores were obtained they were turned into percentages of possible scores. A total score of 120 was possible for each group, and so a score of 93 would mean a percentage score of 77.5. These percentage scores were added, compared, and their probable errors calculated. Thorndike's table was then referred to, to find the chances of there being an actual difference between them.[8] In the tables the different subjects will be designated by the letters: A B C D E F G H I and J. In the graph the averaged results from all the individuals are shown.

TABLE A. SCORES

Subject	6 digits in groups of 2	6 digits in a single group	1st group 2 digits; 2nd group 4	1st group 4 digits; 2nd group 2	2 groups of 3 digits each
A	.558%	.683%	.566%	.625%	558%
B	.708	.766	.600	.735	.658
C	.808	.616	.633	.658	.625
D	1.000	.941	.975	.958	.933
E	.658	.775	.758	.783	.608
F	.916	.833	.791	.775	.783
G	.641	.700	.716	.633	.473
H	.741	.616	.675	.700	.733
I	.725	.800	.666	.616	.683
J	.450	.408	.425	.283	.308
Av.	.7205	.7138	.6805	.6756	.6364

TABLE B. RELIABILITY

Averages	S.D.	P.E.	PEA	A-A	x/Q[9]	Table[10]
.7205	4.01	2.70	.85	.0		
.7138	4.39	2.96	.93	.67	.53	6396
.6805	4.37	3.14	.93	4.00	3.12	9823
.6756	5.16	3.48	1.10	4.49	3.23	9852
.6364	5.12	3.45	1.09	8.41	6.08	certain

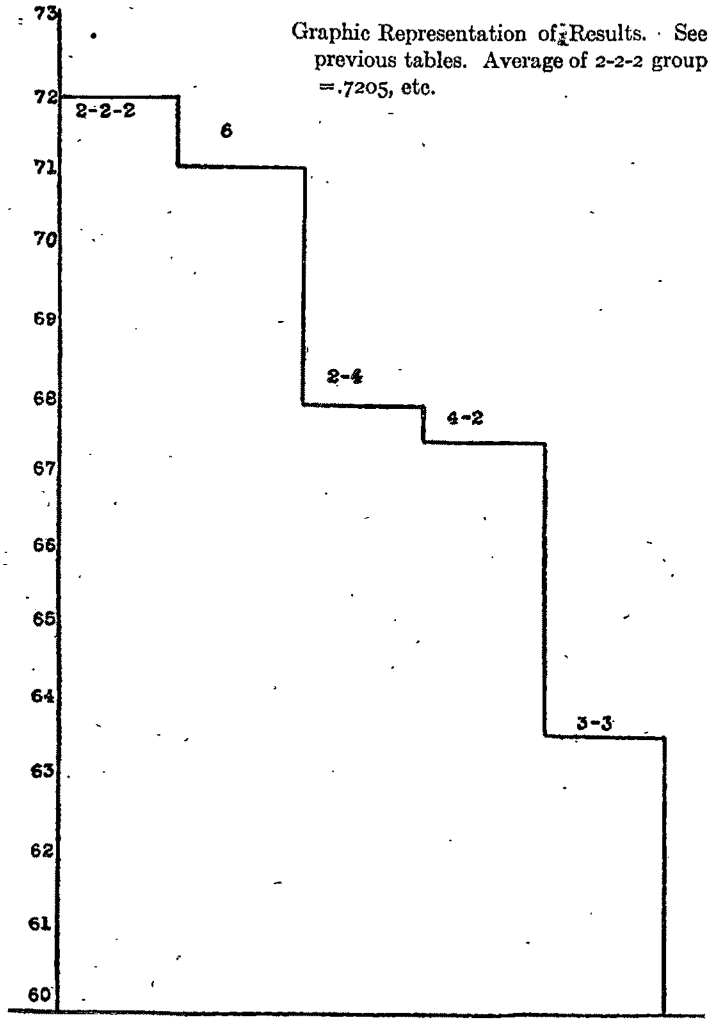
It is evident from the tables given above that the 6 and 2-2-2 groupings are approximately the same, the 2-2-2 group leading but by a margin that is hardly reliable. Both of these groups, however, are better than any of the others.

[8]E. L. Thorndike, *Mental and Social Measurements*, 1913, 200.

[9]$x/Q = \frac{\text{A-A}}{\text{PE of Diff.}}$

[10]Table = Thorndike, chances in 10,000 of a difference: see *Mental and Social Measurements*, 200.

No explanations seem necessary of these findings, save that they are facts. To theorize about them one might say that habit possibly helped in the perceiving of the 2-2-2 group, as



Graphic Representation of Results. See previous tables. Average of 2-2-2 group =.7205, etc.

much of our daily financial dealing is with two-figure groups. That the six-group was better than the others may have been because it was a unit; and that it fared worse than the 2-2-2 group, because it was too large a unit. There was no indication

given in the introspections just why any of the other groups should have been difficult. It may have been because attention was divided. Perhaps one part of the group was overperceived, owing to the separation, with a consequent failure in the other part. These are but possible explanations, however, the facts remaining that the 6 and 2-2-2 groups gave the best results.

Following is a table showing the distribution of the errors throughout the different groups. The letters T, E, O, A stand for transmutations, errors, omissions, and additions of digits by the various subjects during the experiment.

Table of Errors

Grouping	2-2-2	6	2-4	4-2	3-3	Total
T	57	73	75	64	64	333
E	83	65	80	100	95	423
O	18	19	23	18	27	105
A	4	3	5	4	1	17
Totals	162	160	183	186	187	878

It will be observed from the above table that 90 more groups had errrors than transmutations, the type of mistake which followed next in frequency. The 2-2-2 group had a comparatively large proportion of errors, while the 6 row had the fewest groups with errors, but had a high percentage of transmutations.

This table compares favorably with the general findings of the experiment, though it indicates only the number of groups in which the different types of errors were found, and does not take into consideration the number of errors within the groups or their weighting. It will be seen, however, that by this table the 6 and 2-2-2 groupings are comparable in number of total mistakes, with considerably less than the other groupings.

Conclusion

The results show a scarcely dependable difference between the perceptions in the 6 and 2-2-2 groups, but either of these groups is definitely better than any of the other groupings used in this experiment for the perception of 6 digits, the 3-3 group being definitely the worst.

WEBER'S LAW AS TESTED BY FLOWING INCREMENTS

By A. D. Bush and Alice May Austin

Weber's Law has been examined so often that a further presentation would not seem to be justified unless some new method of testing were utilized. As is well known, the usual method of investigating the validity of this law for pressure has been by using definite multiples and fractions of the initial weight as succeeding stimuli for evoking the secondary sensation. An approach to our method was suggested by Witmer, who proposed using flowing sand; but flowing sand is not easily controllable in small increments, or in rapid variation of rate of increment. So we devised the simple method of using water as the variant for measuring definite appreciations of value, both of pressure and of tension increments.

Method. The apparatus used consisted of a glass cylinder (or burette) 152 cm. long and 2 cm. wide, serving as a head reservoir; and a conducting rubber tube, 2 mm. wide, running from the lower end of the reservoir through a stopcock down into beakers of varying weight. The stopcock could be so adjusted that the flow of water into the beaker was at the rate of 0.5 cc. per sec., or at the rate of 1.33 cc. per sec., or at the rate of 3.33 cc. per sec., or at the rate of 7.33 cc. per sec. The burette was connected with a second reloading reservoir so that the initial head in the service burette could be reestablished before each test. Three beakers were used for the successive tests, each so loaded as to give initial pressures of 100 gm., 150 gm., and 300 gm., respectively; being thus in the ratio of 1:1.5:3.

Two sets of experiments were carried out, one testing appreciation of simple varying pressures, the other testing appreciation of varying pressures when there was also involved the factor of steadily-increasing tendon tension; that is, in the first test, the beaker rested on the subject's hand (a thin layer of cotton intervening), and the hand rested on a cotton-padded board; in the second test, the hand holding the beaker was supported from the wrist only which, in turn, rested on the cotton-padded board.

The subjects were 15 students from the classes in Medical Physiology at the University of North Dakota. With each subject three readings were taken for each of the four rates for each of the three beakers for each sense—pressure and tension.

Results. The final average results are tabulated as follows.

TABLE I

Pressure Averages

	Beaker 1 Initial Weight 100 gm. Increment	Beaker 2 Initial Weight 150 gm. Increment	Beaker 3 Initial Weight 300 gm. Increment
Rate I = 0.5 cc./sec.	14.78 gm.	24.24 gm.	36.83 gm.
Rate II = 1.33 cc./sec.	26.63 gm.	36.65 gm.	44.78 gm.
Rate III = 3.33 cc./sec.	36.98 gm.	47.96 gm.	73.60 gm.
Rate IV = 7.33 cc./sec.	46.98 gm.	63.34 gm.	100.5 gm.

The percentage increment in each case is as follows.

	Beaker 1	Beaker 2	Beaker 3
Rate I	14.78%	16.16%	12.28%
Rate II	26.63 "	24.43 "	14.93 "
Rate III	36.98 "	31.97 "	24.53 "
Rate IV	46.98 "	42.23 "	33.5 "

These findings seem not to be in conformity with either Weber's Law or Fechner's formula.

TABLE II

Average of Muscle Tensions

	Beaker 1 Initial Weight 100 gm. Increment	Beaker 2 Initial Weight 150 gm. Increment	Beaker 3 Initial Weight 300 gm. Increment
Rate I = 0.5 cc/.sec.	11.64 gm.	18.15 gm.	26.71 gm.
Rate II = 1.33 cc./sec.	16.50 gm.	22.87 gm.	33.97 gm.
Rate III = 3.33 cc./sec.	32.89 gm.	48.24 gm.	64.14 gm.
Rate IV = 7.33 cc./sec.	41.50 gm.	57.65 gm.	73.95 gm.

The percentage increment in each case is as follows.

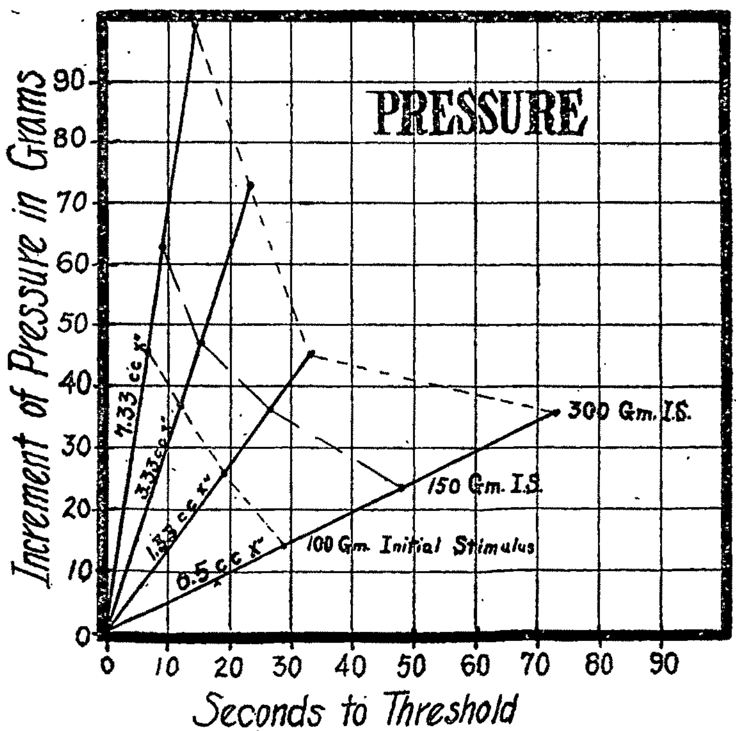
	Beaker 1	Beaker 2	Beaker 3
Rate I	11.64%	12.10%	8.9%
Rate II	16.5 "	15.25 "	11.36 "
Rate III	32.89 "	32.16 "	21.38"
Rate IV	41.5 "	38.43 "	24.63 "

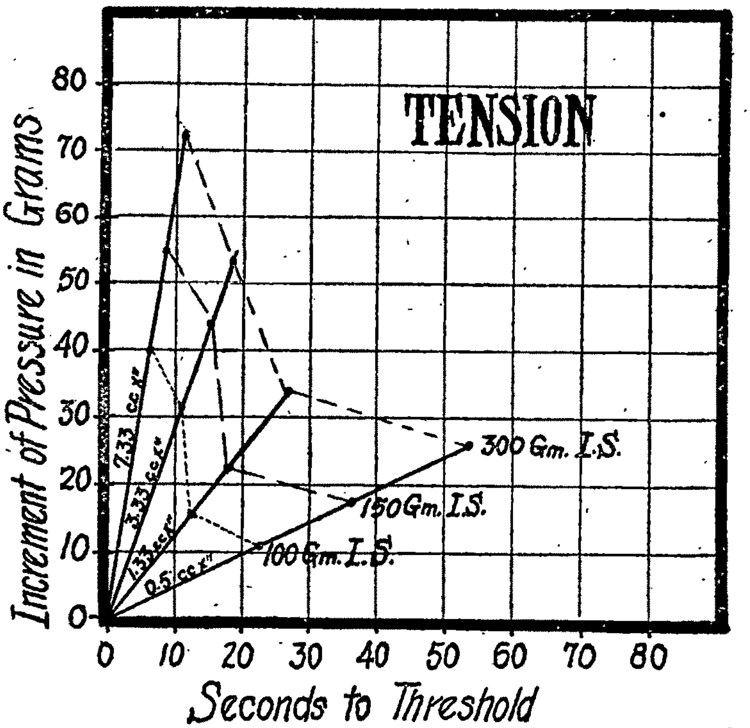
These findings, also, seem not be to in conformity with either Weber's Law or Fechners' formula.

The two following graphs indicate perhaps even more clearly that, in these experiments, the law of Weber did not obtain.

Perhaps one of the most important findings resulting from this investigation is the evidence that the psychophysical reaction is to a measurable degre einfluenced by the gm.-sec. value

of the stimulus. With an initial weight of 100 gm. it required 29.5 sec. for appreciation of an increment of 14.78 gm. if added at the rate of 0.5 gm. per sec., whereas it required a little over 6 sec. for appreciation of an increment of 46.98 gm. if added at the rate of 7.33 gm. per sec., a little over 11 sec. for appreciation of an increment of 36.98 gm. if added at the rate of 3.33 gm. per sec., and a little less than 18 sec. for appreciation of 26.33 gm. if added at the rate of 1.33 gm. per sec. Expressed in gm.-sec. the statement in the preceding statement appears thus: 436.89 gm.-sec., 300.67 gm.-sec., 399.47 gm.-sec., 479.34 gm.-sec. That is, when the initial stimulus was 100 gm., the psychophysical threshold was crossed most quickly when the increment was at the rate of 7.33 gm. per sec., and was most delayed when the increment was at the rate of 1.33 gm. per sec. When the initial stimulus was 150 gm., the threshold was crossed most quickly when the rate was 7.33 gm. per sec. (total equals 544.3 gm.-sec.), and most slowly when the rate was at 0.5 gm. per sec. (total equals 1175.15 gm.-sec.) When the initial stimulus was 300 gm., the threshold was crossed most





quickly when the rate was 7.33 gm. per sec. (total equals 1315.5 gm.-sec.), and most slowly when the rate was 0.5 gm. per sec. (total equals 2712.8 gr.-sec.).

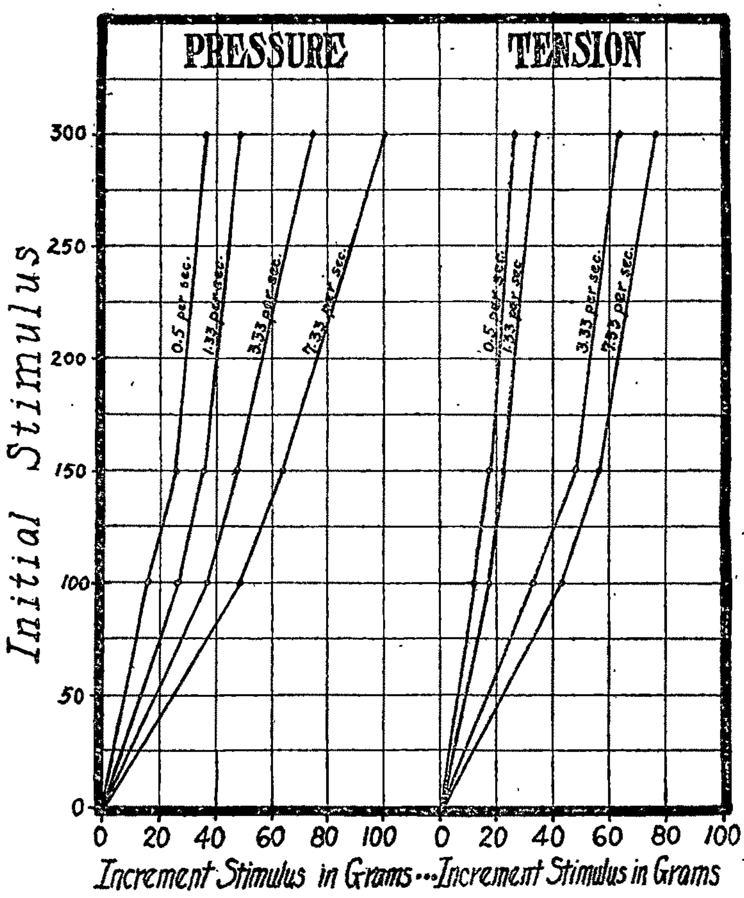
In the series of experiments testing tension, when the initial stimulus was 100 gm., the threshold was crossed most quickly when the rate was at 1.33 gm. per sec. (total equals 198 gm.-sec.), and most slowly when the rate was 3.33 gm. per sec. (total equals 328.9 gm.-sec.); when the initial stimulus was 150 gm., the threshold was crossed most quickly when the rate was at 1.33 gm. per sec. (total equals 381.4 gm.-sec.) and most slowly when the rate was at 3.33 gm. per sec. (total equals 699.6 gm.-sec.); when the initial stimulus was 300 gm., the threshold was crossed most quickly when the rate was at 7.33 gm. per sec. (total equals 739.5 gm.-sec.), and most slowly when the rate was at 0.5 gm. per sec. (total equals 1476.4 gm.-sec.)[1].

That the results obtained by the experiments with water increments are not in accord with either Weber's Law or Fech-

[1]It will be understood that this threshold discussion refers not to elapsed time but to gm.-sec. time.

ner's formula is shown graphically in the two appended curves, each of which summarizes the accrued data; probably they are sufficiently self-explanatory to require no further elucidation.

Summary. A series of experiments wherein Weber's Law and Fechner's formula are tested by means of flowing water increments for the secondary stimuli fail to show conformity to either law or formula. Curves presented show this non-conformity graphically.



EFFECT OF LENGTH OF LIST UPON MEMORY FOR NUMBERS[1]

By Edward S. Robinson and Chester W. Darrow, University of Chicago

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I. Problems and Methods

One method of testing the reliability of experimental results consists in the application of certain statistical inferences based upon the number of observations involved and the dispersion of the quantitative results yielded by those observations. A method which is somewhat simpler than, but by no means exclusive of, the first consists in a repetition of the experiment under the same general conditions and somewhat different detailed conditions. Results that appear in two or more experiments of the same general type not only secure a certain statistical guarantee but also an increase in the generality of their significance. The present experiment is an application of the test of repetition to the results of an earlier experiment conducted in the Chicago laboratory.[2] This second experiment is by no means a complete duplication of the first. If it were, its agreement with the first would scarcely add anything to the implication of their common results.

Each experiment made a simultaneous attack upon four different problems: (1) the relation between the length of material memorized and the form of the curve of memorizing; (2) the relation between length of material and the effort required to memorize it; (3) the relation between length and retention; and (4) the relation between length and susceptibility to retroactive inhibition.

In the first experiment the materials memorized were lists containing 6, 9, 12, 15, or 18 nonsense syllables. In the second experiment the materials memorized were lists of 4, 6, 8, or 10 three-place numbers. In both experiments the exposure time for the single member of a list was 2 sec. In both experiments the anticipation method was employed. That is, after the first

[1]Read in part before the American Psychological Association, December 27, 1923.

[2]E. S. Robinson and W. T. Heron, *J. of Exper. Psychol.*, 5, 1922, 428-448.

presentation of a list, the subject, upon the presentation of any syllable or number, tried to anticipate the next syllable or number in the list. This procedure was employed both in the original learning of a list and in the relearning of it. Two scores were obtained:[3]

$$\text{Recall Score} = \frac{\text{Number of numbers correctly anticipated during first re-presentation of list}}{\text{Number of numbers in list}-1^{4}} \times 100$$

$$\text{Saving Score} = 100 - \left\{ \frac{\text{Number of presentations required on relearning}}{\text{Number of presentations required for original learning}} \times 100 \right\}$$

The time between learning and relearning was 20 min. in Experiment 1 and 15 min. in Experiment 2. This period was devoted either to the memorizing of a second list of syllables or numbers, or to casual reading of the daily paper. Different groups of 10 subjects each were used in the two experiments. In both cases practice effects were reduced, though not eliminated, by 6 preliminary sittings. In Experiment 1 each subject went through all of the conditions 3 times; in Experiment 2 each subject went through all of the conditions 4 times. Of course, the conditions were gone through in varying order by the different subjects. In neither experiment did a subject work under more than one condition on a single day.

Table I displays the plan of our second experiment.[5]

TABLE I

O = Original memorizing of a list of three-place numbers, figures 4, 6, 8, and 10 designating the number of numbers in the list.
I = Memorizing a list of three-place numbers immediately following the memorizing of a first or original list. The figure 6 designates the number of numbers in this interpolated list.
Rest = Casual reading of the daily paper with no attempt to memorize the material read. During this period the subject refrained from rehearsing O or I.
RO = Relearning of original list of numbers. The interval between the completion of O and the beginning of RO was always 15 minutes.

Condition		Interpolated Period of 15 min.	
I	O 4	I 6, Rest	RO
II	O 4	Rest	RO
III	O 6	I 6, Rest	RO
IV	O 6	Rest	RO
V	O 8	I 6, Rest	RO
VI	O 8	Rest	RO
VII	O 10	I 6, Rest	RO
VIII	O 10	Rest	RO

[3]Compare *op. cit.*, 429-430.
[4]The first number was used only as a cue. The subject was not expected to recite it while the last number was before him.
[5]For a tabular plan of the experiment with nonsense syllables, see earlier paper, 431.

II. Length of Material and the Curve of Memorizing

In Figure 1 we have plotted absolute amounts of material recalled against absolute amounts of learning time for the various number lists of Experiment 2. Learning time was measured in terms of the total number of numbers (not necessarily different numbers) presented. Thus two presentations of a 4-number list were considered equal to one presentation of an 8-number list. The curves represent the experiment as a whole. Each curve

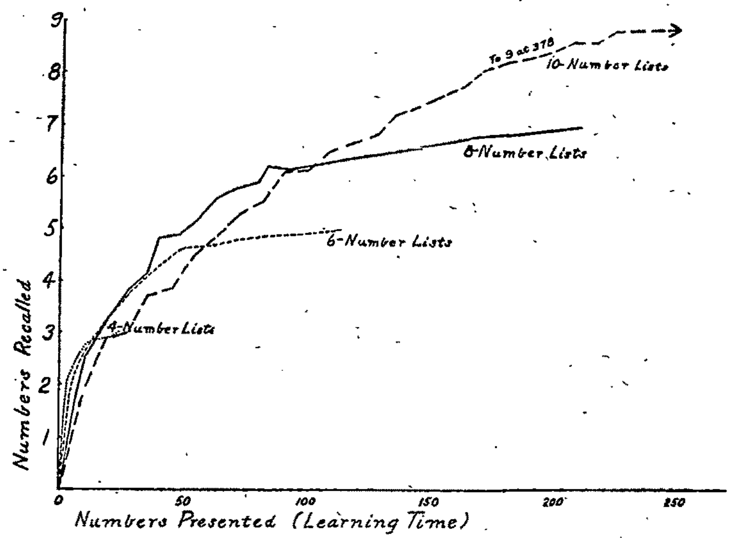

FIGURE 1

therefore is a composite of 80 individual curves. The curves for materials of different length all take the negatively accelerated form frequent among learning curves of most varieties. In this they agree with the curves obtained in Experiment 1, where nonsense syllables were memorized instead of numbers.[6] Perhaps a more important fact is the regular variation of the curves with variation in the length of material. The shorter the list, within the limits of our experiment, the more rapidly learning progresses during its early stages. The presentation of a list of 4 numbers twice, for example, results in the learning of more numbers than does the presentation of a list of 8 numbers once. This finding also is in exact accord with the experiment we were seeking to test. There are at least two possible explanations for the inverse relationship of initial learning rate to length. While the longer lists offer more possible associations, they also offer

[6] *Op. cit.*, 433.

more chances for interference effects, and their presentation during any absolute period of time must of necessity contain a smaller element of repetitiveness than the shorter lists.

Kjerstad concluded from the results of certain experiments of his own[7] that the form of the curve of memorizing is independent of the length of the material memorized. He held this to be true not when absolute study time and absolute amount learned are considered, but rather when the curve for material of a given length is plotted on the basis of the fractions of that material memorized in successive fractions of the total time required to master that material.[8]

In our earlier paper we converted the curves derived by plotting absolute amounts learned in absolute units of learning time into relative curves. That is, we found what fractions of material of a given length were learned in certain selected fractions of the time required to learn material of that length.[9] Our results confirmed those of Kjerstadt. Our method of transforming the absolute curves into relative curves was, however, somewhat different from that of Kjerstadt. We first obtained absolute memory curves from the pooled data of our entire experiment and then converted those curves directly into curves of the relative type. Kjerstadt, on the other hand, transformed the data obtained from the performance of each individual with each list into a curve of the Vincent type. Since the publication of our first paper we have decided that Kjerstadt's method is preferable, principally because the unusual performance and the unusual individual have less effect upon the final result. The total time required to learn a list was, in terms of our previous procedure, the time required for the slowest performance of the slowest learner. But by converting each curve obtained from each individual's learning of each list into a curve of the Vincent type, as Kjerstadt did, and then pooling the 80 Vincent curves thus secured, we obtained for the experiment with three-place numbers the facts shown in Table II.

A study of Table II shows the Vincent curves to be practically identical for lists containing 6, 8, or 10 three-place numbers. So far our findings are in accord with those of Kjerstadt. The curve for lists containing 4 numbers does not, however, conform to the curves for the longer lists. In this case relatively less is learned in the first fractions of the total learning time.

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[7] *Psychol. Monog.*, 1919, 26.

[8] Vincent (*Behav. Monog.*, 1912, 1, 15-17) seems to have been the first to give learning data this particular form of statistical treatment. This type of learning curve has properly been spoken of as the Vincent curve.

[9] *Op. cit.*, 435-436.

TABLE II

Percentages of Lists of Different Lengths Learned in Selected Percentages (sixths) of the Total Time Spent in Learning Those Lists

Percentages of Total Learning Time	Length of Lists: 4 numbers	6 numbers	8 numbers	10 numbers
16.7	19.4	31.8	29.3	30.4
33.3	36.1	44.5	45.9	45.3
50.0	49.8	54.0	55.3	54.6
66.7	63.1	62.7	64.0	64.7
83.3	82.3	71.0	75.8	70.4
100.0	100.0	100.0	100.0	100.0

Since, as we have already said, our method of obtaining the relative curves of memorizing for the nonsense syllables employed in our first experiment was somewhat questionable, we have constructed Vincent curves for memorizing nonsense syllables by means of the method employed for the number lists. The values obtained are included in Table III.

TABLE III

Percentages of Nonsense Syllable Lists Learned in Selected Percentages (sixths) of the Total Time Spent in Learning Those Lists

Percentages of Total Learning Time	Length of Lists: 6 syllables	9 syllables	12 syllables	15 syllables	18 syllables
16.7	23.3	33.9	34.6	30.9	33.1
33.3	45.0	51.2	52.3	52.2	53.2
50.0	60.5	62.0	66.3	62.0	66.3
66.7	72.3	73.6	74.9	74.2	76.3
83.3	83.4	80.8	80.5	80.3	82.8
100.0	100.0	100.0	100.0	100.0	100.0

The relative curves for 9-, 12-, 15-, and 18-syllable lists are practically coincident, but as in the case of three-place numbers there is evidently a lower limit to this law. The curve for 6-syllable lists, like that for the 4-number lists, shows smaller fractions of the total material learned in the first fractions of the total learning time.

III. Length and Difficulty

Figure 2 shows the relationship between the number of numbers in a list and the absolute time required to learn it. The four curves represent different cycles of the experiment. Here again the results are in agreement with those obtained for

nonsense syllables. With increasing length, the time required for learning increases at an increasing rate. The data available from experiments by Binet and Henri, Ebbinghaus, Henmon, Lyon, and Meumann show, with certain minor exceptions, the same increase in difficulty with increasing length.[10] It is true that the relationship between length and difficulty was once

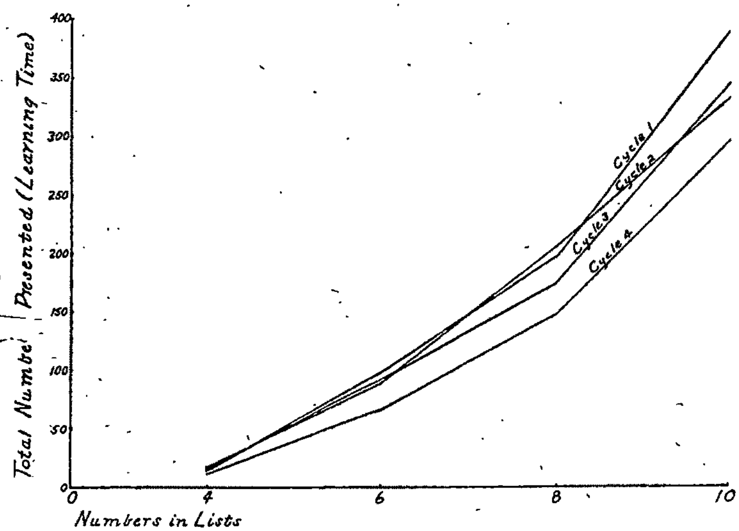

FIGURE 2

debated; but that debate was made possible only because of the procedure adopted by some investigators of discussing the relationship between length and repetitions. Since the repetition is a unit whose value changes with changes in the length of the lists, we may well abstain from dealing with it in this connection. Psychological statistics have horrors enough when they deal with constant units.

IV. Length and Amount Retained

Our experiment with nonsense syllables agreed with the general run of previous work in showing relatively slower forgetting for the longer lists. Table IV contains the percentages of lists of different lengths *recalled* after 15 min. of newspaper reading. Table V contains similar percentages in terms of *saving.*

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[10] *Op. cit.*, 436-441.

TABLE IV

Percentages *Recalled* After 15 min.
$n = 10$ for each value except averages, where $n = 40$

Length of List	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	46.6	66.0	68.5	74.4
Cycle 2	66.6	58.0	64.2	61.1
Cycle 3	50.0	72.0	56.6	71.1
Cycle 4	76.7	70.0	77.2	76.7
Av. of 4 cycles	60.0	66.5	66.6	70.8

TABLE V

Percentages *Saved* After 15 min.
$n = 10$ for each value except averages, where $n = 40$

Length of List	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	−8.6	63.7	58.9	83.9
Cycle 2	56.0	64.9	73.7	74.9
Cycle 3	36.6	86.7	70.0	81.6
Cycle 4	16.0	74.9	75.0	75.3
Av. of 4 cycles	25.0	72.5	69.4	78.9

The clearest tendency, as in the case of nonsense syllables, is in the direction of relatively more forgetting for shorter than for longer lists. This tendency is decidedly more evident according to the *saving* criterion than it is according to *recall*.

V. Length and Susceptibility to Retroactive Inhibition

The reader will remember that the period between learning a list and relearning the same list was sometimes devoted to casual newspaper reading and sometimes to learning another list; that other list, as previously stated, always contained 6 numbers. Following the usual custom we shall call newspaper reading the *rest* condition and learning another list the *work* condition. Now the absolute amount of retroactive inhibition is equal to the

TABLE VI

Differences between Percentages of Different Lists *Recalled* Under *Rest* and *Work* Conditions: Absolute Inhibition
$n = 10$ for each value except averages, where $n = 40$

Length of Lists	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	36.6	48.0	28.5	26.7
Cycle 2	33.3	44.0	34.2	7.8
Cycle 3	10.0	42.0	36.6	27.8
Cycle 4	43.4	46.0	37.3	33.3
Av. of 4 cycles	30.8	45.0	34.1	23.9
Percentages above Differences are of Amounts *Recalled* under *Rest* Condition: Relative Inhibition				
Cycle 1	78.5	72.7	41.6	35.9
Cycle 2	50.0	75.9	53.3	12.8
Cycle 3	20.0	58.3	64.7	39.1
Cycle 4	56.6	65.7	48.2	43.4
Av. of 4 cycles	51.3	68.1	52.0	32.8

TABLE VII

Differences between Percentages of Different Lists *Saved* under *Rest* and *Work* Conditions: Absolute Inhibition
$n = 10$ for each value except averages, where $n = 40$

Length of Lists	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	5.3	34.5	4.7	22.8
Cycle 2	96.1	56.6	10.7	3.1
Cycle 3	31.6	61.2	21.7	23.5
Cycle 4	20.0	34.5	12.9	3.3
Av. of 4 cycles	38.0	46.7	12.5	13.2

Percentages above Differences are of Amounts *Saved* under *Rest* Condition: Relative Inhibition

Length of Lists	4 numbers	6 numbers	8 numbers	10 numbers
Cycle 1	61.6	54.2	8.0	27.2
Cycle 2	169.8	87.2	14.5	4.7
Cycle 3	86.3	70.6	31.9	28.8
Cycle 4	133.3	46.1	17.2	4.4
Av. of 4 cycles	112.7	64.5	17.5	16.3

recall or *saving* under the *rest* condition *minus* the *recall* or *saving* under the *work* condition. The relative amount of inhibition, on the other hand, is the percentage which the above difference is of the *recall* or *saving* under the *rest* condition. Tables VI and VII show the absolute and relative amounts of retroactive inhibition according to *recall* and *saving*.

If we consider the lists containing 6, 8, and 10 numbers, all of our methods of measuring retroactive inhibition indicate with some consistency a decreasing susceptibility to this form of inhibition with increasing length. It is only according to relative inhibition in terms of *saving*, however, that the inverse relationship between length and susceptibility to retroactive inhibition includes the 4-number lists. All other evidence points fairly definitely toward a lower limit to this relationship.

In our experiment with nonsense syllables there was also a tendency toward decreasing susceptibility to retroactive inhibition with increasing length, and there was some evidence for a lower limit to this tendency. The facts are interesting in themselves; but their significance is possibly greater if they are considered in connection with forgetting as it occurs under more normal conditions. There is a perfectly respectable theory of retroactive inhibition which says that this interference effect, while quantitatively more pronounced, is essentially the same sort of interference lying at the bottom of most of our forgetting. If that is so, we might have some reason to expect a relationship between retroactive inhibition and length similar to that between normal forgetting and length. Now we *have* found the most prominent tendency in the one case to be decreasing inhibition with increasing length and in the other decreasing forgetting with increasing length. But for the lower limit to the

more general relationship between length and retroactive inhibition we have found no counterpart in normal forgetting. Why this is so we shall not at this time even hazard a guess.

VI. Summary of Results

1. The absolute curves for memorizing numbers are like those for memorizing nonsense syllables in regard to the following particulars. (*a*) The curves for lists of different lengths all show regular, negative acceleration. (*b*) The shorter the list, the more rapidly does learning proceed in its early stages.

2. A consideration of the percentages of each list learned in selected percentages of the total time required to learn that list reveals practically coincident curves for lists containing 6, 8, and 10 numbers, and another set of practically coincident curves for lists containing 9, 12, 15, and 18 nonsense syllables. Lists containing 4 numbers and lists containing 6 syllables fail to conform to this law.

3. The amount of time required for memorizing increases at an increasing rate as the number lists become longer. The same law holds for nonsense syllables.

4. There is a general, but not very consistent tendency for the rate of forgetting to vary inversely as the length of the number lists. A similar tendency was found for nonsense syllables.

5. In general there is a decreasing susceptibility to retroactive inhibition as the number-lists increase in length. There is good evidence, however, of a lower limit to this law. These facts are in accord with those found for nonsense syllables.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CLARK UNIVERSITY

Communicated by EDMUND C. SANFORD

XXVII. A STUDY OF 100 CONSECUTIVELY RECORDED DREAMS[1]

By ATTILIO RIZZOLO

I. *Dream Facts*

These 100 dreams were collected between Nov. 21st, 1921 and Feb. 27th, 1922 by the simple expedient of keeping writing material at the bedside, making rough notes of the dream at once upon waking, and writing up these notes next morning as fully as memory would permit, at which time also explanatory data when available were jotted down. While material of this sort is, of course, inadequate for a thorough statistical study of dreams, both because of the limited number recorded and because the dreams are those of a single dreamer, it is, nevertheless, quite sufficient for a test of the adequacy and universality of current dream theories and for a rough indication of the variety and complexity of the phenomena to be accounted for.

The material gathered has been studied with reference to *excitation* (*i e.*, the extent to which the dreams or features in them could be traced to sensory stimuli received during sleep), to *motivation* (*i.e.*, the extent to which the dreams were colored by desires or other emotions), with reference to their *effect* in producing noticeable movements, to the involvement of *'higher' mental processes*, to their *logical or absurd character*, to the *chronology of the influences* determining them, and to such general conditions as *fatigue*, *wakefulness*, etc. The main categories together with typical examples follow.

(1) *Dreams excited in whole or in part by sensory stimuli.* Of this familiar type the collection offers a few examples. Dream 17 (first part) is typical:

> The dreamer dreams that his small sister is playing an old melody upon a piano which his people have recently purchased. At the conclusion of the melody the dream changes.

[1]This paper is an abstract of a thesis submitted in partial fulfillment of the requirements for the degree of Master of Arts at Clark University, 1922. The thesis in full (pp. 108, with bibliography of titles in English) is on file in the Library of Clark University and the complete protocol of the dreams is in the archives of the Clark Psychological Laboratory.

Remarks. The dreamer was awakened about 3 a. m. by the playing of a piano in an adjoining room. Although the disturbance continued, he fell asleep after 15 or 20 min. and dreamed the above dream.[2]

In four instances such a causation seems reasonably clear. It may have been operative in three others, but a positive statement is impossible.

(2) *Dreams involving a carrying over of experiences and emotions from the waking state to the dream.* Of these dreams two sorts may be distinguished: one in which the dream repeats with some exactness an experience of the preceding day or hour—thus one in which 'recency' may perhaps be a factor—and another in which the dream falls in with, or gives expression to, a more or less persistent mood or emotional attitude—thus one that would follow the laws of 'frequency' and 'vividness'.

Dreams of the first sort are fairly numerous among the 100 recorded. As an example dream 64—which is also interesting as seeming to be nearly free of the element of desire—will serve.

The dreamer dreams that his room-mates are discussing the teaching methods of a certain professor. They describe, criticise and condemn the methods. The dreamer takes no part in the discussion and passes no judgment upon either the methods or the criticisms made by his room-mates.

Remarks. During the preceding day certain remarks were made by the dreamer's room-mates in reference to the teaching methods of the professor in question. The room-mates maintained that the methods were ineffectual. The dreamer listened to the remarks but did not give his opinion, remaining silent throughout the entire discussion.

Dreams in which an emotional attitude has been efficient in determining the dream content were also very common. In most instances the emotion can, for our purposes, be regarded as some sort of a desire. The term 'desire' belongs, of course, to popular psychology and so lacks precise definition. It stands nevertheless for a well recognized emotional condition and one to which great importance has been assigned in some recent dream theories. The writer will use the term here in a broad way for all mental processes characterized by unrest, discomfort or dissatisfaction,—for all those processes which would tend, if they resulted in action, toward the removal of the disturbing features of the situation. It is evident that under such a definition the only mental states that would *not* be states of desire would be those of rest, comfort and satisfaction,—such processes as would lead, if they should result in action at all, to action favoring the preservation of the *status quo.* He will re-

[2]The wording of the dream records as here presented, as well as that of the text in general, has been revised by Dr. Sanford, but the revisions in the former have been carefully checked over by the writer and have been accepted only when found to convey the full intent of the original record without distortion. The records filed in the Clark Laboratory remain, of course, as originally worded.

gard a dream as motivated by desire when the dream, if it were real, would be in some degree a satisfaction of the desire in question: a dreamer, for example, goes to bed hungry and dreams of a feast. Within the general class of desire-dreams it will be convenient for purposes of presentation to recognize several sub-classes depending, first, upon the extent to which the satisfying of the desire is impeded in waking life; secondly, upon its intensity; and thirdly, upon its object. The next five sections will therefore deal with dreams motivated by: (*a*) simple desires; (*b*) repressed desires; (*c*) suppressed desires: (*d*) impatient desires; and (*e*) desires of particular sorts.[3]

(3) (*a*) *Dreams motivated by simple desires.* The simple desire is always of relatively low intensity and lacks the element of anxiety. It is also usually of short duration. As an example, dream 40.

The dreamer dreams that the piano in his home has been sent to a piano factory for repairs. In this dream he sees the workman putting in new keys, tuning the piano, polishing it, etc.

Remarks. On returning home from the University the dreamer notices that the piano needs repairing and speaks to his sister of wishing to send it to the factory. His sister agrees with him as to the need and both decide to wait for an opportune time to send it. The desire does not rise to any considerable degree of intensity.

(3) (*b*) *Dreams motivated by repressed desires.* Dreams of this sort are extremely numerous in the present collection and a large number could be cited. Instead, however, of offering further miscellaneous examples the writer ventures to give the following seven dreams from a longer sequence, which are interesting in having for motivation a single persistent repressed desire which was at first of high intensity, but later ebbed, and was finally replaced at least temporarily by an emotion of contrary character. He apologizes for their intimate nature. It is sufficient to say, by way of general introduction, that a few weeks before taking up the study of dreams the dreamer had been jilted by his sweetheart. The dreams are at first motivated by an intense desire to regain her favor. In the full thesis,

[3]By a *repressed* desire the writer means a desire the prompt fulfillment of which in waking life is recognized as highly improbable. It differs, on the one hand, from a *simple* desire (*i.e.*, from a natural desire whose fulfilment is confidently looked for in the natural course of events) and, on the other hand, from a *suppressed* or hopeless desire,—a desire the fulfilment of which is regarded as so completely impossible that it no longer tends to modify action. These three classes pass into one another, of course, by imperceptible gradations; a simple desire becomes repressed where its satisfaction is unexpectedly postponed, and may even become suppressed when circumstances are unfavorable (in which case it usually disappears entirely); while a repressed desire may waver between the simple condition and suppression, as hope rises and falls.

the sequence consists of 19 dreams: 8 from the first quarter of the records; 4, 3 and 4 from the succeeding quarters.

Dream 2. The dreamer dreams that he is attempting to regain the love of his former sweetheart. She responds by telling him never to speak to her again.

Remarks. The dreamer was in a state of high nervous tension. Since the moment she left him he had endeavored to discover the motive for her act.

Dream 10. The dreamer dreams that he meets his former sweetheart in a playground where both had worked together. He reproaches her for her conduct toward him, whereupon she begins to weep. Seeing her in tears the dreamer apologizes for his reproaches. His apology is accepted and he asks her to meet him at once in the auditorium. She accepts the invitation and meets him there. Feeling secure from suspicious persons they embrace each other.

Dream 11. The dreamer dreams of a certain doctor and his son. The son is driving a Fiat car through the street on which the dreamer's former sweetheart lives. The sight grieves him, for he fears that she will fall in love with the doctor's son.

Remarks. In the preceding waking state the dreamer was reminded of the doctor and his son, and immediately afterward felt again the desire to regain his lost sweetheart.

Dream 19. The dreamer dreams that in order to regain the love of his former sweetheart he courts another lady, hoping to make her repent her former action and return to him. As he is in the act of kissing this second lady his sweetheart interrupts him. She is grieved and begins to cry. The dreamer seeing the success of his plan embraces and kisses her. She reciprocates.

Remarks. In the evening preceding the dream the dreamer had thought of trying a stratagem like that executed in the dream.

Dream 46 (first part). The dreamer dreams that he encounters his former sweetheart. Ignoring him, she approaches another young man whom the dreamer recognizes as Mr. Z. whom he had met during the preceding waking state. Her conduct with Mr. Z. is rather free and she finally embraces him. This is a great shock to the dreamer. He is depressed and concludes that he will never be able to regain her love.

Remarks. During the day preceding, the dreamer was told by his sister that his former sweetheart would never return to him. Feeling depressed the dreamer set out for a walk through a certain park. As he was walking Mr. Z. approached him. In the conversation which followed he discovered that his former sweetheart had corresponded with this young man. Putting this with his sister's statement he begins to fear that his former sweetheart will indeed never return to him. He is still in this mood when he goes to bed.

Dream 75. The dreamer dreams that he is attending one of Dr. B's lectures. After the lecture as he walks toward the University building, he meets his former sweetheart whose face is blackened with charcoal. She endeavors to speak to him, but he ignores her and continues his walk toward the building. On entering he discovers that his people are living in the gymnasium and that they are making preparations for a gorgeous feast. Different kinds of meat and pastry are strewn over the floor. After 10 or 15 min. in the gymnasium the dream changes.

The dreamer dreams that he is attending a lecture on the nature of man by Dr. X. His sweetheart is also at the lecture. She tells the dreamer that she is there to see him reduced to insignificance by Dr. X. During the first

part of the lecture, the dreamer makes no comment on what the lecturer says, but during the second part he challenges every statement. He tells the lecturer that he knows nothing of human nature, that he is nothing but a statistical sociologist, and that since he has never taken courses in anatomy, physiology, neurology, embryology, physical anthropology, heredity, biology and abnormal psychology, he should not attempt to explain human behavior. After having reprimanded the professor for his rash statements, the dreamer notices that his former sweetheart is laughing at him. Angered at the thought of being an object of ridicule in the mind of his former sweetheart the dreamer leaves the room. As he is descending a flight of nearby stairs he discovers that he is wearing his pajamas and bath robe. The shock produced by this discovery wakes him up.

Remarks. In the evening before this dream the dreamer had had a digestive upset and was in a condition bordering on neurasthenia from overstudy.

Dream 100. The dreamer dreams that his former sweetheart is attempting to regain his love. She approaches him and attempts to embrace him. As she is about to do so, he pushes her aside and expresses his desire never to see her again.

Remarks. During the day preceding this dream the dreamer experienced a strong aversion to his former sweetheart. The thought of having been unjustly jilted was the basis of the aversion.

(3) (*c*) *Dreams motivated by suppressed desires.* As an example of a dream embodying a suppressed desire the following is cited:

Dream 4. The dreamer dreams that he has received a book which was promised to him by one of his friends but which was never received. He received the book indifferently in spite of the fact that before the suppression of the desire for it he had impatiently awaited its arrival.

Remarks. After having looked for the arrival of the book for three or four weeks, the dreamer received a letter from his friend expressing his inability to lend or to give him the book. The dreamer became angry and determined to forget both the friend and the book. As time passed, though incapable of forgetting his friend, he did actually forget the book. The suppressed and forgotten desire obtained satisfaction in the dream.

Desires of a high degree of intensity are rarely wholly suppressed—they are as enduring as hope—and so dreams motivated by suppressed desires are rare in the present dream collection.

(3) (*d*) *Dreams motivated by intense or impatient desires.* As an example of this class the writer offers dream 15.

The dreamer dreams that he has received from his mother jars containing a special kind of chicken soup thickened with a special kind of spaghetti. The sight of the jars delights him. He opens one of them and partakes of its contents.

Remarks. The dreamer, suffering from a chronic intestinal trouble, had had a special diet prescribed for him by his physician, which included the soup mentioned. The dreamer gave the prescription to his mother who promised to send him the soup after his return to Clark University. After his return the dreamer became intensely anxious to receive the soup. From early in the morning till late in the afternoon he was on the look-out for a parcel containing it, and as days passed without its arrival the desire gained still more intensity. Finally it induced the dream recorded.

(3) (*e*) *Dreams motivated by sex desires.* This sort of desire stands out in some measure from the rest and to it, in the dream theories of Freud especially, considerable importance has been attached. Here it will be convenient, after the manner of the biological studies of sex differences, to separate the primary sex desire from the various secondary sex desires, the former expressing the reproductive instinct in its immediate and undeveloped form, the latter in its remote transformations and sublimations.

In the present collection of 100 dreams 6 are dominated by sex emotion of the primary sort. In 5 of the 6 the desire for primary sexual gratification had been present during the day preceding the dream. In a single case the preceding sex desire was of the secondary sort. In 3 of the dreams the full sexual act was dreamed as accomplished; in 3 it was dreamed as having been prevented, once by the entrance of a third person and twice through a moral struggle.

Of dreams motivated by secondary sex desires, such as romantic love, or desire for the society or friendship of the other sex, the collection shows a number of instances. Examples from the largest single group of these, those having reference to the lost sweetheart, have been already given. The following, dream 9, may serve as a further instance:

The dreamer dreams of a teacher who was fond of him when he was a boy of eleven. He pays her a visit at her school. She is delighted to see him and invites him to stay. He accepts the invitation and stays till school closes. After the children are gone he takes a short walk with her. While they walk a conversation begins in which each relates interesting experiences to the other. After the walk the dreamer escorts the teacher to her home.

Remarks. As the dreamer lay awake very early one morning he thought of a certain teacher. This thought brought to mind the teacher who appeared in the dream and the dreamer fell asleep with the thought of the latter in his mind.

(4) *Dreams resulting in muscular movement.* The number of dreams which were noted as leading to overt muscular movement was very small, probably through failure in observation. Such movement was present, of course, in the primary sex dreams and may be inferred with a good deal of certainty in dreams which ended in a "shock" of any sort; but it is recorded in only one or two instances.

(5) *Dreams involving the 'higher' centers.* Several dreams present the dreamer as taking part as speaker and listener in discussions of highly technical subjects, and maintaining his positions with pertinent arguments. The following are examples from a considerable number which might be given.

Dream 42. The dreamer dreams that he is discussing a phase of abnormal psychology with a group of friends. He is asked whether the corti-

cal cells which have remained undeveloped in the brain can be stimulated to normal development. He answers that there is a possibility, but that the stimulation of the undeveloped cells depends upon the stimulation of the normally developed cells, and this in turn on many conditions. He mentions two: first, whether the arrested development is due to heredity; and, second, whether it is due to internal environmental factors (*i.e.*, congenital factors) or to external environmental conditions (*i.e.*, factors which influenced the individual after birth). He asserts that if the factors are hereditary, the possibility of stimulating the arrested cells to normal development is small, in most cases *nil*; but that if the factors are environmental, the possibility is greater.

Remarks. During the evening preceding the dream the dreamer had been reading Donaldson's "Growth of the Brain", and while reading the account of the development of the cells had formulated the above hypothesis as regards the undeveloped cells. On going to bed the hypothesis still possessed his mind and he earnestly desired to know its validity.

Dream 43. The dreamer dreams that he is pondering over the hypothesis presented in the preceding dream. He criticizes it in every respect. At times he finds a possible fallacy in it; at other times he is inclined to believe in it. After weighing and considering its merits and defects he finally gravitates toward acceptance.

Remarks. During the day preceding the dream the dreamer continued his reading in Donaldson; was reminded of the hypothesis which he had formulated, and began to question and to doubt it. He remained doubtful through the rest of the day and had the doubt still with him when he fell asleep.

Dreams of this character, involving as they do such activities as comparison, criticism and the marshalling of arguments, are hardly comprehensible without the functioning of the 'higher' cortical centers. The quality of the functioning is, however, another matter. If there is thinking in dreams, as our collection seems certainly to show, it is, at least in the case of the present dreamer, something considerably short of thinking at its best.

(6) *Dreams showing confusion and absurdity.* The following may serve as an example:

Dream 56. The dreamer dreams that he is going to Detroit. He hires a small boat for the purpose of making the trip, and rows through canals, rivers and lakes until he comes to Boston. There he decides to make the rest of the journey by train. While waiting for his train he chances to cast his eyes toward the harbor and notices a peculiar thing. The gasoline tanks of the garages of Boston and its vicinity are located in the harbor a few hundred yards from shore. They look like floating cylindrical pontoons. At first he is puzzled as to what they are, but is told later by a passer-by that they are gasoline tanks. The train comes in on time and the dreamer leaves Boston. Throughout the entire dream the dreamer is conscious of his purpose to go to Detroit to see a friend who lives there.

Remarks. During the day preceding the dream the dreamer had had a strong desire to see his friend in Detroit. He had promised the friend to pay him a visit, but conditions had made the trip impossible and he realized this impossibility. During the same day he had inquired as to the various ways of going to Detroit. Some of his friends advised him to go by train, while others advised him to go from New York to Boston by boat and from Boston to Detroit by train. Desiring to make the trip as cheaply as possible he was inclined to the latter route.

(7) *Chronology of experiences upon which the dreams were based.*

Dreams based on immediate sensory experience..............1[4]

Dreams based on experiences occurring for the first time during the waking period immediately preceding the dream...............46

Dreams based on experiences occurring more than one day, or waking period, before the dream:

Interval of	2 days	2
" "	3 "	2
" "	2 weeks	2
" "	3 "	4
" "	3 to 4 weeks	1
" "	1 month	10
" "	2 months	2
" "	3½ "	1
" "	5½ "	1
" "	7 "	2
" "	3 years	1

Dreams based on experiences of childhood 3

Dreams based on experiences occurring for the first time during the preceding waking period and on experiences occurring within an interval of 1 year..5

Dreams based on experiences occurring for the first time during the preceding waking period and on experiences of childhood....................1

Dreams based on experiences occurring within an interval of one-month and on experiences of childhood...............................2

Dreams based on experiences which cannot be accounted for in sequence of time..5

II. *Dream Theories*

The first and perhaps the strongest impression produced by a large collection of dreams is the variety and complexity of the phenomena to be explained by any universal dream theory. The dream experiences are as varied as the experiences of waking life—more varied, indeed, since they include the absurd and the impossible. On this aspect of the matter the writer finds himself in complete accord with Prince, who expresses himself thus in the introduction to Mrs. Arnold-Foster's "Studies in Dreams":[5]

> "The fact is, Mrs. Arnold-Foster hits the nail on the head when she says 'there are dreams and dreams, and we must get rid of the assumption that they all resemble each other.' This assumption is a very common one: in particular it is often assumed that a dream implies incongruity or incoherence, or the grotesque, or logical anarchy. Dreams as the author, Mrs. Arnold-Foster, stresses, may not only exhibit orderly imagination, reasoning, and memory and other qualities of the mind, but this imagination, reasoning and memory may be highly constructive, ingeniously inventive and producing imaginings or romances comparable in structure and sequence of ideas to stories of fiction and of real life evolved by the waking mind....

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[4]Although several dreams of this sort were classified under this head for Section (1) above, all except one contained so many other elements that they have been placed in other groups in this chronology.

[5]Arnold-Foster, *Studies in Dreams*, 1921, 10 f.

"What is still needed, as the author has pointed out, is systematic and accurate recording of....dreams....and the correlation of the phenomena with identical phenomena occurring in certain other states of the waking mental life. If this were done, we should be surprised to find what a great variety of forms and structure dreams have, how greatly they differ in type, and in the mental processes involved, We thus should have the material from which we could safely construct theories of mechanisms that would satisfy the different types. After the collection of this varied material we could then begin, with greater safety, to analyse and interpret. In some we should find symbolism, in others none; in some repressed wishes, in others unrepressed wishes, or fears, doubts and scruples; in some sex urgings, in others the urgings of one or more of the other various innate instincts, which are the prime movers of human behavior; in some the solution of problems which have baffled our waking consciousness, in others the mere illogical fantasies of a weak, dissociated extract of our mental selves; in some the reproduction of memories, and living over again in the realistic form previous actual experiences; in others imaginary episodes of apparent super-knowledge constructed out of previous information; in some incongruous, grotesque phantasmagoria in cinema-like scenes, in others romances or well constructed fantasies requiring for their invention a large system of thought and an intelligence and imagination comparable to the waking self-consciousness. And in some we should find that the dream, as in waking life, is only the manifested expression of deeper-lying subconscious processes; and in some, probably in most, that it is just what it appears to be—nothing more."

The most prominent of current dream theories is that of Freud. For him the dream is always a wish-fulfillment, sometimes simple and direct, as in the dreams of children, but more often in the case of adults (since the wishes that find dream expression for adults are often so crude and primitively sexual as to clash with deep-seated moral or other standards) appearing in transmuted and symbolic forms. Adult dreams owe much of this peculiar character also to the fact that the wishes underlying them run back to early childhood.[6] That many dreams are motivated by a wish or desire of some sort our collection abundantly demonstrates, and in so far supports the Freudian view; but it also shows a considerable group in which no certain element of desire can be traced—dreams which appear to be merely the re-living of casual waking experiences. Freud also recognizes the occurrence of such 'sober dreams,' but holds them

[6]Freud's theory is thus briefly stated by Woodworth (*Psychology*, 507): "All dreams are wish fulfillments, even those that seem mere fantastic play of imagination, since no mental activity can accrue except to gratify some wish. All dreams are fulfillments of suppressed wishes and these are either sex or spite wishes, the spite wishes growing out of the interference of other people with our sex wishes."

H. Ellis (*The World of Dreams*, 165) summarizes it thus: "Freud goes far beyond the fundamental....proposition that the dream-imagery is largely symbolic. He holds that behind the symbolism of dreams there lies ultimately a wish; he believes, moreover, that this wish tends to be really of more or less sexual character, and, further, that it is tinged by elements that go back to the dreamer's infantile days....Thus Maeder puts it,

(*Footnote 6 continued on pape 253*)

to be a small minority.[7] Dreams which are simple and undistorted wish-fulfillments he regards as characteristically the dreams of children. But our collection seems to show him to be in error here, for three-quarters of our dreams are of this undistorted sort. Much the same must be said with reference to the presence of factors derived from the experiences of childhood. In the undistorted dreams such factors are extremely rare. Whether they are more frequent even in the distorted dreams is certainly an open question for anyone who is not already a convinced adherent of Freud.

In the full thesis a summary of a considerable number of dream theories from the current literature of the subject follows at this point. Most or all of them set forth views in some measure at variance with those of Freud, and nearly all would find support in some measure in the 100 dreams of this collection.

An appendix deals with the relation of the frequency and character of the dreams to the length of the preceding period of sleep and to the physical condition of the dreamer. It is obvious that in order to record a dream the dreamer must rouse himself sufficiently to take pencil and paper and write; and dreams slip from memory with extreme facility. It is practically certain therefore that every recorded dream is a dream which occurred not long before waking. If, further, the dreamer wakes spontaneously (as was the case for all the dreams of our collection except one), it is only dreams which occur after a period of sleep sufficient to allow the dreamer to wake spontaneously that get into the record. Whether the earlier and deeper stages of sleep are dreamless or are attended by dreams that are forgotten our records furnish no basis for conjecture. It is interesting, however, to consider the length of the sleep period preceding the recorded dreams and the relation of the latter to

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summarising Freud's views, 'behind the insignificant events of the day utilized in the dream there is always an important idea or event hidden. We only dream of things that are worth while'...."

Freud himself (*General Introduction to Psychoanalysis*, 1920, 161) denies that he regards all dreams as sexual. He says: "You must surely have heard that in psychoanalysis it is always maintained that all dreams have a sexual meaning. Now you yourselves are in a position to form a judgment as to the incorrectness of this reproach. You have become acquainted with the wish-fulfillment dreams, which deal with the satisfying of the plainest needs, of hunger, of thirst, of longing for freedom, the dreams of convenience and of impatience and likewise the purely covetous and egoistic dreams. But that the markedly distorted dreams preponderantly ....though again not exclusively—give expression to sex wishes, is a fact you may certainly keep in mind as one of the results of psychoanalytical research."

[7] *Op. cit*, 75.

the physical condition of the dreamer, and this our records permit. A study of the data seems to justify the following conclusions:

(1) When only one dream occurs on a given date and the physical condition of the dreamer is normal the average amount of sleep preceding the dream is about 4 hrs.

(2) When only one dream occurs on a given date and the dreamer is suffering from either fatigue, emotional excitement, physical pain, insomnia, physical or nervous exhaustion—or from a combination of these disturbances—the average amount of sleep preceding the dream is nearly 5½ hrs.

(3) When the dreamer went to bed in a state of considerable fatigue he was more apt to dream than when his condition was normal. When normal he usually recorded but one dream a night; when fatigued he noted two dreams, and on some dates three.

Comparing the normal and fatigued conditions in the light of these conclusions, we seem justified in saying that when the dreamer was fatigued he slept for a longer period before reaching a stage at which he could record a dream, but that, having recorded one, he was more apt to record one or two more. It is perhaps a reasonable conjecture that this difference points to a difference in the shape of the sleep curve (depth of sleep) in the two conditions.

A study of the character of the dreams—logical or absurd—in connection with the physical state of the dreamer on going to sleep seems to justify the following statement. Out of 105 dreams[8], 55 occurred when the dreamer was in a normal condition and 50 when he was suffering from fatigue or some other untoward condition. The proportion of illogical, jumbled and absurd dreams in each class is as follows:

Normal condition: 55 dreams, of which 4 were absurd and 2 partly so.
Abnormal condition: 50 dreams, of which 15 were absurd and 3 partly so.

The abnormal conditions may be further specified as follows:
Moderate fatigue: 23 dreams, of which 3 were absurd and 1 partly so.
Extreme fatigue: 23 dreams, of which 12 were absurd and 2 partly so.
Emotional excitement or physical pain: 4 dreams, all quite logical in character.

If we take all cases together, it is clear that nearly all of the present writer's dreams were logical if his general state was normal, and that the proportion of illogical, absurd or jumbled dreams increased rapidly with increasing fatigue. For normal conditions the ratio of logical to absurd dreams was about 9:1; in moderate fatigue about 6:1; while in extreme fatigue the ratio was reversed, the absurd dreams preponderating in about the proportion of 5:3.

[8]The number 105 includes 5 dreams not considered in the earlier sections of this paper because they were too incompletely remembered to be written out. The dreamer knew that he had dreamed something jumbled or absurd, but could recall no more.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF THE UNIVERSITY OF PITTSBURGH

Communicated by GILBERT J. RICH

I. FURTHER DATA FOR AN ASSOCIATIVE LIMEN

By J. L. ERNST, F. E. SMITH, L. R. MOESSNER, E. S. RUDISILL and M. J. ATWATER[1]

Williams[2] has shown that a modification of psychophysical procedure may be used in the measurement of the strength of association, and that the 'mneomometric function' and statistical limen furnish useful measures of associative strength. It is needless to repeat here his arguments in favor of such procedure. Williams, further, gave data derived from a short experiment by the method he proposed, and attempted so to state the unit of measurement as to fit the *phi-gamma* hypothesis to his curves. His work was admittedly tentative in its nature. The purpose of the present study is twofold: first, to gather additional data for the calculation of an associative limen, and, secondly, to try out a number of mathematical procedures in the statistical treatment of the data obtained. We have extended the previous experimental work by obtaining data from a considerably greater number of observations, and by obtaining 'mnemometric functions' for speed of presentation as the variable as well as for number of repetitions as the variable. We have then applied a number of mathematical procedures to the computation of a limen and tested the accuracy of fit of the curves so obtained to the observed data.

Four *O*s took part in the experiment, all of whom were graduate students in psychology with at least one year of laboratory training. E and S acted as *E*s for each other, as did M and R.[3] Every *E* made up his own series of nonsense syllables, so that as *O* he learned syllables with which he had no previous acquaintance. The 'mnemometric functions' for variable number of repetitions as the measure of associative strength were obtained from *O*s S and M, the functions for variable speeds or presentation as the measure from *O*s E and R.

The material consisted of series of 10 nonsense syllables, constructed to Gamble's modification of Müller and Schumann's rules.[4] The general procedure was that of the *Treffermethode*. The *O* learned a series of 10 syllables in trochaic rhythm at a given rate and for a given number of repetitions. He was not told in advance the number of repetitions or the speed of presentation (whichever was the variable), so that his attitude in learning was kept as constant as possible. Beginning 15 sec. after the last repetition, the *O* was presented with the first, third, fifth, seventh, and ninth syllables

[1]Messrs. Ernst, Smith, Moessner and Rudisill performed the experimental work; Miss Atwater made the computations.

Our thanks are due to Dr. E. G. Boring of Harvard University for his advice during the prosecution of the work.

[2]H. D. Williams, On the Calculation of an Associative Limen, this JOURNAL, 29, 1918, 219ff.

[3]The experiments of E and S were performed in 1920–21; those of M and R during 1921–22.

[4]E. A. McC. Gamble, *Psychol. Rev. Monog.* 43, 1909, 18 ff.

of the series just learned in haphazard order, and was required to respond to each syllable with its associate. The associates were scored as right, half-right, or wrong.

The syllables used by E and S were typewritten on discs and presented stepwise in a Ranschburg memory-apparatus governed by a metronome. At the conclusion of every repetition, *E* turned the disc until he again came to the beginning of the series, and started the apparatus after such a time that each repetition was separated from the next by the time required to expose 4 syllables. The syllables of the test series were typewritten on separate slips of paper, which were laid before the *O* one after the other in making the test.

The syllables used by M and R were presented stepwise on a Spindler and Hoyer exposure apparatus. The series of typewritten syllables were presented on a 14-syllable drum, so that, with the drum running continuously, each repetition was separated from the succeeding by the time required to expose 4 syllables. The test series was placed on the same piece of paper, but each series (the learning series and the test series) was covered from view while the other was being presented. For all *O*s the successive members of the test series followed one another as rapidly as the *O* made his response or indicated that no associate was forthcoming.

The instructions to E and S were as follows. "After the signals 'ready' and 'now' there will be presented to you successively a series of nonsense syllables. This series will be on the revolving card of the machine and you will see one syllable at a time. You are to fixate the window and pronounce the syllables in trochaic rhythm as they appear. Give full and equal attention to every syllable. After a number of repetitions, the first members of the pairs will be presented. When the syllable is presented, pronounce as quickly as possible the syllable which followed it in the series, After you have pronounced it, write it down. If you do not know the syllable which followed, say 'Don't know,' and put a dash on your paper." For R and M, these instructions were modified to meet the type of apparatus used.

Every *O* learned from 100 to 200 series of syllables by way of practice before proceeding to regular series. These practice-series were used to determine the most advantageous speeds and numbers of repetitions for our purpose of computing limens, and these two factors were varied during the preliminary work.

In those experiments in which number of repetitions was the variable quantity, 5 numbers of repetitions were used, namely, 2, 4, 6, 8, and 10 repetitions. The rate of presentation was kept constant at .75 sec. per syllable for S and .8 sec. per syllable for M. S learned 40 series (or 200 pairs of associated syllables) at every number of repetitions, and M learned 50 series (or 250 pairs of associated syllables) at every number of repetitions.

In the experiments with speed of presentation as the variable, every series was learned for 5 repetitions. For E, the 5 rates of repetition used were .50, .75, 1.00, 1.25 and 1.50 sec. per syllable (120, 80, 60, 48 and 40 syllables per min., respectively). These same speeds were tried for R in the preliminary work. It was found, however, that his percentages of correct responses for the two slowest speeds, 1.25 and 1.50 sec. per syllable, were less than the percentages of correct responses for 1.00 sec. per syllable and higher speeds. Since this result is in conflict with previous experimental work, which has shown that strength of association varies inversely with speed of presentation,[5] we asked the *O* for an introspective account of his attitude in learning at various speeds. He reported that, while learning at the slowest speeds, the time between the pronunciation of one syllable and the appearance of the next syllable was so long that his attention

[5]R. M. Ogden, *Arch. f. d. ges. Psychol.*, 2, 1903, 93 ff.

wandered from the series to other ideas. He was not, then, learning the slow series with the same full attention with which he learned the rapid series. This report is in line with the accumulating evidence that attention is a condition of association.[6] We were concerned, however, in this study with strength of association as conditioned upon rate of presentation; and it was, therefore, necessary to keep all other conditions constant, especially the *O*s' attitude, attentional or otherwise.[7] From the report quoted above it appears that, for rapid rates of presentation to this *O*, attitude is a function of rate. What function it may be is a separate problem. We could not allow two variables, rate and attitude, to enter into our determinations, even though one may be a function of the other. We felt justified, therefore, in eliminating for this *O* the speeds at which his attitude was not constant. The 5 rates of presentation used in the experiments with R were: .50, .65, .80, .95, and 1.10 sec. per syllable (120, 93.1, 74.9, 63.1 and 54.5 syllables per min., respectively). E learned 40 series and R 50 series at each rate of repetition.

The primary results obtained are four 'mnemometric' functions, that is, percentages of right associates for every number of repetitions or for every speed of presentation, as the case may be. The number of repetitions (r) or speeds of presentation (s) and the resulting percentages of right associates (p) are as follows:

For S:	$r=$	2	4	6	8	10	repetitions
	$p=$	.2425	.4375	.5400	.7850	.8125	
For M:	$r=$	2	4	6	8	10	repetitions
	$p=$	.290	.484	.618	.756	.804	
For E:	$s=$	.50	.75	1.00	1.25	1.50	sec./syl.
	$=$	120	80	60	48	40	syl./min.
	$p=$	.5300	.6950	.7775	.8050	.9100	
For R:	$s=$	.50	.65	.80	.95	1.10	sec./syl.
	$=$	1.20	93.1	74.9	63.1	54.5	syl./min.
	$p=$	.338	.486	.554	.616	.784	

Inspection of the four functions shows no inversions of the first order, a fact which indicates relative constancy of attitude on the part of the *O*s, and, therefore, relative homogeneity of the data.[8] But, inasmuch as the functions contain inversions of the second order, it is evident that absolute homogeneity was not attained.[9]

The four functions given above represent all the facts which may be *experimentally* determined as a result of our procedure. They show the dependence of correct responses upon either the number of repetitions or the speed of presentation, as the case may be. It is desirable, however,

[6] A. Aall, *Zeit. f. Psychol.*, 66, 1913, 1 ff.; *Ber. ü. d. V. Kong. f. exper. Psychol.*, 1912, 237 ff. G. J. Rich, *Jour. of Educ., Psychol.*, 8, 1917, 239 f.

[7] S. S. George has shown the effect of attitude on psychophysical functions, this JOURNAL, 28, 1917, 1 ff. It is to be expected that attitude will similarly influence memorial functions.

[8] E. B. Titchener, *Experimental Psychology*, 2, 1, 1905, 94f. W. Brown and G. H. Thomson, *Essentials of Mental Measurement*, 1921, 81. Williams' data contain inversions of the first order for both *O*s. One *O* is definitely reported to have been unable to maintain a constant attitude; *op. cit.*, 221, 222.

[9] Brown and Thomson point out that homogeneity is rare in psychophysical data; *op. cit.*, 94.

to summarize the data into four limens or thresholds of associative effectiveness, that is, to state for every *O* the number of repetitions or the speed at which he will in one-half of the cases give a correct response. The determination of such a limen necessitates the assumption of some hypothesis toward the functions experimentally obtained. The point at which a correct response is as likely to be given as not to be given does not occur in the experimentally determined results, and it is necessary to assume that the required point lies upon some curve drawn to fit two or more of the experimentally determined points. The choice of the curve that shall be used is an arbitrary matter, but may be determined in part by the experience of other investigators in dealing with psychometric and 'mnemometric' functions. We have taken for trial 6 hypotheses with respect to the functions for repetitions and 4 hypotheses with respect to the functions for speed, have computed limens according to every one of these hypotheses, and have determined by two criteria the adequacy of every hypothesis to the experimental data.

Because of its frequent (and almost universal) use in psychophysics, we first chose the *phi-gamma* hypothesis[10] as a means of calculating the limens from the functions in which the number of repetitions was the variable.. Next, we adopted the suggestion made by Williams that the frequency of correct response may be a compound function of the variable, such as the *phi-gamma* function of the logarithm of the repetitions or of the square root of the repetitions.[11] In the *phi-gamma* function as ordinarily used, and as used by us in the first computation of the limens, $\gamma = hr$. In our second and third computations of the thresholds, we have the additional assumption that $\gamma = h \log r$, and that $\gamma = h\sqrt{r}$. This meant that we used the logarithm or the square root of the number of repetitions in place of the number of repetitions itself, computed limens according to the standard procedure, and then found the antilogarithm or square of the resulting value, as the case might be.

Turning aside from the *phi-gamma* function, we assumed that the 'mnemometric' function was a portion of a straight line, determined by least squares (unweighted) the best fitting straight line (of the form $p = ar + b$) for the observed values, and found the value of r (repetitions) for which $p = 50\%$. This constituted our fourth calculation of the associative threshold. In making the fifth computation, we assumed that the function was of the logarithmic form ($p = a \log r + b$), and proceeded as in the preceding case. The sixth value of the limen for each *O* was similarly found by determining by the method of least squares (unweighted) the best fitting curve of the form $p = a\sqrt{r} + b$.[12]

Having determined the limens for the two *O*s, our next task was to test the adequacy of every hypothesis as a device for summarizing the experimental data. The adequacy is indicated in every case by the closeness of the curve to the observed values, that is, by the agreement of the values of p as calculated according to the given hypothesis with the observed values of p for the same number of repetitions. To obtain figures comparable with those of Williams, we have computed ϵ for every hypothesis ac-

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[10] E. B. Titchener, *Experimental Psychology*, 2, 1, 92 ff; E. G. Boring, this JOURNAL, 28, 1917, 280 ff.

[11] We made no assumption with respect to a mnemometric unit, as did Williams, nor did we attempt to determine one.

[12] A linear, logarithmic or square-root function cannot logically be the mathematical representation of mnemometric data since it exceeds, possibly within the range of stimulation, the limits set by the experimental procedure ($p = .00$ and $p = 1.00$). A portion of such a function might, however, have fitted the observed values and have been, therefore, useful as a means of computation of limens.

cording to the formula which he gives.[13] Thomson has shown that this criterion is not the most adequate measure of goodness of fit. We have, therefore, calculated for every hypothesis the value of P according to Thomson's adaptation of Pearson's method of testing for goodness of fit.[14] The values of the limens (L) according to every hypothesis together with the measures of goodness of fit are given to the accompanying table.[15]

Hypothesis	Observer S			Observer M		
	L	ϵ	P	L	ϵ	P
Phi-gamma						
$\gamma = hr$	5.09	.048	.2208	4.57	.034	.3586
$\gamma = h \log r$	4.39	.063	.0499	4.52	.091	.0000
$\gamma = h\sqrt{r}$	4.74	.048	.2717	4.27	.015	.9454
Least Squares						
$p = ar + b$	5.15	.056	.0000	4.61	.046	.0000
$p = a \log r + b$	4.38	.062	.0000	3.96	.023	.0985
$p = a\sqrt{r} + b$	4.78	.049	.0000	4.29	.023	.0787

We may now turn to a consideration of the values obtained according to the various hypotheses. It is evident that the limen is dependent in large measure upon the hypothesis used. We are principally concerned, however, with the value of P. P is in every case the probability of obtaining in random sampling a fit as bad as or worse than that observed, if the hypothesis is a true representation of the facts. Thus, a P of .01 means that, if the assumed function were correct, in only one case out of a hundred should we obtain so bad a fit by mere chance, and therefore raises the presumption that the assumed function is not a correct representation of the relationship measured in the experiment. Or a P of .90 means that, if the assumed function were correct, in nine cases out of ten the fit would be no better than that obtained, and raises the presumption of adequacy of the hypothesis to the observed data. A high P means a hypothesis adequate to the facts which are to be summarized; a low P means an inadequate hypothesis.

For both *Os*, the most adequate hypothesis is a combination of *phi-gamma* and the square-root hypothesis, giving a P of .94 for M and of .27 for S. The first of these is unusually high for psychometric data of any sort. The next best fitting curve is that of the *phi-gamma* hypothesis taken alone, while the combination of *phi-gamma* and logarithmic hypotheses gives a probability of less than one in twenty of obtaining the observed values by chance.

For S, no one of the three remaining hypotheses proves to be of value in summarizing the data, for according to none is there as much as one chance in ten thousand of obtaining the observed data in a random sampling. For M, values of P as high as .09 are found, but even this figure represents less than one chance in ten of obtaining the observed values. We find, then, that mathematical functions of the *phi-gamma* type, especially when combined with a square-root function, are most adequate for the purpose of summarizing data of this sort and, therefore, for the computation of limens which represent the observed values; and that portions of simple non-'ogival' functions, linear, logarithmic or square-root, are inadequate for these purposes.

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[13] *Op. cit.*, 222.

[14] K. Pearson, *Tables for Statisticians and Biometricians*, 1914, xxxi. ff. G. H. Thomson, *Biometrika*, 12, 1919, 216ff. W. Brown and G. H. Thomson, *op. cit.*, 77ff.

[15] Linear interpolation gives limens of 5.12 for S and 4.24 for M; logarithmic interpolation limens of 5.12 for S and 4.20 for M; and square-root interpolation limens of 5.17 for S and 4.22 for M.

We may now consider the data obtained from the other two *Os*, in which rate of presentation was the variable factor. Two measures of this variable are possible: the time per unit of material read (sec. per syllable); and the amount of material read per unit time (syllables per min.). It is especially important to note that these two measures of the variable condition of association are not linearly related but are harmonic functions of each other.[16] It is to be expected, therefore, that the limens obtained as well as the adequacy of the hypotheses used will depend upon the measure of the stimulating conditions which is used in the computations. For convenience in comparison, all limens are expressed in sec. per syllable, results obtained by using syllables per min. as the measure being changed back to these units after the computations were completed.

As in dealing with the preceding data, we commenced with the *phi-gamma* hypothesis, using both measures of speed. Our third and fourth computations were performed by finding the best fitting straight lines by least squares (unweighted), for correct responses and sec. per syllable, and for correct responses and syllables per min., and finding on this line the speed at which p is 50%. The same measures of adequacy as already used (ϵ and P) were applied here, and are shown in the accompanying table together with the liminal values (L).[17]

Hypothesis	*Observer E*			*Observer R*		
	L	ϵ	P	L	ϵ	P
Phi-gamma						
Sec./syl.	.370	.033	.5701	.698	.043	.2885
Syl./min.	.487	.138	.2794	.666	.056	.0548
Least Squares						
Sec./syl.	−.249	.070	.0000	.714	.041	.0000
Syl./min.	.458	.034	.0006	.650	.063	.0001

The limens here again show dependence upon the hypothesis used, a dependence which is more marked in the case of E than it is in the case of R. This difference is due, probably, to the more adequate determination of the function as a result of the experimental work on R, for whom the speeds used were on both sides of the limen; while the speeds used with E all gave supraliminal learning.

When the *phi-gamma* hypothesis is used, it is more adequate to the observed data if the conditions of association are measured in terms of the time per unit read than if they are stated in amount read per unit time. We may state the same conclusion by saying that if syllables per min. are used as the variable, the combination of the *phi-gamma* hypothesis and the harmonic hypothesis is more adequate to the observed data than is the *phi-gamma* hypothesis alone. In the case of E, the better hypothesis gives a more than even chance of the observed data occurring in random sampling, while for R the probability is greater than one in four. The linear hypothesis, applied to either measure of speed, is inadequate to the facts, the probabilities of the observed data occurring under it being less than one in one thousand. In one case, the linear hypothesis leads only to the trivial solution of a negative liminal speed.

To summarize: we have obtained data for 'mnemometric' functions from 200 or 250 observations (grouped in series of 5 pairs of nonsense

[16]For (syl./min.) = 60 ÷ (sec./syl.); and (sec./syl.) = 60 ÷ (syl./min.)

[17]For R, linear interpolation in terms of sec./syl. gives a limen of .681, and in terms of syl./min. a limen of .685. For M, linear extrapolation from the two values nearest 50% gives a limen of .454 in terms of sec./syl., and of .468 in terms of syl./min.

syllables), and for two variable conditions of association; we have calculated limens from these data according to a number of hypotheses; we have found that, under our conditions of experimentation, a combination of the *phi-gamma* and square-root hypotheses is the most adequate of the means we have used to summarize the observations and thereby to calculate a limen for associative strength as conditioned upon number of repetitions; and we have found that the *phi-gamma* hypothesis applied to time per unit read occupies a similar position with respect to associative strength as conditioned upon speed of presentation.

A great deal of further investigation along the lines of procedure used in this study remains to be done. Our trial of various mathematical hypotheses was necessarily limited in its scope by the time at our disposal. It is possible that there are other hypotheses (such as the arctan, Pearsonian skew curves, etc.) which are more adequate to the problem than any we have used. These should be tried on data obtained from as large a number of observations as possible, especially as *P* becomes more sensitive as a criterion of fit as the number of observations increases. We have obtained results for only two variable conditions of association. The adaptation of psychophysical procedure here used may be applied to the measurement of other factors determining associative strength. A still further extension of the method would consist of the determination, for the same *O*, of the associative strength as conditioned at the same time upon two independently variable factors, as, for instance, number of repetitions and speed of presentation.

II. Constant *vs.* Accelerated Speed in Learning

By Anne E. Fineman and Selina Ruderman

Meumann[1] reports an experiment in learning nonsense syllables in which the *O* was allowed to control the speed at which the syllables were presented and was instructed to regulate it in such manner that he might learn most conveniently and might feel that he was making the most satisfactory progress. The results showed that the drum was rotated slowly at first and then more rapidly as the learning proceeded. Such results show only that an accelerated speed of learning was most comfortable to the learner, not that it produced the most efficient learning. Proof is lacking that the speed of presentation chosen by the *O* was the one at which he learned most efficiently, that is, that the *O* could choose introspectively the best speed for learning. It still remains an open question whether or not an accelerated speed of presentation actually results in more efficient learning than does a constant speed. We have attempted in a preliminary way to answer this question for a particular set of conditions.

The material to be learned consisted of nonsense syllables constructed according to Gamble's modification of Müller and Schumann's rules.[2] They were made up into series of 10 syllables and presented stepwise on a Spindler and Hoyer exposure apparatus. The 14 syllable drum was used, so that the successive repetitions of a series were separated from one another by the time required to expose 4 syllables. The procedure was that of the *Treffermethode*. The *O*s learned every series for 4 repetitions. Immediately after the completion of the fourth repetition, 5 of the syllables just learned (the first, third, fifth, seventh and ninth, in haphazard order) were presented, and the *O* was instructed to respond to every one with the syllable

[1] E. Meumann, *The Psychology of Learning*, 1913, 261f.

[2] E. A. McC. Gamble, *Psychol. Rev. Monog.* 43, 1909, 18ff.

that followed it in the series. Every test-syllable followed the preceding one as soon as the O gave the associate or indicated that no associate was forthcoming. Responses were scored as right or wrong.

Four constant and one variable (constantly accelerated) speeds were used. The constant speeds were: .50, .60, .75, .80 and 1.00 sec. per syllable (120, 100, 80 and 60 syllables per min., respectively). In the variable speed, the first repetition was at the rate of 1.00 sec./syl., the second at .75 sec./syl., the third at .60 sec./syl., and the fourth and last repetition at a speed of .50 sec./syl.[3] Each O learned 50 series (250 pairs of nonsense syllables at every speed). The order in which the various speeds occurred was determined by chance, but every speed was included in every set of 5 series in order to equalize practice.

Both *Os* (who also acted as *Es* for each other) had had a year of laboratory training which had included a similar experiment by the method of right associates, with the same apparatus. As both knew something of the problem, a procedure with knowledge was followed on the ground that a full knowledge of the experiment and results was more likely to lead to a constant attitude than was partial knowledge *plus* guesses, surmises and attempts to give the result which the O thought was desired. The *Os*, therefore, knew everything about the experiment except the syllables they were to learn.

Speed (sec./syl.)	*p* F	*p* R	Time for Learning (sec.)	*p*/Time for Learning F	*p*/Time for Learning R
1.00	.800	.540	40	.0200	.0135
.75	.776	.440	30	.0259	.0147
.60	.692	.412	24	.0288	.0171
.50	.568	.348	20	.0284	.0174
Variable	.704	.444	28.5	.0247	.0156

The accompanying table shows, in the second and third columns, the percentages of correct responses made by each O at every speed. For the 4 constant speeds, the percentages of right associates decrease with increase of speed. The lack of inversions of the first order indicates that fairly constant attitudes were maintained by the *Os*. The number of correct responses at the variable tempo are, for both *Os*, intermediate between the numbers for the slowest and the fastest tempos.

Our problem, however, is the relative efficiency of learning under the various presentations, that is, the ratio of amount learned to time expended.[4] As a basis for determining this ratio, we first calculated for every speed the total time of exposure of the 10 syllables during the 4 repetitions (excluding the time between repetitions) and considered this the time utilized for learning at that speed (fourth column). To obtain the efficiency of learning, we divided every percentage of correct responses by the time for learning. The resulting measures of efficiency of learning are shown in the fifth and sixth columns of the table. The relative efficiency of the constant speeds is not the same for the two *Os*. The discrepancy is understandable, however, when we compare the percentages of correct response. F learned a greater proportion of the syllables than did R, indicating that a speed which was slow for one was fast for the other. F reported, introspectively, that the slowest speed was so slow as to be annoying to her, while R did

[3]The speed, in terms of amount read per unit time, was *constantly accelerated*, as the four repetitions were made at 60, 80, 100 and 120 syl./min., respectively.

[4]This is Meumann's criterion of advantageous learning; *op. cit.*, 260.

not find this to be the case. R's maximum efficiency was not passed at a speed of .50 sec./syl., while F appears to have reached a maximum of efficiency in learning at .60 sec./syl.[5]

For both *O*s, however, the efficiency of learning at the constantly accelerated speed is intermediate between the highest and lowest efficiencies found for the constant speeds. The variable speed of presentation is more efficient than the slowest speed, but less efficient than the rapid speeds. Although the *O*s had, in the variable presentation, better opportunity to become familiar with the syllables during the earlier repetitions than in the case of the rapid presentation, the increased strength of association resulting therefrom does not compensate for the extra time consumed, so far as the ratio of amount learned to time expended is concerned.

The results of this experiment must, of course, be interpreted in the light of the experimental procedure. We used the method of right associates, not the method of complete mastery, and Ephrussi has shown that the effect of tempo on efficiency depends upon the method by which the strength of association is tested.[6] The values of the ratio of correct responses to time used for learning may or may not bear the same relations to one another as do the times required for complete learning. Moreover, our speeds were chosen within limited range. The fastest speed, .50 sec. per syllable, was not beyond the speed for maximum efficiency for one, and only slightly beyond it for the other *O*. The effect of a constantly accelerated speed of repetition which reached in the later repetitions a considerably higher rate than any we used is a matter for further investigation. The increasing familiarity of the material might possibly admit of the economical use of higher speeds.

We may then conclude that, when working by the method of right associates and with speeds not exceeding .50 sec. per syllable, a constantly accelerated speed of presentation will not produce in 4 repetitions as great an amount of material learned in proportion to time expended as will a constant rapid rate of presentation within the same limit. An adequate answer to the general question stated in our first paragraph requires further investigation with more rapid tempos, as well as by more than one method of testing retention.

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[5]R. M. Ogden has shown that a middle speed is more efficient than very rapid or very slow presentation. *Arch. f. d. ges. Psychol.*, 2, 1903, 93 ff.

[6]P. Ephrussi, *Zeit. f. Psychol.*, 37, 1905, 192; E. Meumann, *op. cit.*, 262.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF YALE UNIVERSITY

III. A Quantitative Experiment on the Purkinje Phenomenon

By Llewellyn T. Spencer

In the existing textbooks or outlines for courses in experimental psychology available to the writer no thorough experiment is given on the subject of the Purkinje phenomenon. The phenomenon consists in a loss of saturation when colors are viewed under decreasing illumination and also a shift of the point of maximal brightness of the spectrum from the yellow toward the green. Judd,[1] Seashore,[2] Hollingworth,[3] Langfeld and Allport,[4] and Poffenberger[5] make no mention of the phenomenon. Myers[6] and Foster[7] call the student's attention to the fact that a color mixture which matches gray in daylight fails to do so if the illumination is reduced. Sanford[8] and Titchener[9] direct the student to compare the brightness of red and blue in daylight and then note the change in brightness-relation upon entering the dark. The writer has found the following experiment practicable for groups of 14 or 15 students and clear results have been obtained with satisfactory uniformity.

The materials required are a set of colored papers and a two-candle-power lamp in a dark room, in series with which are a number of resistances which may be thrown into the circuit at will. The resistances used at the Yale Laboratory are arranged on two resistance boards, consisting of 100, 200, 300, and 400 ohms and 1000, 2000, 3000, and 4000 ohms. This makes it possible to obtain the series of 200, 400, 600, 800, 1000, 1200, and 1400 ohms used in the experiment. The set of colored papers is composed of five slips each, 1 by 1½ in., of red, orange, yellow, green blue, and violet, and also five each of white, gray, and black. These colors are chosen so as to approximate as nearly as possible the relative brightnesses of spectral colors.

The students are first asked to arrange the six chromatic papers in the order of their brightness, under daylight conditions, ranking the brightest as 6, the next as 5, and so on down to 1. All the slips are then thoroughly shuffled and the class enters the dark room. The two-candlepower lamp is suspended at some distance above the center of a table at which the students sit. The instructor throws in the resistances, as required, from a nearby table. Each part of the experiment is preceded by a 5 min. period of dark adaptation under the illumination to be used in the particluar observation. During the first of these periods, with a 200 ohm resistance in the circuit, the students are given the instructions for the experiment. At the instructor's signal they are to sort the colors by *hue* into piles, arranging the order of piles in the order of the colors of the spectrum to insure identification when the full lights are turned on. They are cautioned against (*a*) changing a judgment once it is made, (*b*) comparing a slip being considered

[1]Laboratory Manual of Psychology, 1907.
[2]Elementary Experiments in Psychology, 1909.
[3]Outlines for Experimental Psychology, 1914.
[4]Elementary Laboratory Course in Psychology, 1916.
[5]Experimental Psychology, Loose Leaf Manual.
[6]Textbook of Experimental Psychology, 1911, Part II., 32-33.
[7]Experiments in Psychology, 1923, 191.
[8]Course in Experimental Psychology, 1898, 142.
[9]Experimental Psychology: Instructor's Manual, Qualitative, 1918, 7.

with colors already sorted, (*c*) sorting by any other characteristic than hue, for example, by brightness, thickness, or texture, and (*d*) inferring the hue from the fact that five slips have already been sorted as of the color in question. They are requested to sort a slip into the colorless piles if they are unable to distinguish the hue with certainity. Guessing is forbidden. It is explained that the only function of the white, gray, and black is to provide places where unrecognizable colors may be sorted. It is found advisable to provide pieces of white cardboard upon which the sorting is done.

After the first sorting is completed the full lights are turned on and the sorting is scored. Record blanks ruled in 9 columns and headed with the 9 colors are required. The student lists under each color the number of slips correctly sorted into that pile, then the number and name of each color incorrectly sorted into that pile. The columns are totaled and the totals added. This should give the number of slips, *viz.*, 45. Then for each column the number of slips *correctly* sorted is divided by the number of slips *incorrectly* sorted *plus* 5, the number which should be sorted there. This percentage is thus a weighted score under which the student is penalized for not sorting the slips of the given color into that pile (by reducing the numerator) and also for sorting other colors into that pile (by increasing the denominator). Four red, one violet, and one blue in the red pile would score $\frac{4}{2+5} = 57\%$, while four orange slips in the orange pile would score 80%, and five orange would score 100%. The slips are then reshuffled, 200 ohms are added to the resistance, and 5 min. are again allowed for dark adaptation. This is done for the resistances of 200, 400, 600, 800, 1000, 1200, and 1400 ohms.

In treating the results the colorless slips are disregarded. The percentages of success for the 6 colors for each resistance are averaged, and the curve of accuracy is plotted against the degree of illumination. The percentage of accuracy for each color at the 1400 ohm level is plotted to obtain the curve of brightness at that level. This curve is compared with a similar curve plotted from the rankings of brightness made in daylight at the beginning of the experiment. The ranking figues, 6,5,4, etc., may be divided by 6 to render them comparable to the percentages of success in darkness when plotted on the same graph.

The following results were obtained by the writer with 6 subjects.

TABLE I

Red	*Orange*	*Yellow*	*Green*	*Blue*	*Violet*
Average rank for brightness under daylight conditions					
1.16	4	5.83	4.83	3.33	1.83
Average rank under daylight ÷6, then minus 7%, in percentages					
12	60	90	73	48	23
Average percentage correct with resistance of 1400 ohms					
13	47	52	90	26	16

In order to plot the values for daylight brightness on the same curve as the values for weak illumination, the ranks for the 6 colors must be converted to percentages. This may be done by dividing by 6, the highest possible rank. Since the values thus obtained give a curve slightly higher than the curve for 1400 ohms, they are best compared with the latter curve if some constant such as 7% is subtracted from each, so that the modes of both curves are of equal height. These are the results which are given in the second line of the Table.

The average percentage correct for the various resistances was for the 6 subjects:

200	400	600	800	1000	1200	1400
91.7	92.1	83.3	75.4	72.6	54	41

Plate I gives the data of Table I. The continuous line represents the brightness of the colors under daylight, and the broken line represents the average accuracy of the 6 subjects for each color with the 1400 ohm resistance. Plate II represents the average percentage of accuracy for all colors as the resistance in the circuit is increased.

It is evident that the experiment demonstrates the two significant facts of the Purkinje phenomenon, *viz.*, that the point of maximal brightness shifts from the yellow under daylight conditions to the green under weak illumination (*cf.* Plate I), and that the ability to distinguish the various hues decreases with the decrease in illumination (*cf.* Plate II).

The experiment takes about 90 min. of actual running time, so that, with a 15 min. period for instruction, etc., it may easily be done in a single 2 hr. laboratory period. There is sufficient intrinsic interest in the experiment for the student, in the writer's experience, to make its incorporation into the laboratory course very satisfactory as a quantitative experiment on the Purkinje phenomenon.

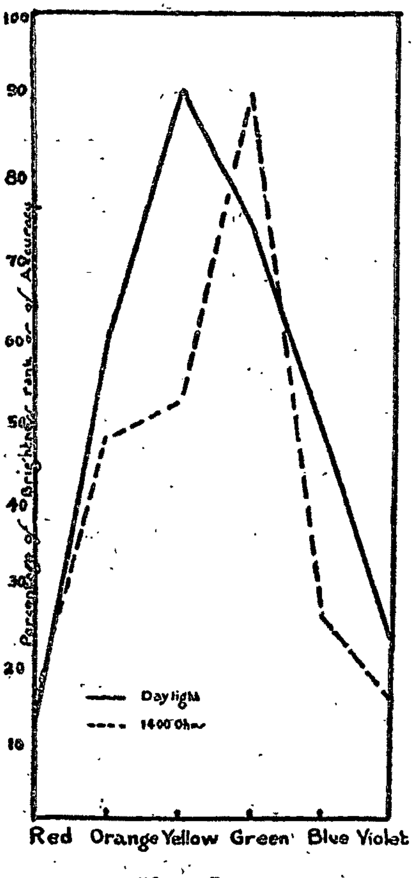

Plate I

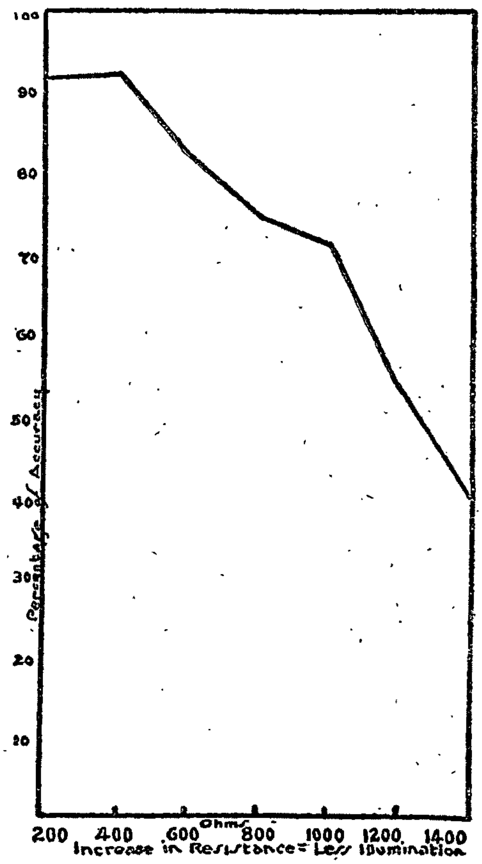

Plate II

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. Titchener

LXVII. Position *vs.* Intensity as a Determinant of Attention of Left-Handed Observers

By Ruth S. Burke and Karl M. Dallenbach

The study entitled "Position *vs.* Intensity as a Determinant of Clearness,"[1] recently published from this laboratory, showed conclusively that position was, within the limits of our experiment, a condition of attributive clearness and a determinant of attention; and that the left-hand position and the position above had an attentional advantage. As the *O*s who served in this study were right-handed, these results suggested a neurological explanation[2]; and is seemed worth while to ascertain the positional preference of left-handed *O*s.

Observers. The *O*s were Miss Clara C. Medlin (*M*) and Mr. Ernest R. Holmes (*H*), graduate students in the summer session of 1923.[3] H wrote with his left hand and was, on that basis alone, accepted as left-handed; but, as will later appear, he is not of the pure type. M is unquestionably of the pure type. She does everything but write with her left hand; sewing, buttoning, knitting, using knife and fork, wiping dishes, dealing cards, throwing, playing tennis, hammering, shoveling, etc., are all done with the left hand. Writing with her is a case of nurture overcoming nature; for until she was nine years old she also wrote with her left hand, but at that age she came under the instruction of a teacher who in accordance with the pedagogy of the time forced her to change.

We should have been glad to extend the experiment more widely; but, unfortunately for our purpose, these two *O*s were the only members of the summer classes in psychology, which numbered close to three hundred, who could qualify as left-handed.

Apparatus and Procedure. The same apparatus was used as in the earlier work, and the same procedure was followed: two circular areas, which varied only in position and in intensity of illumination, were compared; and the objective intensity at which the two were reported equally clear was determined for each *O* for every one of the four cardinal and four intermediate positions. The positions, as before, were: 1, horizontal, variable left, standard right of fixation; 2, sinistral oblique at 45°, variable left and below, standard right and above fixation; 3, vertical, variable below, standard above fixation; 4, dextral oblique at 45°, variable right and below, standard left and above fixation; 5, horizontal, variable right, standard left of fixation; 6, sinistral oblique at 45°, variable right and above, standard left and below fixation; 7, vertical, variable above, standard below fixation; 8, dextral oblique at 45°, variable left and above, standard right and below fixation. The length of exposure was, also as in the earlier study, approximately 0.1 sec.

[1]K. M. Dallenbach, this Journal, 34, 1923, 282ff.

[2]*Ibid.*, 286.

[3]Our thanks are due to Miss Medlin and Mr. Holmes for their co-operation and for the many hours that they spent in observation.

The following instructions, the same as those used in the previous experiment, were read to the *O*s at the beginning of every experimental hour "At the signal 'Ready' fixate the spot of light before you; at 'Now' two areas of light will be shown; report which, if either, is the more clear. Give the position of the clearer as 'right', 'left', or 'above,' 'below', whichever the case may be; if they are equally clear, say 'same'; if you are uncertain report 'doubtful.' Experiments reported 'doubtful' were, without *O*'s knowledge, immediately repeated; there were but few of these 'doubtful' reports.

After determining, in a series of preliminary experiments by the methods of limits, what the approximate intensive values of the variable necessary for equality of clearness would be for every one of the 8 positions, 5 stimulus-values were selected for each *O* for every position, and the main experiments were conducted, as in the earlier work, by the method of constant stimulus-differences. 40 observations were taken for *H* and 25 for *M* at every position for every value of stimulus. In accordance with the findings of the earlier experiment the positions as well as the stimuli were varied in haphazard order.[4] The *O*s never knew before the exposure what position the comparison areas would occupy.

Results. The results of this Study are shown in Table I, which gives for each *O* and for every position of the comparison areas the percent. of illumination necessary for the variable to be reported equally clear with the standard, the intensity of which was, as in the earlier study, 16%.

TABLE I

Showing for Each *O* and for Every Position of the Stimuli the Percent of Illumination of the Variable Necessary for the Report of Equality in Clearness

OBSERVER

POSITION	*H*		*M*	
	% ILLUMINATION (L)	PRECISION (h)	% ILLUMINATION (L)	PRECISION (h)
1	12.5012	.1071	63.8136	.0396
2	9.5924	.1157	18.7536	.0379
3	18.7184	.0559	9.2192	.0740
4	40.2896	.0497	9.5236	.1154
5	24.8632	.0496	7.5964	.0900
6	17.5368	.0538	15.7928	.0602
7	17.0656	.0606	45.2392	.0529
8	9.9392	.1324	55.4568	.0482

These data show that position is effective for both *O*s; but also that it is effective in opposite directions. *M*, the undoubtedly left-handed *O*, is disposed toward the right. The areas to the right and below the point of fixation (positions 3, 4, 5 and 6) require an illumination of less intensity to equal the standard in clearness than do the areas above and to the left (positions 1, 2, 7 and 8). The most favorable position for *M* is 5, the position in the horizontal in which the variable is at the right of fixation; the most unfavorable is 1, the position in the horizontal in which the variable is at the left of fixation; the percent. of illumination required to equal the standard of 16% being for these positions 7.6% and 63.8% respectively. *H*, on the contrary, is disposed, as were the right-handed *O*'s, toward the left. The areas to the left and above fixation (positions 1, 2, 7 and 8) require less illumination to equal the standard in clearness than do those

[4] *Op. cit.*, 285.

below and to the right (positions 3, 4, 5 and 6). The most favorable position for *H* is 2, the position to the left and 45° below fixation; this result is in agreement with the results of the right-handed *O*s.

Time did not permit of an extended investigation of *H*'s type. We found, however, that he, unlike *M*, did many things right-handedly. He was for example, right-handed in batting, in swinging an ax or club, and in shoveling, and right-footed in kicking a football, and in jumping; he was also, as the results of this experiment show, right-handed in 'seeing.' He cannot, therefore, be regarded as a representative of the pure left-handed type, but belongs to some form (unfortunately, not further determined) of the intermediate type.[5]

Conclusions. We feel justified, on the basis of our experimental results with these two *O*s, in reaffirming the conclusion already drawn: that position is a condition of clearness and a determinant of attention; and in adding that the specific preferential position seems to be a matter of right- or left-handedness; the positions to the left and above have a distinct attentional advantage for right-handed *O*s, and contrariwise the positions to the right and below have an attentional advantage for left-handed *O*s.

LXVIII. The RL of Increased Chroma with Film Colors

By Diana Ginsberg and L. B. Hoisington

The problem of this Study grew out of the work of Elliott, West and Hoisington on the spatial limen for the four principal film colors.[1] With colored discs of equal tint and chroma their *O*s reported a pronounced increase in chroma as well as a luminous character when the stimulus color showed through a small opening in a screen of the same subjective brightness.[2] Our problem was to find, in terms of visual angle, the RL for this increase in chroma. In other words we proposed to increase the visual angle till the stimulus color shown through an opening should have the same degree of chroma as an identical stimulus standing free before the *O*. We hoped also to determine the nature of the psychometric function, if such a function should reveal itself, representing the changes in chroma when the stimuli are two film colors. Owing to unexpected complications, this part of the problem remains untouched.

The conditions were the same as, and the method similar to, those of the previous Study. We used the same apparatus with slight modifications, colored discs of the same composition, and the same sources of illumination with the fixed light in the same position as for the latter part of the previous work, except that we used 200 watt bulbs, and that the ends of the cones which confined the light had to be cut off to allow the illumination of a somewhat larger area on the screen.

Our problem demanded the use of two identical discs on separate motors; *O* was to judge the chroma of a film color in comparison with that of a color which entered into a total perception of surface. One disc stood just in front, and to the left of center, of the back member of the double grey screen, which was now covered over its entire surface with the no.6 grey. We shall refer to this as the perceptive stimulus: it presented to *O*, when the front screen was up, a colored surface like that of any ordinary rotating

[5]For types of dextrality see J. M. Rife, *Psych. Review,* 29, 1922, 474–480.

[1]This Journal 35, 1924, 125 ff.

[2]*Ibid.*, 131.

disc. The chroma of the color experience correlated with this stimulus served as standard of reference for the judgments. Just to the right of center in the back member of the double screen was a circular opening, 18 cm. in diam., which received cards, covered with the same grey, with circular openings at their centers of 31, 33, 35, 37 and 39 mm. diam. These dimensions resulted from a long series of preliminary, experiments; they gave the variable visual angles with the G stimulus. The second disc, which was 60 cm. behind the screen and which was the stimulus for the film color,[3] showed through the holes above mentioned. It was essential that these openings should be sharp cut and clean on the edges, so as not to offer contours or differences in tint. The front part of the double screen, which was covered over its entire surface with the grey paper, and which *E* could raise or lower (just in front of the front disc) by means of a lever and an attached cord that ran over a pulley screwed to the ceiling, exposed the two stimuli simultaneously, and served to control the time of exposure and to maintain a uniform state of adaptation.

To secure equality of tint, we adjusted the fixed light in the lateral direction till a grey disc on the front motor disappeared against its background. We then placed a disc of the same grey on the back motor, and adjusted the variable light in the near-far direction till the hole in the screen went out. Now the grey discs on the two motors received such intensity of illumination that each was the same in tint as the screen. The adjustment of the variable light had to be made at the start of every hour of observation, and sometimes also within the hour; for, although the two lights were on the same circuit, the same settings did not always result in equality of tint. When the colored discs displaced the grey, it happened very rarely that one or other or both of the colors had not the same tint as the screen. Sometimes this difference in tint between color and grey quickly disappeared; sometimes it persisted, and a new adjustment had to be made.

It was soon found that the same position of the variable light would not give equality of tint with every one of the different-sized holes that served as the variable factor. In general, the smaller the hole, the darker in tint did the grey appear through it. This meant that with the smaller holes the light came forward and with the larger it went back; it meant also that the light had its particular position for every one of the variable stimuli. That position had also to be determined at the start of every hour of observation. Matters were, indeed, even more complicated: variable no.4 (the next to the largest, 37 mm. in diam.), which should have given the next to the lightest grey, gave a grey that was almost as dark as that of variable no. 1, the smallest. In the hope that this result might be a function of the particular card used, and not of the size of the opening, we made two other cards with openings of the same size; but all gave the same reversal. We then made two other cards, with openings 2 and 4 mm. greater in diam. than our largest variable. These cards gave a grey lighter in tint than that of card no. 5. We have no explanation of the anomaly to offer.

Since the experiences of R and G had shown the richest chroma in the previous work, we decided to work with these stimuli. We were able to complete the observations on G; these on R suffered for lack of time.

O sat 3.5 m. in front of the screen, with his head held in a head rest. The variable stimuli came in haphazard order with the proper adjustment of the variable light for the particular stimulus. The variables with the R stimulus were 20, 21.5, 23, 24.5, and 26 mm.

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[3]For a general discussion of the film color see M. F. Martin, Film, Surface and Bulky Colors and Their Intermediates, this JOURNAL, 33, 1922, 451-480.

The *O*s were Dr. H. G. Bishop (B), Diana Ginsberg (Gi), and Dr. L. B. Hoisington (H). B and H, members of the department of psychology, were trained *O*s; and Gi, a senior with her major work in psychology, had had a good deal of experience as *O*.[4] The instructions were: "You will be shown, simultaneously, two colors of the same hue. You will judge the relative chroma of the right hand color in terms of the left. If the color experiences show other differences, you may note them; but make your judgment in terms of chroma alone."

Table I gives the limens for the increase of chroma in terms of visual angle. In the case of R, B reported that the experience correlated with the back disc had better chroma than the other with the largest variable, and even with the 18 cm. opening, despite the greater distance of the stimulus. For this reason we were unable to compute a limen for him.

TABLE I

O	Color			
	G		R	
B	40′	5.7″	—	—
Gi	41′	41.9″	26′	36.0″
H	35′	24.5″	23′	4.2″

It should be noted that the judgments were comparative and not absolute as is usual in the determination of an RL. The point of subjective equality could not be determined, as the judgment 'less' was never given. We chose to compute the limen, using Urban's formulae for the method of constant stimulus differences, for the judgment 'greater.' The results would be the same if we had used the distribution of the 'equal' judgments, since the *O*s employed but the two categories of judgment.

The two simultaneous experiences gave rise to two distinct modes of experience, demanding different attitudes. It yet remained to determine whether attitude affects the judgment of relative chroma, or whether the chroma of a color is independent of its mode of experience. As appears later, we must conclude that it is not the same thing to judge the chroma of a perceived color and that of a color that is non-perceptive, as object. The film color differs from the surface color in other characteristics as well, so that it is difficult to say whether the characteristics of an experience are transformed in the perceptive integration, or whether some other of the characteristics of the film color cut across the judgment of chroma. We are, however, certain that in the perceptive experience the attributive aspects themselves get 'perceptualised' separately, whereas in the non-perceptive experience all the attributive aspects come beaten up into an undifferentiated totality. It may be, therefore, that our two alternatives amount, in the end, to one and the same thing: the perception of the color transforms the total non-perceptive experience, and in so doing it changes the chroma along with the other aspects of experience.

That the judgments were not wholly unequivocal is borne out by the fact that the results of Gi show inversions of the first order with both G and R; that those of B contain inversions of the first order with G, while he was unable to make the judgment with R (although the limens of both Gi and H were less for R than for G); and that the results of H contain inversions of the second order. Statements by B and H tend to prove the equivocal nature of the judgment. B reported: "There is an increase in filminess, but I am not sure whether it is chroma." "Even if the chromas are equal, the experiences are not." H on one occasion remarked: "There is something else there than chroma. What this something else is I can not say."

[4]A. J. Rubin acted as *E* when Gi was *O*.

The intruding factor or factors may be due to fixation or to attitude or to a combination of the two. H reported: "If I fixate the screen just at the side of the hole, there is an added perception of transparent bulkiness in the color that I see through the hole. This does not mean that I see the hole and the color separately. It comes as a greater amount of color, but not as a difference in degree of chroma. If I fixate half-way between the two colors, there comes a soft rosy glow. This glow looks very much like greater chroma. I get this in a more pronounced degree with some of the smaller holes. If I look at one color and then at the other, with this large hole (41 mm.), I see no difference in chroma. There is a difference in the apparent depth of the two experiences." And again: "If I fixate the edge of the screen in front of the front disc, the standard, I get a white-veil effect. The color seems to lie behind the white veil, and to be rather decreased in chroma on account of it. If I fixate the disc itself I get none of this. The color as seen through the hole (37 mm.) does change from practically true film to a little way towards bulkiness with a shift of fixation from within the color to any point on the screen. This carries a meaning of more color, and often increases the glowiness which at times comes as greater chroma."

Under certain not well-defined conditions the standard or perceptive color split up, as in the above report from H, into a grey and a red component. B made a like observation. "I took the front disc, my standard, as red as I could see it, and compared it with the hole which I tried to take as red without regard to its filminess. The glowy red of the hole is a redder experience. The other, the standard, is red and greyish. This time I took the red apart from the grey. The glowing character of the color through the hole remains fairly constant."

The film color, especially at the moment of exposure, also presented changes. B further reported that the variable was a washed out red, till filminess came in; and for H the luminous character came with or before the increase in chroma.

In the light of the above discussion, we offer the limens for whatever they may be worth. Whether they prove upon repetition to be approximately correct (as we are disposed ourselves to believe, since our *O*s were trained) is not very important. The main aspect of our work, we believe, is the attempt to get at the problem of the changes in the attributive aspects of experience under various perceptive attitudes. Our results show that this problem is at least not impossible of attack.

In summary, our *O*s were able to judge the relative chroma of a film color when compared with a surface color. In five out of six cases we could compute the limen for the increased chroma of the film color. Under our conditions and with our technique, the factors of chroma, volume and luminosity were bound together in the film color; we could not eliminate one without eliminating the others.

LXIX. A Preliminary Study of the Bourdon Illusion

By B. R. Rubin and H. P. Weld

In a discussion of the "tendency to imagine movement when similar objects appear successively in different positions" Bourdon describes an apparatus for its demonstration. The apparatus consists of a rotating white disk, upon the periphery of which are drawn a number of black figures. These figures are identical in size and shape and are equally distant from one another and from the center of the disk. In front of the rotating disk is placed a screen, in which a slit or window is so cut that the figures,

according to the direction of rotation, may be seen as successively rising or falling. "If", Bourdon says, "the interval between the disappearance of one of the figures and the appearance of the next following is sufficiently short, one notices, in addition to the real movement of the figures, a movement in the opposite direction which is produced every time that a new figure appears in the slit. And, in fact, this second figure succeeds an identical figure which has just disappeared; consequently, one thinks one sees the figure which has just disappeared return to the position occupied by the figure which is now visible."[1] Bourdon does not attempt an explanation of the illusion; and although its conditions are in some respects similar to those of the stroboscopic and tachistoscopic illusions of movement, they are still so different that this illusion cannot be brought offhand into relation with the others. We have thought it advisable, therefore, to undertake its study. Our immediate problem was to determine, first, the optimal and limiting rates of objective movement, for both directions of rotation; to obtain an accurate account of the appearance and behavior of the moving object; and finally, to get a psychological description of the processes in the perception.

Apparatus. Our apparatus consisted of a large disk, 31½ in. in diam., made of heavy white compo-board and mounted on a ball-bearing axis. On this disk were painted 10 black figures, trapezoidal in shape, and measuring 4¼ in. on the top and bottom sides, 3⅛ in. on the side facing the periphery of the disk, and 2¼ in. on the side toward the center. They were placed 2⅛ in. from the periphery and 9⅛ in. from the center of the disk; the distance between them was 5½ in. on the outer side, and 3⅞ in. on the inner side. The disk was rotated behind a screen, with a window, of the same white as the disk. The window was also a trapezoid, the upper and lower sides being 6¼ in., the side toward the periphery 4¾ in., and the side toward the center 3½ in.; the distance between screen and disk was 5 in. When the disk is rotated slowly, *O* sees the black figures rising, if the direction of rotation is clock-wise, and falling, if the direction of rotation is counter clock-wise; when, however, the disk is rotated at certain intermediate speeds, the black figures are frequently seen first to rise and then to fall, when the direction of their objective movement is upward; and by some *O*s, first to fall and then to rise, when the objective movement is downward.[2]

Attached to the back of the screen was a white shutter, covering the entire window, which could be lowered and raised from the side by means of a wooden handle. In the center of the windows was placed a fixation point, which consisted of a knot made in the middle of a length of hair and painted with china white; the hair was stretched vertically across the center of the opening. Although the experiments were performed in daylight, the illumination of screen and disk was increased and kept more nearly constant by a 200-watt nitrogen lamp with a good reflector placed at the side of *O*.

To control the rate of rotation of the disk we used an Edison phonograph motor which, under a pressure of 8 volts, has an approximately constant rate of speed. Between the motor and the disk was a reducing cone, so that by various adjustments of the belts a number of different rates of rotation could be obtained. These rates were calculated from the circumferences of the motor wheel, the disk wheel, and the grooves on the cone, and were checked by a stop-watch. The following rates (times of a single rotation of the disk) were employed: 1, 1.5, 2, 4, 6, 8, 10, 12, 14, 16, and

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[1]B. Bourdon, *La perception visuelle de l'espace*, 1902, 193f.

[2]The disk and screen here described were made to Professor Titchener's specifications, for purposes of lecture demonstration, to replace a less accurate model in use since 1905. Cf. E. B. Titchener, *Textbook of Psychology*, 1910, 359.

18 sec.; since there were 10 figures on the disk, a new figure appeared every .1 sec. for the fastest, and every 1.8 sec. for the slowest rate of rotation.

The *Os* were Miss G. K. Adams (A), a graduate student in psychology; Dr. H. G. Bishop (B), Mr. S. Feldman (F), and Professor L. B. Hoisington (H), members of the staff; and Miss Schuster (S), an undergraduate student majoring in psychology.

In determining the optimal rates we gave the following 'meaning' instruction: "A stimulus will be shown you, which will arouse a visual perception of movement. Report the direction of the movement and the nature of the moving object."

The procedure was as follows. *E* first set the apparatus in motion at the desired rate of rotation; then, when the rate was well established, gave the "ready, now" signal; raised the shutter; and exposed the disk for the period of one complete revolution. *O* then gave his report. After *E* had given the 'meaning' instruction for two series,—the first starting from the slowest and ascending to the fastest rate, and the second starting from the fastest and descending to the slowest rate, or conversely,—a 'process' instruction was substituted for it. This read: "You will be shown a stimulus which in previous experiments you have characterized as a downward (an upward) moving black object. Now describe the processes in strictly psychological terms."[3] Series with both modes of instruction were repeated until the reports became consistent for every *O*.

Results. Under the 'meaning' instruction four types of judgment concerning the direction of the perceived movement were given. These were 'up', 'down', 'up and down' and 'no direction.' The perceptions under these categories, when the disk was moving in the clock-wise direction, were in general as follows.

The 'Up' Movement. A black figure, called at various times a 'rectangle,' a 'sector,' a 'wedge,' appeared with a 'jerk' or 'jump' at the bottom, then rose, and finally disappeared at the top of the field. There followed an interval when only the white background was seen. Then another black figure like the first came into view, and crossed the field as before. With the slowest rates all four borders of the figure were sharp and distinct; with faster rates the upper and lower edges became jagged and blurred; with still faster rates the figure became narrower in the vertical dimension, and the upper edge became slightly concave, the lower convex. Ultimately, with the fastest rates of all (1, 1.5 sec.), a flickering gray band crossed the field in the vertical direction; with the slower of these rates transverse bands of gray (two or three of which were always present in the field) were occasionally reported as having an upward motion, but they never entered and left the field.

The 'Down' Movement. With the faster rates at which discrete objects were seen (2, 4 and 6 sec.), something (said at times to be like the 'rectangular' figure and at other times to be identical with it) appeared suddenly at the upper and then quickly dropped to the lower edge of the field, where it as suddenly disappeared. It was never reported as seen in the act of entering and leaving the field. Although, therefore, the trapezoidal figures were objectively rising, they were not in these cases perceived as rising. This is an illusion which is not reported by Bourdon.

The 'Up and Down' Movement. The black figure first appeared at the bottom, rose, and then, with the slower rates, disappeared at the top. But just at the instant of disappearance it, or something similar to it, suddenly

[3] *O* was also asked casually to report direction of movement if it was noted. This request was not incorporated in the typewritten instruction given to the *O* for fear that it might predispose him to an objective attitude.

TABLE I

Showing the Relative Frequencies with which Every Direction of Movemen was Perceived by Every *O* at Every Rate: Clock-wise Direction

Direction	Obs.	1	1.5	2	4	6	8	10		14	16	18
Up	A			17	14			17		33	14	67
	B		17							17	17	17
	F	20	20		20	20	40	60		100	100	100
	H	14					12.5	37.5	.5	87.5	100	100
	S		28.5		14	33	71	71		86	86	87.5
	Total	6	14	3	10	9	24	37		66	64	75
Down	A	17	33	33	71	62.5	57	50	[illegible]	17		
	B		17					17				
	F		20	20	20							
	H			33	17							
	S		43	100	28.5	14						
	Total	3	24	37	30	18	12	13	3	3		
Up and Down	A	83	66	50	14	37.5	43	33	[illegible]	50	86	33
	B		17	100	100	100	100	83	3	83	83	83
	F	60	60	80	60	80	60	40	[illegible]			
	H		40	50	83	100	87.5	62.5	7.5	12.5		
	S	14	14	14	57	57	28	28	4	14	14	12.5
	Total	30	38	57	59	74	64	50	2	31	36	25
No direction	A											
	B	100	50									
	F	20										
	H	86	60	17								
	S	86	14									
	Total	61	24	3								

reappeared at the top and quickly dropped to the bottom, where it was met by the next rising figure. The downward was more rapid than the upward movement: the figure was said to 'stamp down by itself', 'to beat down' (A), to 'flash down' (B), to 'kick down' (F), to 'flit down' (H), to 'pop down' (S).[4] The downward moving body was not well defined throughout its course. H says (rate 8 sec.): "It was a rectangle in the first part of the downward movement, but it faded out and became just something moving;" and again (rate 10) "I do not see any definite object move down: something moves down, and it is taken as the same rectangle." B reports (rates 18 and 16): "It was a pretty poorly marked flash down;" "It was a flash down all the way across the field;" (rate 10) "It is a flash part of the way, and a full sector the rest;" (rate 6) "For very little distance at the top it was a flash, for the rest of the way it was a sector;" and finally, (rate 4) "It was a sector both ways." This, then, is the illusion described by Bourdon.

The 'No Direction' Movement. This type of movement occurred almost exclusively with the two fastest rates 1 and 1.5 sec., and more frequently with the former than with the latter. With the 1 sec. rate the object was a "dark gray circular strip that extended in a vertical direction across the field;" the movement was a 'flicker;' and 3 of the 5 *O*s reported it generally as having no direction. With the 1.5 sec. rate the field was a flicker of narrow black and white bands. H says: "It is hard to say they are objects; they are just streaks of light and dark." The other *O*s, however, refer to them as 'sectors.' B describes the movement as an appearance and disappearance without (spatial) movement; and H also characterizes it as "a disappearance and reappearance." The reappearing streak, however, "may appear further up the field when it has the meaning of moving up." Similar reports were occasionally made also with the 2 sec rate.

In Table I we give the relative frequencies with which these types of movement were perceived at all the different rates of rotation. The horizontal line marked 'Total' gives the relative frequencies with which all the judgments, at the rate marked at the top of the column, fell into the particular category. For example, of all the reports given when the rate was 1 sec. per revolution of the disk, 6% were 'up', 3% were 'down,' 30% were 'up and down,' and 61% were 'no direction.'

The table shows that, in general, the relative frequencies of the 'up' judgments increase as the rate becomes slower. Although rate 18 was not slow enough always to inhibit the illusion for more than two *O*s, 75% of all reports at this rate were 'up;' *i.e.*, accorded with the direction of objective movement. Despite the fact that all *O*s occasionally gave 'down' judgments, only two (A and S) gave them with any degree of consistency, and the total number is smaller than for any other category. These judgments are most frequent with the faster rates at which discrete objects were seen (2 and 4 sec.), and decrease as the rates become slower. The 'up and down' reports are more frequent than any other, and for 3 *O*s occur even at the slowest rate. Their frequency has its maximum at 6 sec. (74%), and decreases on either side of that rate. The judgments of 'no direction' are most frequent with the most rapid rate; they decrease rapidly as the rate becomes slower; and they do not appear beyond 2 sec.

Finally, the table shows marked individual differences. Except at the extreme rates A reports the 'down' more often than the 'up and down' movement. B saw the Bourdon illusion almost invariably, while S gives a figure of only 57% at the optimal rate.

Before considering the content-processes involved in the perception, we shall report briefly the quantitative results of the experiments in which the disk was rotated in the counter clock-wise direction. All other condi-

[4]Bourdon uses the phrase "brusquement redescendre" (*op. cit.*, 193); and, in describing another and similar illusion, speaks of 'saccades' (194).

tions, of course, remained the same as before. Under these circumstances the stimulus object moves downward, and the illusion in Bourdon's sense should first be a downward and then an upward movement. We performed in all 814 experiments: the perception was 'down' in 81%, 'up' in 5.5%, 'down and up' in 10%, and 'no direction' in 3.5%. Two *Os*, F and S, never at any time saw the illusion; of the other 3 *Os* none saw it at the 1 sec. or beyond the 10 sec. rate. A reported it in only 3.6%, and B and H in only 25% of all trials. B, however, observed the illusion in 80% and 94% of the experiments at the rates 2 and 4 sec. respectively. A saw the 'up' movement in 16%, and H in 25% of all experiments. The net result of these experiments with the disk rotated in the counter clock-wise direction is that the Bourdon illusion occurs with a frequency much less than when the disk is rotated in the opposite direction. Some *Os* do not see it at all; and for those who do the range of rates at which it is seen is much narrower.

The Introspective Results. Except for a difference in the spatial aspect of the perceptions, the reports obtained with the disk moving in the two directions were substantially the same. We shall therefore describe the perceptions in the sense of the clock-wise direction. Furthermore, since the 'up and down' movements seen with the most rapid rates (1 and 1.5 sec.) were only vibratory or flicker movements of small extent, and not therefore of the kind mentioned by Bourdon, we shall disregard the reports of them. Finally, since the reports will be more easily understood if we give general accounts of the processes involved in the perceptions of the 'up' and of the 'down' movements before we describe the Bourdon illusion, we shall take them in this order.

The 'Up' Movement. H "A rectangular-shaped extent of black quality and definite contours came into the field below and spread upward across the field and disappeared. As it left the field its lower edge was a bit fuzzy, shading off through gray to white. The rate of progression was not quite uniform; it spread rather quickly at first, then rose at a uniform rate across the field to the top, and spread a little quicker. The black was followed by a white quality, and then the black appeared again as in the beginning." A and B further describe the black as a surface, and A says that the white between the successive blacks is "more silvery and misty" than the white of the background.

The 'Down' Movement. A "Black on white above the center of the field; then something flashy, I cannot report its extent or tint for it was so quick, but it does not belong to the black or the white, it rather extends over both at the same time. I changed to the meaning attitude and the 'down' was very strong: it seemed as if the whole black were moving. I

was black." H "A black quality sharply defined on the right and left sides appeared near the top of the field. The black quality then progressed downwards. As it approached the lower part of the field it faded into a lighter gray; then it darkened a little more." "A stretch of black at the top of the field. The lower edge was very fuzzy, practically white at the bottom; the denser black part was a little more extended in the vertical than in the horizontal dimension. Then extension downward, a spread of the black or, better, dark gray quality down pretty well across the field. This gets lighter as it approaches the bottom, where it is a fairly light gray." None of the other *Os* reported this perception in the 'process' series.

The 'Up and Down' Movement. A "Black first at the bottom of the field; moved up to the top. Then a gray flash which, in a way, went from top to bottom; this was of short duration. Black then appeared at the bottom; it expanded and expanded until it reached the top; then gray flash again from top to bottom." B "Lots of white between black sectors. The

lower edge of the sector at the top is going out as the upper edge of the sector at the bottom is coming into the field. Just as the top sector goes out there is a flash of something which starts at the bottom of the top sector. It is a very elusive experience. The flash is slightly grayish, practically like a curtain of gray, a diaphanous gray; it lies before the white, comes in with a snap, and ends pretty suddenly." H "A broad spread of black quality quite sharply outlined on the right and left sides and a little fuzzy on the lower edge spread up from the lower part of the field to the top; then an extension of gray downward. In quality it was a gray black, and it faded out to light gray at the bottom. It spread pretty well over the entire field; the black itself spread down a little." These reports, selected at random from a large number, are perhaps sufficient to show that the processes involved in the Bourdon illusion are a combination of those in our 'up' and 'down' perceptions. The characteristic process in the downward movement is a gray which seems to grow out of the lower sector of the disappearing upward moving black and to spread downward across the field. In quality it is generally described as a dark gray, lighter than the black; H says that its quality is not uniform, and that it shades off into a lighter gray at the bottom of the field. It is also said to be 'transparent', 'vaporous', 'misty', 'foggy', and the like. All three of the *O*s who describe it agree that it has a short duration. A and B habitually refer to it as a 'flash of gray', and occasionally both characterized it further as 'lively'. H, however, purposely avoided the word 'flash' because that meant for him an extension solely in time; he employed instead the term 'spread' or 'extension' of gray. Frequently he reported difference in amount of 'spread', as, for example: 'it was not uniform in extent throughout the exposure period; sometimes it was clear across the field, at others not half the way." He used these terms 'spread' and 'extension' without prejudice to the question of spatial movement of the gray. He once reported: "a total spread over almost all the field; the spreading seems to be all the movement there is, but I do not know whether it is time or space." To this question B gives no answer, and A gives two,—one of which is probably, and the other flatly negative: "There was not much change in its position or amount; I see it, and then I don't;" "It did not really move in any direction nor was it still."

Discussion of Results. There would seem to be no doubt that the downward phase with clock-wise movement of the disk is, on the side of content-process, a gray quality of short duration which differs phenomenologically from both the black of the figures and the white of the background; it is a 'lively', 'flashy' gray. In these respects it is apparently the same as the gray reported by Dimmick; and the Bourdon illusion seems therefore psychologically to be the same as the tachistoscopic illusion.[5] There is, however, a possible difference. Dimmick's gray did not change its position spatially, whereas our gray may have moved; our results, despite A's negative report, are inconclusive on this point. It would not be surprising, however, to find a difference of this sort where the conditions of illusion are different: in Dimmick's experiment there is no objective movement, while in ours the stimuli are moving across the visual field.

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[5]F. L. Dimmick, this JOURNAL, 31, 1920, 317. In his lectures of the last three years Professor Titchener has employed the Bourdon illusion to illustrate Dimmick's result. The demonstrational disk is given a sharp swing (in the clock-wise direction) and is allowed to die down. As the movement slows, the illusion of down-stamping movement appears; now this and now that member of the audience, within a brief period of time, 'gets' the illusion; and after it has established itself there is still plenty of time to observe the gray smudge. The demonstration requires only a few minutes, and is reliable. A disk of the dimensions given in the text is large enough for an audience of two hundred.

Both the characterization and the psychological description of our 'down' movement indicate that this perception is simply the downward phase of the Bourdon illusion; for some reason the upward phase is not seen. A, the *O* who most frequently made this report, suggests the reason. In her 'meaning' reports she says: "Movement was up and down in a continuous string of movements; the downward movements were more pronounced." "Black figures with smooth edges stepped down very quickly; the downward movement forces itself clear, the fall is so rapid." "Black on white beating down; movement of up was only secondary and did not attract attention." These reports were given when the *O* was obscurely aware of the upward movement; the lively gray flash coming suddenly caught the attention, and the more slowly moving surface black dropped to the lower level of consciousness. In the extreme case the upward movement does not get reported at all.

The individual differences in the frequency with which our *O*s saw the illusion mean that there is some condition which we have not controlled. Under our conditions the exposure time (the time of a single rotation of the disk) at the more rapid rates was very short; it may be that some *O*s would have had the illusion more frequently with a longer exposure time. Furthermore, in the course of our experimentation we found that even with a fixation point in the field steady fixation is difficult; our *O*s frequently reported that their eyes moved. We found also that a constant distribution of attention over the entire field of observation is difficult to maintain since the moving figure tends to demand attention. Finally, the fact that the illusion appeared with a frequency much greater when the disk was rotated in the clock-wise than when it turned in the counter clock-wise direction indicates that some more fundamental disposition or attitude was at work. Some item or items of this list may have been responsible for individual variation.

Summary. The Bourdon illusion appears more frequently and over a wider range of rates of succession of the figures when the disk is rotated in the clock-wise than when it is rotated in the counter clock-wise direction. With the former the range is from 0.2 sec. to about 2.0 sec., and the optimal rate is 0.6 sec.; with the latter the range is from 0.2 sec. to 1.0 sec., and the optimal rate for the two *O*s who had the illusion most frequently was 0.4 sec.

The downward movement (with clock-wise rotation), the illusory part of the Bourdon illusion, has as psychological correlate a filmy and lively gray which is not given objectively, and which is phenomenologically different from the black figures and the white background of the disk. With allowance made for differences in objective conditions, this gray is appar-

phenomenon.

Individual differences in the frequency of the illusion suggest the action of conditions not yet controlled.

BOOK REVIEWS

Handbuch der vergleichenden Psychologie. Herausgegeben von G. KAFKA. Bd. I.: *Die Entwicklungsstufen des Seelenlebens*, pp. vi.+526. Bd. II.: *Die Funktionen des normalen Seelenlebens*, pp. vi.+513. Bd. III.: *Die Funktionen des abnormen Seelenlebens*, pp. v.+515. Verlag von E. Reinhardt in München, 1922.

The brief Introduction which opens this comprehensive handbook has two purposes,—to explain and justify the titular use of the term 'comparative,' and to explain and excuse the actual plan and contents of the three volumes.

Every science, the Editor tells us, passes through three stages of development. It begins with hypotheses. It then seeks to test its hypotheses by applying them, for the most part deductively, to facts of observation; and the sounder the hypotheses turn out to be, the more surely do they lead to the discovery among the facts of special uniformities, of which they themselves in their generality had been unable to take account. As these local laws increase in number, the science feels the need, thirdly, of bringing them all together into uniformities of a higher order.

The three genetic stages are paralleled by three forms of established science. Every science, in its first period, seeks to group its facts under laws of the most general kind. It then passes into a period of specialisation, in which theoretical inquiry is restricted to special questions; in this period an applied science is likely to grow up side by side with pure science. In a third period it aims to supplement the general and special laws of the preceding periods by setting up general laws of the second order, which obtain between the facts and laws of the special fields of the second period; and it is in the work of this third period that it becomes, in the narrower sense of the term, comparative.

Psychology is no exception to these rules. The first period of scientific psychology, the period of Lotze, Bain, James, Spencer, Wundt, Ebbinghaus, is predominantly speculative. We find, no doubt, a certain amount of specialised and even of comparative investigation,—the work of Lazarus and Steinthal on social psychology, Waitz' Anthropology. On the whole, however, the science is directed upon general uniformities. The second period is a period of specialisation: psychologists are students of sensation, of feeling, of will, of attention, of memory, of the thought-processes, of psychological aesthetics, of social psychology, of applied psychology, of pathological psychology. Wundt could lay the foundations of his system by first-hand work in all departments of psychology. Nothing of that sort is possible to the younger generation, who are bound to specialise; though they are resigning themselves too easily to the limitations of specialised thinking. All the greater, then, is the present need of a comparative psychology, whose task it shall be "by means of the categories furnished by general psychology to group the results of special psychology into empirical laws," laws too special for general psychology and too general to fall within the competence of the special psychologies.

So much on the formal side; we have now to attempt a material definition of this comparative psychology. Current specialisation has taken four principal directions: those of genetic psychology, social psychology, pathopsychology, and special psychology in the narrower sense (psychology of sensation, feeling, will, etc.). Only the first three of these psychologies can contribute to comparative psychology. We find an analogy in biology. It is possible to investigate the phenomena of organic life solely from the

physical point of view; we then have a physiological mechanics, a physiological chemistry, a physiological electricity, etc. These are, of course, special fields; they belong, nevertheless, not to the special biologies that can serve as basis for a comparative biology, but to general physiology; and they take that place for the simple reason that they do not operate with the specifically biological 'function.' So it is, then, with psychology. What we have called 'special psychology in the narrower sense,' the psychology of 'faculties' taken in abstraction from the concrete life of mind, belongs properly to the general psychology of the first period; the other three psychologies, on the contrary, deal with the actual mental functions. Comparative psychology itself is thus primarily a biological psychology, and may accordingly be defined afresh as the science of the relations between the individual functions of the mental life.

The concept of function enables us, further, to bring out the systematic relationship of genetic psychology, social psychology and pathopsychology. Function implies norm; and we may therefore discard the loose nomenclature of 'social psychology' in favour of 'psychology of the normal functions of consciousness.' What, in fact, distinguishes 'social' from 'individual' psychology is not the collective as distinct from the particular object of observation, but rather the direction upon concrete mental 'functions' as distinct from abstract mental 'forces.' To be sure, the amended definition robs social psychology of certain genetic and abnormal phenomena (mental life of primitive peoples, cannibalism); but these find their proper place in the correlated genetic psychology and pathopsychology. And, finally, the restriction of social psychology to the over-individual effects of human environment is wholly unsystematic; for, on the one hand, all mental functions whatever are more or less affected by social influences, and on the other hand the definition draws an artificial line of separation between genetic stages and abnormalities whose conditioning is primarily social and those whose conditioning is primarily individual.

We are left, then, with the three coordinate psychologies: the psychology of the normal functions of consciousness, the psychology of the stages of development which these functions pass through (genetic psychology), and the psychology of the deviations of the same functions from their norm (pathopsychology). Here is the threefold basis of a comparative psychology in the sense of the handbook.

So far I have followed the Editor. I now break off to give a table of the contents of the three volumes.

Vol. I. The Stages of Development of the Mental Life

I. Animal Psychology, by G. Kafka (Munich), pp. 11-144
II. Psychology of Primitive Man, by R. Thurnwald (Halle), pp. 147-320
III. Child Psychology, by F. Giese (Halle), pp. 323-518

Vol. II. The Functions of the ...

I. Psychology of Language, by H. Gutzmann (Berlin), pp. 3-90
II. Psychology of Religion, by G. Runze (Berlin), pp. 93-180
III. Psychology of the Arts, by R. Müller-Freienfels (Berlin), pp. 183-336
IV. Psychology of Society, by A. Fischer (Munich), pp. 339-456
V. Psychology of Vocations, by O. Lipmann (Berlin), pp. 459-508

Vol. III. The Functions of the Abnormal Mental Life

I. Psychology of the Abnormal, by H. W. Gruhle (Heidelberg), pp. 3-151
II. Psychology of the Criminal, by M. H. Göring (Giessen), pp. 155-229
III. Psychology of Dream, by S. de Sanctis (Rome), pp. 233-329
IV. Psychology of the Sexual Life, by R. Allers (Vienna), pp. 333-506

The second part of the Introduction, as I said above, explains and, where necessary, excuses the plan and contents of the book. The Editor finds the arrangement of vol. I. satisfactory, and, for the present, exhaustive; someday a section on Plant Psychology may be in order. He is less satisfied with vol. II. Language, he remarks, can lay claim to special treatment on account of its threefold rôle as product, mediator and developer of psychical functions. The Psychology of Society, however, should have had more space; the relation of man to man is at least as complex, at least as finely differentiated, as his relation to God and the world. If, nevertheless, the single section stands over against the three sections devoted to Religion, the Arts, and the Vocations, the reason is historical: the psychology of society has in the past been conceived, too narrowly, as the psychology of custom, and custom has been set directly alongside of language, myth, etc.; so that the free course of investigation has been checked. This second volume should have contained, further, a section on the Psychology of the Sciences, ground for which has been broken by the studies of Groos and Jaspers; the Editor was unable to find a psychologist willing to undertake it. Lastly, the Introduction is at pains to defend the inclusion in vol. III. of the section on the Psychology of Sex. The sexual function as such, we are told, is altogether normal; but its activity may condition, both in the animals and more especially in man, disturbances of other biological functions, which themselves approximate the pathological, and which may further be the cause of a strictly pathological *habitus*; it is therefore from the point of view of aetiology that the function takes its place among mental abnormalities.

All these are matters of detail, though the detail is in some cases important. The main criticism which the Editor foresees, and the objective justice of which he admits, is that the handbook is, after all, not a handbook of comparative psychology; it is a collection of special monographs; and the work of comparison, the aim and justification of the whole enterprise, is therefore left to be done for himself by the reader. The excuse offered is that nothing else than a book of this sort is now possible, and that the writing of the handbook will at any rate bring our need of a comparative psychology into clear daylight, will arouse and reinforce interest in its particular problems, and—by showing where the work of the specialist is still incomplete—will serve to strengthen the foundations upon which it must ultimately rest. To all of which one may say Amen, while yet one wishes that the Editor had had the courage of his insight, and had taken upon himself something of the labour and responsibility that he now hands over to his readers.

Meantime, the specialist remains in possession of the field. I hope that we shall be able, in later numbers of the JOURNAL, to publish reviews by competent hands of the special sections of the Handbook. The names of the authors are a sufficient guarantee of good work, and the Editor deserves our grateful thanks for having persuaded them to cooperation in what is undoubtedly a valuable reference-book. Whether his teleological position is necessary, and how far teleological modes of inquiry can be scientifically fruitful, are questions that can best be raised in connection with the contents of the special sections.

E. B. T.

Experiments in Psychology. By WILLIAM S. FOSTER. New York, Holt and Co., 1923. pp. x., 309.

Whether the student's ultimate interest be psychology or something else, it is desirable that he should have at least an elementary knowledge of scientific method. In his *Experiments in Psychology* Foster has arranged a laboratory course which is designed as a "general introduction to scientific method" and at the same time as a "partial basis for more advanced work

in experimental psychology." The book is made up of two Parts and an Appendix. Part I consists of introductory matter, Part II of the experiments, while the Appendix gives directions for the construction of much of the simpler apparatus. There is no index.

The first chapter in Part I discusses the relation of the psychological experiment to scientific psychology, introducing a certain amount of controversial matter,—always a questionable practice in an elementary textbook. The next two chapters deal with the conduct of experimentation and with graphic representation, and are explicit, simple and well written. They should do much toward getting the class started right, and will lighten the burden of the instructor. The last two chapters take up representative values (including central tendency), measures of variability, and the reliability of comparisons of representative values. They discuss both the theory and, by the use of concrete illustrations, the methods of obtaining the values in question. These chapters should give the student a good idea how to work out the desired results; but, it is to be feared, will aid him little in understanding what they mean. For this the instructor must use supplementary material.

The experiments of Part II have been selected with the idea of including "methods of measurement and control which are characteristic of scientific procedure in general." Some are individual, the pupils working in pairs; in others the instructor is the experimenter (E) and all members of the class are the observers or subjects (S's); still others are demonstrational, the pupils taking notes on what they observe. They include such topics as space-perception, Weber's Law, reflex action, reaction-time, learning, taste and smell, dermal sensations, vision, correlation, attention, memory, association and judgment. The presentation of each experiment has three main parts: (1) an introductory statement that treats of the theory involved; (2) the experiment proper; (3) results and questions. The questions are excellent for bringing out the principles underlying the experiment and for impressing them on the mind of the student. Introspection is made use of at times but is not emphasized. The results are usually quantitative as well as qualitative.

The experiments seem to the reviewer well chosen, clearly explained, and workable with large classes. Should the work reach a revised edition a freer use of illustrations both of apparatus and for the tabular arrangement of results would perhaps be advisable, and would still further lighten the burden of the instructor. The book in its present form will, however, be of great use to instructors who deal with large classes, especially to those who aim to emphasize scientific method rather than to open the door for systematic psychology.

Clark University MILES A. TINKER

The Psychology of Power, by J. [illegible]
York, The Macmillan Company, 1923. pp. vi, 54. Price 75c.

In this little book Captain Hadfield proposes, "in the first place, to produce evidence of the existence of resources of power normally untapped; secondly, [to] show that these are psychic rather than physical in character; and, after discussing their relation to the instinctive emotions and the will, . . . [to] consider the means by which they can be made available" (4). To carry out his first aim he points to the many examples in his own experience with cases at the Ashhurst Neurological War Hospital, Oxford, of which the following one is typical: "A corporal, whose courage won the V.C., was for several days cut off from our troops, was exposed the whole time to bombardment (subsisting meanwhile on the barest rations), and yet, in spite of the awful strain, he came out feeling cheerful, elated, and

without fatigue. Several men with him had the endurance to pass through the same experience, but to the end were exhausted and broke down. The corporal had evidently discovered sources of power which were not exhausted by the terrible strain he underwent, but provided an ample resupply" (6).

Captain Hadfield thinks that Mosso's experiments in fatigue, which show that the *mind* fatigues much before the body, are proof of his contention that these "untapped resources" are psychic. He arrives at the same conclusion from his own experiments at the Hospital, which show that when, "under hypnosis, I had given the men the idea that they were very weak, the average grip was only 29 lbs., one of them, a prizefighter, remarking that his arm felt 'tiny, just like a baby's.' My suggestions of strength produced an average grip of 142 lbs. as against 101 lbs. which was the best they could do in their normal waking condition. A second test, measured by the time occupied in holding out a weight, gave similar results. In brief, when I suggested 'weakness', the full flood of energy was checked and the men were capable of only one-third of their normal strength, whereas by suggestion of 'strength' latent powers were liberated and their normal strength increased by half as much again" (11-12).

The application of the above discovery led to the discussion of the relation of will to instincts, because, in dealing with neurasthenics, Captain Hadfield noticed that the patient may be willing to get rid of his obsession but, at the same time, may be actually unable to do so. At this point Captain Hadfield is at issue with M. Coué. As he says, the "energies which give the driving force to our lives are not derived from the will, but from another source; they will be found to have their origin in the instinctive emotions" (22). "Instinctive emotions", for Catain Hadfield, are the same as for McDougall, from whom he is constantly borrowing ideas (23). At this point, especially in his unquestioning reliance on the old fashioned dualism of will and emotion, Captain Hadfield is not at all clear, particularly in his discussion of the relation of the "instinctive emotions" to will. For example: "The instinctive emotions give driving force to the will and put life into great ideas, and, being liberated like the winds from the cave of Aeolus, burst forth, either to do their work of destruction or, if rightly controlled, to speed us, with full-bellied sail, on the voyage to the harbour of our destiny" (23). What seems clear, however, is that a neurasthenic person must have confidence and self-assurance of his power before he is able to use it to his advantage. Therefore Captain Hadfield asks the question, why it is that in "men paralysed in arms and legs . . . the will was quite impotent to restore to health: yet there was nothing organically wrong with such patients. Why, then, were they paralysed? Why did not their strenuous efforts enable them to walk? Because they believed they could not move their limbs. They could say 'I will', but they had not learnt to say 'I can' " (40). What, then, is necessary to gain that all important power—confidence? "Confidence removes this paralysis and turns belief into a mighty impluse to act. It fills men with the strength which makes the soul master of its fate" (40). This, of course, gives the author an opportunity to argue for the ever-present need for faith—even in normal life—and that the reader finds to be, in the course of the book, the faith supplied by the "Christian's confidence and hope" (52). Captain Hadfield is really beyond the realm of scientific imagination in this part of the book, when he writes in the spirit of the following: "Nature is economic in her gifts: she will not give strength to those who will not expend it. These remain uninspiring and uninspired. She is lavish in her gifts to those who will use them, and especially to those who devote them to nature's altruistic ends, for such ends harmonise the soul" (41). "In some cases I have attempted to cure nervous patients with suggestions of quietness and confidence, but without success until I have

linked these suggestions on to that faith in the power of God which is the substance of the Christian's confidence and hope. Then the patient has become strong" (52).

The little book has, however, an intrinsic value, being suggestive to the students of psychotherapy, because it contains cases derived from the actual experience of a field worker. The interpretation, we fear, is confusing; it is beyond the realm of science as we know it to-day.

Northwestern University A. J. SNOW

The Unadjusted Girl. By WILLIAM I. THOMAS. Boston, Little Brown & Co., 1923, pp. xvii+261.

While this book is sociological in general content, there are, in its basic approach, some important implications for psychology, especially those phases dealing with educational and social matter. The author, in developing his psychological foundations for understanding the conduct of delinquent women, presents a schema of wishes as the fundamental motives of human behavior. These are: the desire for new experience; the desire for security; the desire for intimate, face-to-face response; and the desire for recognition. While this particular classification may be criticised, although it agrees fairly closely with Dunlap's, it is important to note that we have here a social-psychological treatment which is not founded upon a plan of theoretical instincts in the manner of McDougall and others. In fact, the author has long since broken away from attempting to write his social psychology in terms of innate tendencies. Rather he has emphasized for many years the importance of desires and social attitudes. These are concrete data, around which we may construct a psychology of social life.

Likewise for education, it is time we were giving up the futile attempt to segregate by statistical methods the "original nature of man" from the acquired. A better idea would be to trace genetically the development of deep-seated ideas and mental sets, with their affective and emotional accompaniments. A lead in this direction could be taken from Thomas' book and from his other writings as well.

While Thomas took his notion of the wishes from Freud, even before Freud was known in this country, he never fell into the mystical particularism prevalent in psychoanalytic camps. He made his wishes much wider than those of sex alone; and furthermore in his present treatment he has begun to see the important implications for social psychology of the work of Pavlov and Watson on the conditioned reflex. We still await, however, some one who will connect up systematically the contributions of Pavlov and Krasnogorski, on the one hand, and those of Sherrington and Watson on the other, with the work of Freud, Adler and Jung. When this day arrives, much of the present misunderstanding and mist will be cleared away from these allied standpoints.

In a foreword to this book Ethel S. Dummer, who has done much for the development of the juvenile courts of this country, writes one of the most succinct reviews of the foremost thought of the day upon women and delinquency that the reviewer has read.

University of Oregon KIMBALL YOUNG

Mind and Heredity. By VERNON L. KELLOGG. Princeton University Press, 1923, pp. v + 108.

This little book is a series of ten popular lectures delivered on the Louis Clark Vanuxem Foundation at Princeton University by the well known Permanent Secretary of the National Research Council. The first three or four constitute the best brief popular introduction to the problems of

Comparative Psychology known to the reviewer, and later chapters give an almost equally excellent account of the scope and limitations of mental testing. The main purpose of the course, however, is the examination of certain pressing social and national problems—education, immigration, communism, and democracy—in the light of biology and of the writer's recent experiences in Soviet Russia. It is all interesting reading.

E. C. S.

Intelligence Testing. By RUDOLH PINTNER. New York, Henry Holt and Company, 1923. pp. 406. $2.50.

This book is designed as a college text and as a guide for teachers interested in mental testing. Part I is largely historical, with a chapter on the concept of "general intelligence;" Part II describes the methods of testing; and Part III gives a brief summary of the results so far obtained.

Binet is the focal point of the testing movement. Two influences shaped his work: on the practical side, the need of more exact tests for the feeble-minded, the deaf, delinquents and the insane; and, on the side of technique, the beginnings already made in experimental psychology, eugenics, differential psychology and anthropology. Here the author does not, perhaps, give experimental psychology and anthropology all the credit due. After Binet, a critical trend led to revisions of his method and to the construction of performance-scales and group-tests, while practical applications continued to be made in ever wider and wider fields.

For "general intelligence" the author adopts Stern's definition: "the ability of the organism to adjust adequately to new situations." Spearman's two-factor theory has stimulated discussion which has clarified the concept; but the hypothesis of specific abilities has received a warmer welcome in many quarters. Nevertheless, for practical purposes "the concept of general intelligence as shown in the empirical requirements of an intelligence test as we know it today" is more important. The modes of expressing intelligence ratings are evaluated and subjected to valuable criticism.

In the Part on methods the different Binet scales are described rather fully. The American revisions are presented with special attention to the Stanford Revision, because of its importance and general use. The various performance-scales are next considered. Types of material common to group-tests, to which the mental testing in the army gave a great impetus, are then taken up, and several of the more common are briefly noted. But to obtain a satisfactory knowledge of any test, especially of any group-test, the clinical psychologists must supplement the information here given by much practice with the test itself.

In the third Part, dealing with results, the material is taken up under such headings as the feebleminded, the soldier, the school child, the college student, the delinquent, the negro, and the foreign-born. The summing up is factual and objective, the author seldom venturing to generalize or speculate. A final chapter deals with the inheritance of intelligence. The section as a whole is the best of its kind that we know; but its chief merit, as that of the book, is its usefulness as a guide for wider reading in the field of testing.

Clark University MILES A. TINKER

Human Character. By HUGH ELLIOT. London and New York, Longmans, Green and Co., 1922, pp. xvi, 272.

The author believes that a proper approach to the understanding of the human mind is to be made in no other way than through a knowledge of the emotions. Somewhat like Rousseau, he sees "the world of human life

governed, not by reason, but by passion, emotion, and sentiment." This contention is supported by an array of behavioristic and introspective evidence. Setting out with three major emotions,—egoism, love, and social or moral feeling,—the author attempts therewith to explain action, suggestion, belief, thought, genius, vice and crime. Egoism makes for the survival of the individual; love for the perpetuation of the species; social or moral feeling for the maintenance of society.

Elliot does not insist upon hairsplitting when it comes to a matter of terminology. Instinct and emotion are for him one and the same thing. The text is profusely sprinkled with gems from the great poets. In fact, the reader is led to suppose that Goethe and Shakespeare knew vastly more about the human mind than do the academic psychologists of to-day.

West Virginia Collegiate Institute F. C. SUMNER

Psychology: The Science of Human Behavior. By ROBERT CHENAULT GIVLER. New York and London, Harper and Brothers, 1921. Pp. 382.

The subtitle is the key to the contents of the book. Although the author speaks of a mechanistic psychology, the point of view is overtly that of a teleological behaviorism. "The brain is the great central telephone exchange whereby both vital and mental functions—life and thought—are regulated. It is the instrument whereby man's many energies may be guided and combined and brought into harmonious and profitable relations with one another." Mind is action and "*mental activity is a combination of physical, chemical and electrical energies developed by the nerves, muscles and glands of the human organism.*" These special mental energies always express themselves in actions, "some of which we call sense-perceptions, some we call thinking, some we call feeling, some we call skill, and so on throughout the long list of mental powers and functions." The problem assigned to psychology indicates the scope of the book. "For just as a scientific study of the physical world and of the chemical world and of the world of mechanics and geology arose out of an ambition to control and manipulate the physical environment, so the chief motive underlying the scientific study of psychology was a desire to employ the mind to the greatest possible advantage." The method of attack is by induction;"the above case of proof is typical of all scientific experimentation, and is called proof by induction."

This general orientation indicates fairly accurately what the headings as well as the contents of the 36 separate chapters must be. After the introductory chapters, the plan of procedure is, generally, first to reduce the particular mode of mind to kinaesthesis, controlled and guided by the [illegible] secondly to explain in neural terms, and finally to offer suggestions for further useful development. The treatment of attention is typical. "The mental activity known as attention is, like all other mental processes, a sensorimotor response to stimulation." "The distribution of the environment into focus and margin, foreground and background, is to be found in the principle of the *final common path.*" We are able to maintain any focus continuously, for "the kinetic system regulates the supply of energy to one set of muscles rather than to another, so that a response once begun is continuously maintained." The particular focal point depends on interests. Therefore, if we are to develop the powers of attention, we must develop our interests, a point missed by most authors of books on the "power of concentration."

The book, which is a combination of functional, dynamic and behavioristic psychology and good advice, contains nothing essentially new, although the remarks about hypnosis, will, mental hygiene and industrial adjustments, just as the above about books on attention, are sane and healthy enough to bear repetition. Since in the author's belief all sensa-

tions reduce, in last analysis, to kinaesthesis, it is apparent why a continuous series of sensations or ideas is necessary to maintain attention; for, after all, "environment drives the brain, and the brain drives the various organs of the body." But it is not so apparent how pleasure, which is not a sensation but a quality that attaches to any sensation, or how retinal disparity, as such, reduces to behavioristic terms: both seem to carry meaning in their own right; pleasure signifies a state of the kinetic system rather than is a state of that system, and the cues from the double images of binocular vision are subconscious. It also seems a little far-fetched to explain hypnotic suggestion in terms of the conditioned reflex, especially since we find nothing about the conditioned reflex in the previous chapter on suggestion. Here we are told that "autosuggestion is the basis of all suggestibility. What others say and do excites imitative tendencies in us only so far as we have subconscious leanings in that direction." I am free to confess that I do not know what the subconscious is; it may be a matter of conditioned reflexes.

The style and atmosphere of the book suit the contents very well. We deplore the lack of an index; the frequent failure, except when quoting from medical men, to give credit; the general lack of references; and in some cases, where quotation is almost verbatim, the lack of quotation marks.

For those who believe that science in general and psychology in particular have as their rightful subject the study of human conduct in all its various aspects, the book will be of interest; for those who believe that the province of psychology is the study of experience in some one or other of its various aspects, the book has no appeal.

Cornell University L. B. Hoisington

Introduction to the Use of Standard Tests. By Sidney L. Pressey and Luella Cole Pressey. World Book Company, Yonkers on Hudson, N. Y., 1922, pp. 263.

In preparing this manual the authors have sought to be comprehensive rather than to present an exhaustive treatment of any one phase of the testing question. Their avowed purpose is to produce a book for the use of teachers, supervisors, and superintendents not especially trained in the use of modern educational measurement methods. Questions of theory have been left undiscussed, and attention is given to the use of tests as they appear on the market.

The book is divided into three sections. In the first section, "How to Use Tests", the first chapter, entitled "What is a Test", discusses the difference between the modern Test and the "Examination", pointing out the advantages of each. The specific nature of the information gained from the use of a standardized test, the saving in time, and the possibility of a comparison between individuals and groups resulting from a standardized procedure in presenting and scoring standardized material, are the principal advantages of the "Test".

Chapter 2 is given over to "School Problems and Tests". These problems are presented in three separate groups: the problem of instruction, which is primarily of interest to the teacher; that of supervision, which concerns the supervisor or principal; and, finally, the problem of administration, which belongs to the superintendent. The applications of the different types of tests to these problems are indicated. In this connection the authors make a plea against testing merely for the sake of testing, and urge that the tester always have some practical problem in mind.

The third chapter, "The Common Sense of Statistics", is especially worth-while. It stresses the fact that statistical treatment is only "refined

common-sense" used in finding "convenient ways of handling data." Each of the statistical methods ordinarily employed in educational measurement is taken up from this point of view with particular emphasis placed on the value of tabulations. The general thesis of the chapter seems to be that statistical treatment should ultimately yield not simply a median or statement of relationship, but also some idea of what must be done to solve the problem at hand.

Chapter 4, on "The Use and Misuse of Tests", is devoted to pointing out certain common errors and their attendant results.

Section 2 of the book includes chapters 5 to 10. It deals with the tests used in measuring achievement in school subjects. Under the headings "General Nature," "Practicability," and "Use" the section discusses "Tests in Content Subjects: History and Geography," "The Measurement of Ability in Written English," "Tests in Reading," "The Measurement of Handwriting," and "Tests in School Subjects," giving a chapter to each.

Section 3 on "Tests of General Ability" points out the need for such tests in supplementing achievement tests and describes a number of tests of this type (Chapter 11). More specific treatment of the use of these tests in problems of vocational guidance, the study of disciplinary cases, and the comparing of classes and school systems as to "pupil material" is given in the final chapter of this section (Chapter 12).

The concluding section bears the heading "Important General Principles Regarding Tests." "How Tests are Made," "The Testing Program," and "How to Make the Testing Program Worthwhile" are chapters particularly suited to the educational executive who lacks an intensive training in standard measurements. They are designed primarily to prevent the waste of time that comes from "testing for testing's sake."

There are four appendices which supplement the contents of the book. Appendix A gives detailed instructions for "Finding the Median for Large Distributions;" Appendix B covers "Tests and the Diagnosis of Feeble-Mindedness;" Appendix C gives "Suggestions for Further Reading;" and Appendix D is a list of "References for Further Reading."

A further convenience is a glossary of technical terms. In the book these terms are indicated, so that the reader may know whether or not an unfamiliar word will be found in the glossary.

Cornell University F. L. Bixby

PSYCHOLOGICAL PERIODICALS

L'année psychologique, vingt-deuxième année (1920-1921). Ed. by H. PIÉRON.* Paris, Félix Alcan, 1922. pp. xii, 608. Price 40 fr.

The plan of the 22nd volume of the *Année* is the same as that of its immediate predecessors: a few original papers, notes and reviews, digests of psychological literature, and the year's chronicle. The *Analyses bibliographiques*, which occupy more than half the volume, cover the two years 1920 and 1921. This section of the *Année* is therefore, for the first time since the war, brought up to date. Henceforth the digests will be limited to the books and papers that belong properly to the year's review. In his choice of articles and books for report, the editor has attempted to do justice to the entire range of contemporary psychology. He thinks it worth while, however, to state that in the multitude of Freudian articles he has found nothing useful. "While the fundamental concepts [of Freudian psychology] merit examination and discussion, the concrete observations and particular applications of its method coming from believers without criticism and impregnated with subjectivity are of no general value; the collection of sexual interpretations is like a collection of brilliantly colored butterflies."

The first of the *mémoires originaux* is an investigation of visual qualities beyond the central region of the retina (*Les sensations visuelles élémentaires en dehors de la région centrale de la rétine*) by M. Foucault. Then follow investigations of the behavior of various species of spiders (*Recherches expérimentales sur le comportment de diverses araignées*) by É. Rabaud; of the law which relates the duration of latent intervals to the intensity of stimulus (*Nouvelles recherches sur l'analyse du temps de latence sensorielle et sur la loi qui relie ce temps à l'intensité de l'excitation*) by H. Piéron; of motor reactions in emotional crises (*Les réactions motrices dans les crises dues à l'émotion*) by H. Wallon; of meaningless tactile impressions (*A la recherche d'une sensation tactile pure*) by J. Philippe; and of the mental functions of children of school age (*Recherches sur les fonctions mentales de l'enfánt à l'âge scolaire*) by J. Abramson. There are, then, three papers in experimental psychology, one in animal, one in child psychology, and one in psychology of the abnormal. Wallon's study is an analysis of cases resulting from the war; and Abramson turns again to the old problem of measuring the functions of apprehension, imagination, memory and thought. Of the three papers in experimental psychology, that of Foucault reports the discovery of the intermittence of sensation in indirect vision, and of a critical region, of 5° or 6° in extent, surrounding the blind spot, where there are alterations of hue which resemble those in the eccentric retina. His stimuli were colored papers, and his experiments are too few in number to permit of a general conclusion. The study of Piéron is one of a series devoted to the general law that "when an organism submits to stimulation and reacts to it, the duration of the interval which elapses between stimulation and reaction varies inversely as the intensity of stimulus." This law has already been shown to hold for reflex actions in the lower animals and for reactions to sensation in man. The author now seeks a mathematical formulation for it, and finds that the decrease of reaction time regarded as a function of increase in intensity of stimulus follows an hyperbolic curve and tends asymptotically toward a limit. When checked by experiment the interpolation formula, while remaining true to type, is found to vary with sense modality. Piéron then undertakes an interpretative analysis of the formula by reference to the peripheral and central nervous system. The explana-

tion is cleverly worked out; but in so far as it relates to human reactions to sensation, it could be more readily accepted if an analysis were first made of the reaction consciousness as regards both the type of reaction and the nature of the 'sensation' to which reaction is made. In the third paper, Philippe reports that in the two-point limen experiment the nature of the cutaneous perception is conditioned upon the observer's knowledge of the stimulus. When, for example, the stimulus is quite unknown, a blindfolded observer neither perceives two discrete points nor is able to characterize the perception by reference to object. The same observer, however, as soon as he knows something about the stimulus, perceives patterns similar to those described in other investigations. Philippe thus challenges the traditional psychophysical experiment; and although he does not bring his results into relation with others in the same field, it is apparent that they support Titchener's critique of the two-point limen as an ethnological test, and Friedline's explanation of the failure of the experiment as a measure of fatigue.

Perhaps the most conclusive result of all the experimental papers is that of Rabaud's study of spiders. By careful observation of different species with different kinds of prey, and then by skilfully conducted experiments with artificial stimuli, he not only substantiates the fact already known that the spider reacts to a situation and not to an object (as, for example, to fly-on-web and not to fly as fly), but he is able to go further and to analyse the situation into a 'vibrating web.' By means of tuning forks held in contact with the web and vibrating with the proper frequency, intensity and intermittence he succeeded in inducing all the characteristic behavior of his spiders, including typical variations in the order of the acts employed in attacking and killing their prey.

The Notes and Reviews consist of two short papers, the one on a mode of economic brain-function (*Mode de fonctionnement économique du cerveau*) by A. Imbert, and the other on new laboratory apparatus (*Appareils nouveaux de laboratoire*) by H. Piéron. In the former it is argued that the cessation of respiration in muscular effort serves to relieve the brain from control of the necessary changes in rhythm of respiration, and thus permits a more economic employment of the brain's energy for the muscular effort. In the latter Piéron describes a pair of rotating discs whose absolute and relative rates of rotation may be controlled, a new differential photometer, and an aesthesiometer which is similar to von Frey's but in which a glass or quartz thread takes the place of the hair.

The *Chronique* contains the necrology of the two-year period, some personal notes, news of psychological periodicals, and reports of meetings, and of new psychological organizations, commissions, and institutes. The last of these topics has a summary account of the development of applied psychology in Germany.

As a whole this volume of the *Année* appeals to us as better than that of the preceding year; the original papers are more timely, and are in better accord with the present status of experimentation in their fields; while the digests are still of the high order which we have come to expect. The spirit of the book is that of physiological psychology; and there is, therefore, a certain fitness in the fact that this volume is dedicated to the memory of Henri Beaunis, who died in 1921, and who was the father of physiological psychology in France.

H. P. W.

Zeits. f. Psychologie. Bd. 91, Heft 1 u. 2. W. Blumenfeld. 'Untersuchungen über die Formvisualität: I.' [*Formvisualität* is defined as the capacity to produce and manipulate in visual ideation bidimensional or tridimensional forms. The present paper offers a critical examination of some thirty tests or types of test, classified tentatively into four groups according as the material for ideation (bidimensional or tridimensional) is

given to perception or is itself ideational. There were thirty observers, principally mathematicians, engineers and architects. Provisional treatment of the results by rank-order methods shows that four of the examined tests possess a high symptomatic value; and their discussion enables the author to lay down with some definiteness the conditions which an adequate test of *Formvisualität* must meet.] E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: VI.' E. R. JAENSCH. 'Uebergang zu einer Schichtenanalyse des Bewusstseins und einiger seiner Substrate, gegründet auf die Strukturanalyse der eidetischen Entwicklungsschicht.' [Hitherto the study of eidetic phenomena has been confined to cross-sectioning. But if our adult worlds of perception and ideation have developed in time out of a matrix in which the *Anschaungsbild* predominated, then the study must be extended to longitudinal sections: we must trace the eidetic history of the individual from early childhood, and must also apply the eidetic touch-stone to the adult psychology of primitive peoples.] E. R. JAENSCH. 'VIII. Die Völkerkunde und der eidetische Tatsachenkreis.' [A study of Lévy-Bruhl's *Les fonctions mentales dans les sociétés inférieures* brings abundant evidence of the eidetic *Anlage* of primitives; extraordinary memory, confusion of *Anschauungsbild* with perception, phenomena of 'privileged' perception (shamans), mythological figures, multiplication of names (especially of spatially determined names) for concrete objects, phenomena of mystical participation (the 'internal' *Anschauungsbilder*), artistic productions.] Literaturbericht. F. HILLEBRAND. 'Berichtigung.'

Bd. 91, Heft 3-5. F. SCHUMANN. 'Untersuchungen über die psychologischen Grundprobleme der Tiefenwahrnehmung: III.' W. FUCHS. 'Experimentelle Untersuchungen über das simultane Hintereinandersehen auf derselben Sehrichtung.' [Reports experiments made by various methods to decide the question whether two objects in the field of vision can be seen simultaneously at different distances in the same visual direction, *i.e.*, whether monocularly one and the same retinal point or binocularly identical retinal points can mediate two impressions of different color at different distances; Helmholtz had affirmed, Hering denied the possibility. The result for stimuli in a single plane (*Flächengestalten*) is positive, under the condition that the two objects are apprehended as shaped wholes (*Ganzgestalten*), *i.e.*, that some parts of the nearer object project beyond the farther object, or better still that parts of each object project beyond the other, while the common field does not stand out as a special form. Transparency is abolished (1) if the form of the common field becomes dominant; the *O* then sees a 'mixed' color; (2) if attention is so occupied with points or contours of the farther object that the nearer figure ceases to be seen as shaped whole; and (3) if the nearer figure is so small as to be surrounded by the farther, and the farther figure itself is homogeneous. This last result may be reversed by voluntary 'inversion' (localisation of the nearer behind the farther figure), which can also turn opaque objects (strips of paper, iron rods) into transparent. Movement of one or both objects enhances (or may produce) the impression of transparency by enhancing (or producing) the impression of shaped wholes. The color of the projecting parts exerts a strong assimilatory influence on the color of the corresponding whole figure; the author promises to deal in a later paper with the general subject of color change as conditioned by apprehension of form.] W. BLUMENFELD. 'Untersuchungen über die Formvisualität, II.' [Tests with wire figures (tridimensional bending, ends not brought together; symmetrical pairs) lead to more satisfactory results than even the best of the tests previously discussed. The new tests are two: (1) comparison: two identical or symmetrical figures are shown in different positions; the subject is to decide whether they are identical or symmetrical; (2) recogni-

tion: a figure is exposed for impression (*Einprägung*); after its removal single figures, in different positions, are shown; the subject is to say whether or not the figure now shown is identical with the figure impressed. Different subjects work in different ways: by imaginative shifting of the seen figures, by imaginative adoption of a different standpoint toward them, by projecting the body into them, by 'traversing' them in imagination; in all these procedures, even if kinaesthesis plays a part, visual functions are essentially involved. The same thing appears to hold of identification by fingering.—A third test, in which the drawing of a figure was shown for perception or impression, and the subject was required to identify the figure in a group of simultaneously exposed identical or similar figures proved unsuccessful.] Literaturbericht.

Bd. 91, Heft 6. E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: VIII.' H. FREILING und E. R. JAENSCH. 'Der Aufbau der räumlichen Wahrnehmungen.' [Maintains that 'movement of attention' is the primary condition of spatial localisation. (1) Experiments on rotation show that *Anschauungsbild* and after-image are localised at the point of attention, uninfluenced by nystagmus. (2) Movement of attention has a dynamic component; *Anschauungsbilder* of heavy objects are moved less easily than those of light objects. (3) This component accounts for the absence of localisation in tones, and for the connection between space-perception and motility. (4) The antagonistic processes of attention (near-far, right-left), attested by the tendency to orthogonal localisation and by the pleasure in symmetry, derive (like those of color vision) from a self-regulative tendency of the organism. (5) A perceptive complex formed under the influence of movements of attention may persist for some time, engrammatically, after its conditions have ceased to be effective. The complex of the normal world of perception is a like engram of indefinitely stronger impression. (6) Movement of attention is prior to the space-values of the retina, but avails itself of organic structures and functions for the consolidation of perception.] E. R. JAENSCH. 'IX. Beziehungen von Erlebnisanalyse und Sprachwissenschaft, erläutert an den Verben der sinnlichen Wahrnehmung.' [Verbs of sense-perception take the accusative because the functions of perception, in the eidetic stage, stand in causal relation to the objects of perception,—influencing, changing, even constructing these objects.] E. F. HAZELHOFF und H. WIERSMA. 'Untersuchungen über die Frage der sensorischen Asymmetrie.' [Experiments with liminal faradisation, with passive application of light weights (for discrimination), and with the aesthesiometer show that, in right-handed persons, the left hand is the more sensitive. The authors suggest that the development of motor functions in the right hand has inhibited its sensitivity . In experiments with liminal faradisation, under sensory and motor instructions, the motor attitude is definitely more effective to reduce the right cases for the right than it is for the left hand. A single left-handed subject gave opposite results.] Literaturbericht. Einladung zum achten Kongress für experimentelle Psychologie in Leipzig.

Bd. 92, Heft 1 u. 2. E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: XII.' E. R. JAENSCH. 'Der Umbau der Wahrnehmungslehre und die Kantischen Weltanschauungen, nebst einer Erörterung über Johannes v. Kries' Grundlegung der Wahrnehmungslehre.' [The older sense-physiologists, Johannes Müller, Helmholtz, Aubert thought that they were following in the footsteps of Kant; they were, in fact, upon a side track. We find, nevertheless, if we look at current Kantian theories of knowledge, that they have a common psychological substrate, whose central problems are open to technically

psychological examination. It is the main thesis of the paper that such examination proves the psychology inadequate.—The author starts out from his own newer nativism, which stands to the older nativism as the Kantian a priori stands to the doctrine of innate ideas; it is concerned with the functions and methods whereby a plastic content is moulded. He then criticises von Kries' argument that the key to theory of perception is to be found in judgments of reflection; the eidetic psychology meets all difficulties, since it shows that the invariance of perceptive phenomena derives from that of the *Anschaungsbilder*. Next he disposes of the objections that genetic considerations are not decisive in the Kantian issue; that psychology has no relevance to the philosophical question; and that his own starting-point in psychological optics, *i.e.*, in phenomena of consciousness, confirms rather than refutes the Kantian position. Finally, he makes a plea for realistic as opposed to transcendental idealism: values, in particular, must not be banished from psychology.] T. SCHJELDERUP-EBBE. 'Weitere Beiträge zur Sozial- und Individualpsychologie des Haushuhns. [(1) Lists, describes, and refers to sex and occasion, all cries of the domestic fowl: laying note, cackle (or rhythmical note), crowing, warning-note, cooing, courting-note, cry, threat-note, nest-notes, lust-note, clucking, invitation-note, minor cry, alarm-note. In general, chicks do not learn their cries by imitation; with the single exception of the crowing of cockerels, the cries appear earlier in isolation than in the (inhibiting) company of older birds. (2) Hens blush under pain or emotion; the cock's face is too red and leathery for observation. Both sexes pay keen attention to face and head, less to body color, of their companions. (3) Recognition of locality is well developed; and hens brought up together may recognise each other after a three months' separation. (4) The law of pecking (Bd. 88) appears among cockerels earlier than among hen-chicks; it holds, in specialised form, for the companionship of young and old birds, and birds of different sex. A hen whose appearance is altered by sickness is attacked, probably because she is not recognised.] A. HERMANN-CZINER. 'Experimentelle Untersuchungen zum Problem des Verstehens.' [Reports experiments, with words (for the most part abstract conceptual terms) as stimuli, upon the process of reproductive understanding. The experience of understanding has three forms. (1) Simple understanding is characterised by isolation and unresolved unity; word and meaning are quasi-identical; there is no conscious apprehension of meaning, and no objective reference. (2) In represented understanding the representation of meaning may be impalpable (affective, motor) or palpable (mostly visual images); the experience is still unitary. (3) In explanatory understanding the meaning becomes conscious either in its determination by other meanings or in its relation to other meanings. The first of these experiences is still unitary, differing from simple understanding, not psychologically, but only by its intentional object; the second is discontinuous, implying the arousal either of fresh experiences of understanding or of thought-fixations and voluntary acts. The most frequent combinations are of (1) with (3) and of (2) with (3); conscious connection appears rarely in the former case, never in the latter; the first member is a closed unity. The three forms are, in certain circumstances, equally satisfactory. The visual images of (2) are, negatively, symbolic rather than representative; positively, they unify the whole experience and give the represented object a sort of form-quality. Form (3) occurs most frequently (owing to the nature of the *Aufgabe*), but may be characterised as irrelevant or unimportant (probably because the understanding is reproductive); it appears whenever the stimulus-word can be set in a thought-context. The results refute all sensualistic, associational and judgmental theories of understanding; they necessitate the assumption of experiences of the nature of form-qualities (for unitariness) and of unconscious processes (for discontinuity) and the adoption of a genetic point of view (for origin of representative images).] Literaturbericht.

Bd. 92, Heft 3 u. 4. H. HENNING. 'Starre eidetische Klang- und Schmerzbilder und die eidetische Konstellation.' [The eidetic experience is not so much an image as a state or attitude of mind in which the ego plays an active rôle. Pains, and impressions of the lower senses in general, tend to orient the ego toward reality, and so favor the eidetic attitude; supervening visual and auditory reproductions are then likely to assume eidetic form; it is thus, and not physiologically, that Urbantschitsch's results with heterogeneous stimulation are to be explained. We find, indeed, that persons who show no trace of the eidetic disposition in direct experiments may get eidetic experiences if several sense-departments are together brought into play; others get them only under the freer conditions of everyday life; others only in the hypnagogic state; others again only in dreams. The experiences are referable, physiologically, to centrifugal stimulation of the end-organ.—Various modes of auditory eidetic phenomena are described.] H. HENNING. 'Eine neuartige Komplexsynästhesie und Komplexzuordnung.' [In the case described, numbers and letters, presented visually or auditorily, are accompanied by a constant and compulsory synaesthetic experience, which is characterised in precise visual terms and yet is not a palpable visual image. The concomitant proves to be a complex quality, the total conscious attitude that is aroused by perception of a color, only with the color-perception itself left out. So we may experience the same Beethoven-complex-quality as we listen to a symphony or as we contemplate the Beethoven mask.] H. HENNING. 'Ein neuartiger Tiefeneindruck; die Versuchsanordnung des Rasterdiapositivs.' [Two stereoscopic positives, each of which has been taken through a fine square-meshed screen, are superposed on the back of a transparent celluloid plate, the front of which is etched in vertical lines of the same separation as the corresponding lines of the screen; in transmitted light the arrangement gives a plasticity far exceeding that of the stereoscope. A series of apparatus, ranging from this through the stereoscope, etc., to reversible perspective figures and thence finally to a plane figure, permits the demonstration of phenomenological tridimensionality. The impression of depth depends on detachment of background and apprehension of objective (*dinglich*) form. A study of Oriental pictures shows, in particular, the influence of ground-level on perspective interpretation.] T. CHIBA. 'Ueber die Asymmetrie der Unterschiedsempfindlichkeit.' [The times of discriminative reaction to pressure intensities, and the results of experiments on discrimination of tonal pitches and light intensities, prove or indicate (what is suggested by earlier studies) that sensible discrimination is better above the standard stimulus than below. This asymmetry is primarily a matter of relativity or contrast; in particular, attention in the ascending series goes to the variable, in the descending to the standard stimulus, so that ascending change is the more noticeable. There is also a tendency, in all sense-departments, for the sensation aroused by the standard stimulus to approach the proper or intrinsic sensation of its modality; hence decrease of stimulus becomes less noticeable than increase. Teleologically, the asymmetry of discrimination makes for protection of the sense-organ.] Literaturbericht.

Bd. 92, Heft 5 u. 6. W. FUCHS. 'Experimentelle Untersuchungen über die Aenderung von Farben unter dem Einfluss von Gestalten ("Angleichungserscheinungen").' [Reports experiments on color assimilation (1) when both forms are unitary surfaces (*e.g.*, episcotister set up before a large letter cut from colored paper); (2) when one form is composed of discrete elements (*e.g.*, row of colored circles, seen by reflection, the middlemost circle behind a square of a different color); and (3) when both forms are discrete (bicolored patterns of small colored discs, with or without reflection). The results are in all cases of the same type: we quote the summary given under (1). "The color of the projecting parts has a marked

influence upon the color of the common part; this, by assimilation, partakes as it were of the color of the projecting parts, and in its turn tries to assimilate the projecting parts in color to itself. The reciprocal assimilation is stronger, the more successful the pregnant apprehension of the total form of each surface. In the extreme case, each surface is of one and the same color in all its parts. The assimilation is a consequence of form-apprehension." Experiments on (4) negative after-images and (5) contrast bring out like facts; the transformations of after-image color with shift of form-apprehension are especially noteworthy. The results are offered in explanation of a large variety of phenomena: our overlooking of small objects that lie before us, the even color of a wall covered with monotone paper, the filling out of the blind spot, etc. An appendix discusses assimilation of form, size and localisation.] E. VOIGTLAENDER. 'Ueber die "Art" eines Menschen und das Erlebnis der "Maske;" eine psychologische Skizze.' [Manners, deportment, social carriage are very subtly compounded of genuine and deceptive elements, and stand in very various relation to the true inner character of a man; it is the task of characterology to work out this relation. If the position, character, rôle assigned to a man by his social environment does not accord with his true aims and nature, he is likely to assume a protective mask: Nietzsche is an outstanding instance.] K. MARBE. 'Ueber den Okkultismus; Erörterungen im Anschluss an v. Schrenck-Notzings Materialisationsphänomene.' [Schrenk-Notzing's book is valuable both as a cultural document and as an example of current popular psychology. Those of his kidney cannot be argued with; they have an unshakable will to believe. The writer, nevertheless, offers his laboratory as a testing place of any medium who is willing to be tested under strict conditions.] J. PLASSMANN. 'Psychologische Erfahrungen mit einem Zeitsignal, II.' [Cf. Bd. 88. The results show, as before, that the three-long signal is apprehended too early, the long-short somewhat too late, and the long-long-short considerably too late. Almost all the ten signals of the second minute (in which the beginning and not the end of the Morse stroke is apprehended) are perceived a little too late.] Literaturbericht. Zweiter Kongress für Aesthetik und Kunstwissenschaft.

Bd. 93, Heft 1 u. 2. G. E. MUELLER. 'Ueber Jaenschs Zurückführung des Simultankontrastes auf zentrale Transformation.' [Shows, by critical examination of Jaensch's procedures and by appeal to relevant facts of which Jaensch has failed to take account, that the reduction of contrast to transformation is impossible. Physiologically, the zone of transformation lies behind (centrad of) the zone of contrast.] W. KORTE. 'Ueber die Gestaltauffassung im indirekten Sehen.' [Campimetrical experiments (horizontal meridian) on the cognition of letters (large and small Roman, large and small German) and of meaningful and meaningless words (various lengths, small German letters). The cognition of a meaningful word begins with the total visual impression, which may of itself touch off a verbal report: this is almost invariably wrong. Then follows the identification of dominant letters (these may sometimes be small letters) as constituents of the total impression. Their identification arouses an auditory-motor image, *i.e.*, another total impression or *Gesamtform*, whose formation is essentially determined by meaningful connection. As soon as the dominant letters are noticed, a *Gestaltungsdrang* becomes effective; it operates together with visual form, auditory-motor form and meaningful connection to raise a word-meaning to a high state of preparedness; this word-meaning fuses, as a sort of complex-quality, with the other three moments The true from of the stimulus-word is then cognised. (The author thus takes a position between the extremes of Erdmann-Dodge and Zeitler, but nearer to Zeitler.) Among the conditions of mistaken reading we may note the abbreviation or condensation of the perceptive image in a certain region of the retina; the

author leaves its explanation open for special study.] A. GELB. 'Farbenpsychologische Untersuchungen, I.' A. GELB und R. GRANIT. 'Die Bedeutung von "Figur" und "Grund" für die Farbenschwelle: erste Mitteilung.' [Describes experiments in which a tiny disc of colored (usually red) light was thrown upon a colorless figure (Maltese cross at center of cardboard disc) or ground (rest of disc-surface). There were four discs in which the figure was darker than the ground; in all four the brightness of the figure, that of the ground, and the brightness-difference between figure and ground, were different; and there were four discs in which these brightness-relations were precisely reversed. The main result is that the color-limen is always higher for figure than for ground, and that the difference is greater, the more striking the difference between figure and ground. The explanation is that the little color-disc is itself a form, and that (owing to the tendency to pregnancy of form) the already formed figure resists the new form more energetically than the non-figured ground. Further experiments (incomplete) indicate that the *DL* of brightness also depends on the figure-ground difference.] Literaturbericht.

Bd. 93, Heft 3 bis 6. E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: XIII.' E. R. JAENSCH. 'Wahrnehmungslehre und Biologie: die Lehre von den Gesichtswahrnehmungen vom Standpunkt der Organologie des Auges (mit einer Beilage über José Ingenieros' Versuch einer Grundlegung der biologischen Psychologie, und seine Kritik der Psychologie in Deutschland).' [The structural relationship between the original visual phenomena and ideas is far closer than that between our developed visual perceptions and ideas. We should therefore expect to find that the eye is not a mere mechanism of reception and conduction, but rather a brain-organ. And we find, in fact, that the eye begins as a brain-pocket, while many features of its structure (inversion of the light-receptors, plexiform layers, centrifugal fibres) indicate a continuing brain-function; only at the fovea is the ordinary schema of a sense-organ realised. We find further that the incidence of a 'central factor' in perception over the lateral retina, in individuals of different age, and as between different individuals in general, is paralleled by anatomical characters. We may even suppose that in the foetus, as the eye is pure brain, so the visual experience is pure dream. The brain-nature of the visual apparatus accounts for the assimilation of our world of scientific ideas and theories to the world of visual perception.—The article contains, besides the critique of Ingenieros' book, a criticism of the *Gestalt*-psychology.] E. R. JAENSCH. 'XIV. Ausblicke auf kulturphilosophische und pädagogische Fragen und die Jugendbewegung unserer Zeit.' [Eidetic studies have emphasised the plasticity of human nature, and therefore justify a pedagogical optimism.] T. SCHJELDERUP-EBBE. 'Aufmerksamkeit bei Mücken und Fliegen'. [In the attitutde of attention *culex pipiens* raises its hind legs, *musca domestica* (with some other flies) plants its forelegs firmly and slightly raises the forepart of the body; the attitudes are teleological. Flies are very sensitive to temperature changes.] D. KATZ. 'Kleine Beiträge zur angewandten Tierpsychologie.' [Flies in movement are more easily caught than flies at rest; their visual perception during movement seems to be reduced. Under the influence of injurious stimuli both flies and gnats show a pronounced phototaxis.] D. KATZ und A. TOLL. 'Die Messung von Charakter- und Begabungsunterschieden bei Tieren (Versuche mit Hühnern).' [Five birds, which showed in human regard marked differences of character, were tested for memory, apprehension of relations, 'counting,' reaching a high-swung food-dish, leadership, reaching the food-dish by indirect paths, and spontaneous behavior (order of leaving and entering the coop). The rank orders are stable enough to suggest that it would be worth while to apply analogous tests to dogs.] T. SCHJELDERUP-

EBBE. 'Beiträge zur Analyse der Träume.' [In a well-defined half-conscious state between waking and sleeping 'sentences' form themselves, which show on analysis a marked flight of ideation, change of subject within the single sentence, an inexhaustible range of associations, and a manifold formation of meaningless words. The writer brings these sentences into relation with the speech and logic of dream and with some types of insanity.] T. G. HEGGE. 'Ueber Komplexbildung in verschiedenen Gebieten der Gedächtnistätigkeit.' [Compares in detail the formation of complexes in the purely visual learning of series, *e.g.*, of numbers or consonants with their formation in the learning of visually presented series of substantives. The tendencies in 'mechanical' and 'illustrative' memorising are essentially the same: stable connection, limitation, unification, differentiation; indeed, 'logical' grouping will be likely to intervene in mechanical learning, as there is always a mechanical component in logical learning. The differences are differences of means, not of ends: collective impression is of less importance for logical learning; unity is not so much a matter of form as of pragmatic and causal relations; range is a matter not of energy of attention and characteristic total form but rather of possibility of 'natural' connection; etc. In principle, the resemblances are far more striking than the differences.] Literaturbericht.

Bd. 94, Heft 1 u. 2. C. STUMPF. 'Singen und Sprechen.' [The main difference between speech and song, phenomenally regarded, is that speech makes wide and varied use of continuous tonal changes, whereas song (like music in general) confines itself in principle to fixed intervals and discrete tones. The difference is one of degree, and there are all sorts of approximations, mixtures, transitions; it is, nevertheless, primary and decisive. The emotive effects of speech, *e.g.*, are connected with continuous tonal changes, while those of music are connected with fixed intervals between fixed tones. Secondary differences consist in the larger part played in speech by noises, the reduced clearness of pitch in unaccented syllables, the greater freedom of temporal course.—Köhler thinks that the sounds of speech, as contrasted with those of music, lack the attribute of 'musical quality' (*c*-ness, *d*-ness); but the hypothesis does not bear examination. It leads, however, to a consideration of the phenomenology of continuous tonal change or tonal movement. The moving tone appears to differ only in degree from the resting tone; but, again, the difference is very great. Intensity is fairly clear; pitch less clear; clang-color still less clear; and musical quality least clear of all, distinguishable only by the trained ear in slow tonal movements. Köhler himself admits that a 'gliding' musical quality is conceivable.—Greater than the phenomenal differences between song and speech are those in the subjective attitude of singer and speaker; both general intention and special conscious set are markedly different.] E. R. JAENSCH. 'Ueber den Aufbau der Wahrnehmungswelt und ihre Struktur im Jugendalter: Schlusswort.' E. R. JAENSCH. 'Ueber Gegenwartsaufgaben der Jugendpsychologie.' [The eidetic studies so far published do not magnify a chance discovery; they are the outcome of a determinate plan, which goes back to 1909. The resulting child-psychology will replace, fruitfully, the economy-and-technique studies of experimental pedagogy.—The paper ends with a critique of the *Gestalt*-psychology.] A. PRANDTL. 'Die Koordination der Gehirn- und Bewusstseinsvorgänge.' [Coordination, in the sense of Mach and Avenarius, should replace the theories of parallelism and interactionism. Parallelism gives up the uniformity of the psychical life, and fails of explanation by way either of brain or of an unconscious; interactionism either gives up the uniformity of the physical or reduces the psychical to an epiphenomenon. Both theories have the same fundamental faults: they start out from the notion of 'action' (*Wirkung*), which is altogether unscientific (here the author interpolates a

discussion of the concept of causation), and they involve that doubling of perception and object of perception which, in its ultimate reach, was the object of Avenarius' polemic against introjection. What, then, remains? We are not helped by the distinction of act and content, subjective and objective, qualitative and quantitative; nothing remains but to fall back, with Mach and Avenarius, on a difference of our own standpoint over against the given. The objections that a 'physical' object must antedate its corresponding brain-process, and that certain brain-conditioned experiences may still be 'physical,' are readily met.] Literaturbericht. Notiz. [Congress of Applied Psychology at Würzburg.]

Archives de psychologie. Tome xviii., No. 3. A. Lemaitre. 'Les avatars d'une servante mystique: contribution à la psychologie religieuse.' [Describes the traits and experiences of a servant-girl of markedly mystical temperament: inner illumination rising to hallucination, abiding belief in the guidance and protection of a divine presence, admiration of nature, lyrism, altruism, clairvoyance. There are clear traces of the Electra complex. The mystical experiences have a biological value.] J. Piaget et P. Rossello. 'Note sur les types de description d'images chez l'enfant.' [Individual clinical tests of the description of pictures carried out with 43 children, 8 to 10 years of age, who a month earlier had been tested collectively in the class-room. There emerge four types: objective, subjective, intelligent, and superficial, which represent a general attitude of the subject and correspond with stable personal characters. The authors raise the question whether these types remain constant from test to test; and after evaluation of their own results, and comparison of them with the results of Binet and Lelesz, decide to leave it open; we need a great many more clinical examinations, and the publication of monographic 'case reports' like those usual in medicine.] H. Flournoy. 'Çiva androgyne: contribution à l'étude psychanalytique des principaux symboles et attributs d'une divinité hindoue.' [A psychoanalytic study of cult-objects, symbols and attributes of Shiva leads to the conclusion that the god is in reality androgynous. The only alternative to this mode of interpretation is recourse to simple 'imagination.' The worship of Shiva combines the glorification of sexual enjoyment with that of the principle of life.] Recueil des faits: documents et discussions. J. A. Bierens de Haan. 'Sur les représentations des animaux.' [The results of Volkelt (1914) and other authors are insufficient to prove that the lower animals lack the perception of objects and can take account only of the total situation. Even the topographical memory of carrier-pigeons may be a character acquired during the evolution of the species and perhaps strengthened by practice.] G. Volait. 'XVIIe Réunion des philosophes de la suisse romande: Rolle, 11 juin 1922.' Bibliographie. Nécrologie. [A. Lemaitre, W. Baade.] Notes diverses.

Tome xviii., No. 4. J. Piaget. 'La pensée symbolique et la pensée de l'enfant.' [The thinking of children up to 7 or 8 years of age shows marked analogies, structural and functional, to the symbolic (prelogical, not paralogical) thinking of adults; it is characterised by lack of direction, amnesia, inability to frame definitions and to follow arguments, autism, confusion of self and not-self, self-centredness, preponderance of images (with condensation for generalisation and displacement for abstraction), overdeterminations and contradictions, failure to grasp causal relations or the relation of whole and part. Functionally, it is playful (Baldwin, Groos); structurally, it is simple, economical, restricted (Janet's 'weak tension'). The author leaves open all questions of genetic relationship.] S. Spielrein. 'Quelques analogies entre la pensée de l'enfant, celle de l'aphasique et la pensée subconsciente.' [The ideas of the child of 2½ years are associatively interconnected; both the ideas themselves and the associations are little

varied; ideas come to expression 'crumbled,' intermixed with bits of other ideas, and not clean-cut; thought in general is not singly directed and may be contradictory; it shows, accordingly, phenomena of perseveration and of dual determination ('crossing'); the train is kept going by emotive factors, interests. Analogies to the phenomena of perseveration, crossing and affective motivation may be found in subconscious thinking, in the language of children's drawings, and especially in the thought of the aphasic. All this evidence seems to point to the Jacksonian explanation of aphasia in terms of neural level rather than to the mere loss of verbal-motor images. It is possible, however, to bring the two theories into relation: subconscious thinking is kinaesthetic-visual, and the loss of connection between subconscious kinaesthetic images and conscious verbal thinking would mean reduction to a lower Jacksonian level.] G. Révész. 'Expériences sur la mémoire topographique et sur la découverte d'un système chez des enfants et des singes inférieurs.' [(1) A series of boxes, one containing chocolate, is laid before a child; when he has learned, in a number of trials, to go directly to the chocolate, the position of this box is changed. Beyond 4 years of age there is no evidence of associative inhibition; the second learning is easier than the first. Rhesus monkeys, who have passed the stage of mere mechanical memorising (fowls) but who do not understand the situation, are inadequate to the problem. (2) If the place of the chocolate is changed in the order 12345, children from 5 to 12 years of age master the system fairly easily; monkeys never grasp it, even if only two boxes are used. If the order is 15243, the problem is much more difficult, insoluble by 6-year-olds. The author analyses procedures in detail, and suggests that the two tests may (after a certain age) be employed to diagnose intelligence independently of age.] Recueil des faits: documents et discussions. E. Claparède. 'Formule commode pour le percentillage.' [If R is the rank corresponding with a given percentile, P this percentile, and n the number of individuals to be percentiled, then $R=1+[P\times(n-1)/100]$. This formula makes the percentiles 0 and 100 significant.] Bibliographie. Nécrologie. [E. Solari.] Notes diverses.

NOTES

Attribute and Sensation

In 1920-21 Dr. M. Yokoyama completed certain experimental series in a research that he had undertaken at my suggestion in the Psychological Laboratory of Clark University. At the end of that year Dr. Yokoyama returned to Japan, and since that time his duties at Keio University have prevented his bringing his results to publication. The general conclusion to be derived from his experiment was well understood before Dr. Yokoyama left the Clark Laboratory and was a matter of discussion among the laboratory group. Since the conclusions seem to me of particular interest, I believe that they should be reported without further delay in order that they may be available until Dr. Yokoyama's full account can appear. The quantitative data and protocols are not at hand, but I think there can be no doubt about the general findings. Dr. L. D. Boring and Dr. C. C. Pratt, who were with me *O*s in the experiment and were later acquainted with the results, have verified for me the statements of this Note. The responsibility for the Note is solely mine, though all credit for the experiment belongs to Dr. Yokoyama.

From the systematic position of psychologists like Wundt and Titchener the subject-matter of psychology is certain aspects of experience which stand up existentially under observation just as other aspects do in physics. Psychology and physics may be said to differ in their observational point of view. This distinction means that the fundamental materials of the two are identical though the systematic motives differ.[1]

In this setting there arises a problem of the nature of observational data: What is the simplest observational datum? Twenty years ago the answer to this question was tied up with the theory of mental elements. It was good psychology to say that the sensation was the observational ultimate and the attribute of the sensation the logical ultimate.[2]

This view of sensation, however, never quite fitted the facts. In experimental observations the *O* was always predetermined in favor of particular aspects of the sensory material. For a long time the fact of predetermination was taken more or less for granted, as something necessarily implicit in observation. In the Würzburg school, however, it became explicit in the *Aufgabe* and determining tendency. Since then the psychologist has to consider carefully instructions or other observational determinants very much as the physicist takes into account the constants of his instruments.

Such a change means that we cannot regard observation as the mere having of experience,[3] or psychological observation as more immediate than

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[1]E. Mach, *Beiträge zur Analyse der Empfindungen*, 1885, *etc.*, and R. Avenarius, *Kritik der reinen Erfahrung*, 1888-90, are the primary sources of this view, which has been elaborated into psychological systems by O. Külpe, *Grundriss der Psychologie*, 1893, and by E. B. Titchener, *Text-Book of Psychology*, 1910 (*cf.* also this Journal, 1912, 23, 427-448, 485-508), and is implicit in other writers. Wundt's position was different ("mediate" *vs.* "immediate" experience), but not in so far as his view comes into relation with the content of this Note.

[2]*Cf.* E. B. Talbot, *Philos. Rev.*, 1895, 4, 154-166; M. W. Calkins, *Psychol. Rev.*, 1899, 6, 506-514; M. F. Washburn, *Philos. Rev.*, 1902, 11, 445-462; *Psychol. Rev.*, 1903, 10, 416-422.

[3]*Cf.* Mach's quotation of Krause: "Problem: To carry out the self-inspection of the Ego. Solution: It is carried out immediately." *Op. cit.*, trans. 1914, 20.

physical. Whatever is in mind under any particular conditions has always to be reconstructed by scientific logic from many observations. Under the best conditions we observe one attribute at a time, and then, because we can find any attribute when observation is changed and directed upon it, conclude that they were all "there". Because of this necessary limitation of scientific observation at any one moment, the entire sensation seldom constitutes the immediate observational datum. We observe only aspects of it, primary attributes or attributes of higher orders. These are the observational elements (Külpe's "conscious actuals"). The sensation may then be variously described, according to the interest of the writer. It is the "psychic real" (Külpe) if the writer's interest is in reality; in what is really "there" though it can be gotten but piecemeal by observation. It is a physiological construct (Rahn) if the interest is physiological; in the constant excitation in relation to which observation is determined. It is a systematic term (Titchener) if the emphasis is placed upon descriptive method; upon the scientist's reality induced from his observations.[4] And, if anyone is concerned still about elements, it is as fair to say that the sensation is the mental element as it is to say that the attribute is the observational element.

The question now arises as to the degree of limitation that observation places upon a sensory experience. With the *Aufgabe* as a new tool we may very properly seek to measure its effect. In concrete form this question becomes: How much does the predetermination to observe one attribute of a sensory experience prevent the observation of other attributes? The extreme case is especially interesting. If an *O* is maximally determined for one attribute, can he observe any other attributes at all? This question gives the setting for Yokoyama's problem.

Historically, on the experimental side, the conditions of the psychophysical experiment give the best assurance for intensive observational determination. Judgments near the limen are difficult to make and spur the *O* to the most concentrated observation. The continuous repetition of presentation and judgment habituates the *O* in the single type of observation. The presentations should, moreover, be brief, partly because they thus render the judgments more difficult and so help to fix observation, and partly because they reduce the likelihood of change in the experience which makes observation equivocal, more complicated, and hence more difficult of fixation. For these reasons Yokoyama had his *O*s pass relative judgments upon two stimuli for the determination of a differential limen, and limited his presentations to considerably less than 100σ.

He used a dark exposure-box, similar to the Dodge tachistoscope, in which the *O*, fixating a tiny spot of light, was presented, after warning signals, with two rectangles of light, end to end. The rectangles could be varied relatively in brightness and in length.

In one series the *O* was predetermined to observe relative intensity. He was to pay attention to intensity only and to judge the one stimulus with respect to the other. The instruction was vigorous, the time was short, the sensory difference was small, and the task was continued for many observations. Altogether the *O* may be supposed to have been as definitely predisposed for intensity as he could have been. At irregular intervals in this series, however, and not oftener than once in fifty trials nor more than twice in a session, the *E* broke in, immediately after the exposure and often before the judgment of intensity could be voiced, with a secondary instruction: "How about extent?" The *O* was then expected to give *ex post facto* a judgment of relative extents.

In other series the primary determination was for extent, and intensity was called for at infrequent intervals.

[4]C. Rahn, *Psychol. Monog.*, 1913, No. 67: Titchener, this JOURNAL, 1915, 26, 258-267.

After a judgment the *O* wrote an introspective account of the consciousness.

We may in this experiment speak of the instruction for an attributive judgment given before the stimulus in all the trials as the 'primary determinant,' and the instruction occasionally interjected *ex post facto* after the stimulus as the 'secondary determininant.' In these terms the principal results are as follows.

(*a*) Under the primary determinant the judgment follows immediately, often without the mediation of any content other than the perception of the stimulus. This is the familiar consciousness which the psychophysical experiment often brings out.

(*b*) The judgment under the secondary determinant is usually delayed. The determinant, moreover, never acts retroactively directly upon the past perception to give an immediate judgment of the perception (except in the sense that the judgment refers logically to the perception, and that, being mediated by a surrogate which is in turn conditioned by the perception, it has an indirect functional relation to the perception). A surrogate of the perception is always present after the introduction of the secondary determinant. Sometimes the original perception is still persisting in visual imaginal terms of the order of the memory after-image when the secondary determinant is given. In such cases the judgment appears to the *O* to come quickly. When the experience has lapsed, however, it has to be reinstated as an imaginal surrogate (also usually visual for these *O*s), and then the judgment issues. These cases account for the delay. Usually an *O* accepts the surrogate as adequate; only occasionally does he become aware of a dilemma, and doubt his judgment because it is not based directly upon the process judged, or fail to make a judgment because it is impossible to 'decide' to make a judgment after the experiential process is past. I am not able to say without the protocols in hand whether there is any difference between the surrogate and the original perception.

(*c*) It is plain that the original perception under the primary determinant is inadequate to a judgment of any difference other than the attribute upon which the primary determinant is directed. If the capacity to make such a relative judgment is taken as the test of the presence or absence of an attribute, then it would be necessary to conclude that under a strong determinant one attribute might be present and the others missing. Thus the complete selectivity of a determination would be established. But the introspections of these *O*s show that, although they cannot make relative judgments upon the attributes for which they are not predetermined, they nevertheless can appeal to various aspects in the description of the perception. For instance, when determined for intensity, they make the judgment of intensity immediately and have a good deal to say about intensity in their descriptions of the perception; but they also frequently note something about extent,— 'lengths', 'widths,' and even the 'square shape' of the corners of the rectangles. Similarly when determined for extent, they note 'lengths' as the primary datum, but frequently mention the intensive pattern of the field. In a few protocols *O*s even insisted on the original existential presence of an attribute upon which they made no judgment or upon which they could not make a judgment without the intermediation of a surrogate.

From these results several conclusions can be derived.

(1) An instructional determination is always predetermination. A determinant cannot act retroactively.

(2) The apparent readiness with which a determinant seems to act *ex post facto* upon a perception is due to the intermediation of a subsequent surrogate for the perception. The surrogate may be a reinstatement or a persistence of the perception. It is not necessarily discrete from the perception, however; and in the case of persistence there is not even a tem-

poral gap. The adequacy of the surrogate under these relatively refined conditions of introspective observation argues that, for purposes of observation, the surrogate may be, and ordinarily is, identified with the perception. The total perception which underlies the judgmental report is a changing process, but the fact that it functions for observation as if stable means that a perception for the purposes of observation is a temporal 'mental object,' that is to say, a temporal series of events is integrated into an 'objective' whole in the sense that it functions for report as if it were a fixed datum.

(3) Moreover, it is interesting to note that Yokoyama did not succeed in reducing a perception observationally to a single attribute. To fail under given observational conditions in the reduction of the perception to a single term is not to prove that it cannot be done, and to find more than one attribute ready for descriptive report is not to prove that all the attributes were observationally available. We cannot conclude from these experiments anything more than that predetermination limits the observational range and that it is nevertheless very difficult to reduce it to complete simplicity.

(4) Finally, we may note that the test of the existential presence or absence of an attributive dimension does not lie in the capacity of the *O* to make relative judgments upon the attribute. Description is the finer instrument, and even it is subject to difficulties which arise out of the indeterminate nature of the 'mental object.'

As is so often the case when the experiment is over, it is easy to see that the conditions were not ideal. The determination was not entirely univocal; it was simply more nearly univocal than the determination has been in most psychophysical work upon which the theory of attributes has been based. One tends under these conditions to get space rather than pure extensity, and there is no agreement among psychologists as to the nature of intensity as a visual attribute. There were good reasons for taking two stimuli and making the judgments relative, but the experience was thereby complicated. It is not plain how these difficulties can be overcome. It seemed easiest to Yokoyama to use vision, and he could not use its quality as a variable with such short exposures on account of the *Anklingen*-times of the colors.

It is also natural to object that the occasional interjection of the secondary determinant spoiled the univocality of the primary determinant. I do not know how to combat this objection, but I believe it to be invalid. The evidence is the *Os*' reports. Moreover they were experienced *Os* (L. D. Boring, M. B. Pratt, C. C. Pratt, and myself); two other, less experienced *Os* were discarded on the basis of their reports. The person who makes this objection should first try making hundreds of difficult observations with a secondary instruction interjected not oftener than once in fifty times or twice in an hour, and with the knowledge, not only that he does not have to keep the secondary instruction in mind, but further that he must try his best not to keep it in mind. This person will find, I think, that the secondary instruction lapses entirely and that its interjection comes always as an unexpected shock.

E. G. B.

The Overlooking of Familiar Objects

In his recent study of the phenomena of assimilation, *i.e.*, of changes of color under the influence of form (*Zts. f. Psych.*, 92, 1923, 249 ff.), W. Fuchs suggests that our overlooking of things in everyday life, even when we may be gazing steadily at them, is due to the levelling out of their color, by as-

similation, to the color of their surroundings (307 f.). The explanation may hold in certain instances; it certainly does not hold in those with which I am most familiar.

I very often 'lose' things in the tangle on my desk. I can always find, without trouble, objects that look more or less alike from whatever side they are regarded (*e.g.*, round or oval boxes) and objects that 'naturally' fall into all sorts of positions (pencils, pens, letter-opener, paper-cutter); any the least exposed portion of these objects touches off cognition. I have great difficulty, on the other hand, with spectacles and desk-shears. I habitually lay these things down, after use, in a particular position; if, in the shuffle of papers and desk-furniture, they are brought into other positions, I do not see them; sometimes, though I know that the object is lying before me, I am obliged to ask the assistance of another pair of eyes.

My habit of laying down the glasses in a particular way began with a voluntary act; that of laying down the shears, so far as I can recall, grew up of itself under the conditions of usage. I have now, and have had for a good many years, definite schematic visual images of these two things in their 'proper' positions. I have also a definite motor set or attitude for picking up and replacing the glasses (both hands) and for taking up the shears (right hand). Over and over again, when I have been looking for the lost article, I have caught myself looking for it in terms of the visual image; I do not project the image on the desk, but rather carry it 'in my eyes;' and what I try to do is to fit the image over something that I see in the litter before me. When the image is clear, I do not notice the set (at least I have no record to that effect), though undoubtedly the set is there. When the image fails to appear (it may be kept under, *e.g.*, by other imagery due to visual preoccupation), I am able by shift to the observational attitude to remark the set. If, then, the image does not fit any visible object, or if there is nothing in the visual field that discharges the motor set into overt movement, then I fail to see what I am looking for.

Discovery may come about in various ways. Ordinarily, of course, I want the glasses or shears at the moment when I look for them, so that, after a discursive view of the desk-surface, I begin to move things about. Oftentimes, however, I have paused to observe image or attitude, and then have dismissed the image or relaxed the attitude, with the result that the object stares me recognisably in the face. Sometimes, when I am deeply absorbed, I sit helplessly, looking and looking without result; then I either give up the search, for the time being, or call in assistance.

There is, evidently, no need to invoke phenomena of color assimilation to explain these oversights; visual image and motor set are enough. I can, however, offer positive testimony against assimilation. It frequently happens, when the object is finally discovered in the 'wrong' position, that I have a flash of amused or irritated recognition, as if I should say "Oh, *you*, is it? I saw *you* before." I have never noticed, in this recognitive experience, any hint of color change. Moreover, when one considers the multiplicity of small, variously colored objects with which the desk is covered, it is difficult to see how assimilation could there come into play.

E. B. T.

Dr. Johnson on Reaction-Time Experiments

Dr. H. M. Johnson, in his summary of the studies of the past decade that have involved measurements of reaction-time[1], reviews two of my Studies with E. E. Cassel[2], and offers such criticisms of one of them—the

[1] *Psych. Bull.*, 20, 1923, 562-589.

[2] E. E. Cassel and K. M. Dallenbach, The Effect of Auditory Distraction upon the Sensory Reaction, this Journal, 29, 1918, 129-143; An Objective Measure of Attributive Clearness, *ibid.*, 204-207.

Study on "An Objective Measure of Clearness"—that a reply seems necessary. Dr. Johnson challenges our introspective findings, questions our data, charges us with a misapplication of method, and instructs us in the method of computing a correlation.

(1) The "introspective findings" which Dr. Johnson calls into question and the excerpt which he quotes as illustrative of them are, however, drawn, neither from the body of the experiment nor from the conclusions, but from a footnote[3] at the end of the article. Moreover, his omissions from the passage quoted are such that its implication is seriously altered.

(2) Dr. Johnson phrases his objection as follows: "The introspective findings might, of course, be challenged. Reaction certainly tends to become quicker with practice; and as in other instances of habit-formation, some reactors, including myself, have incidentally noted a tendency for the degree of consciousness of everything connected with the response to diminish, and almost to vanish, with continued practice."[4] Has he overlooked the fact that we were working not with a muscular but with a sensory reaction; or does he, as a behaviorist, deny the distinction? If the former, this correction meets his difficulty; if the latter, he takes his position in the face of experimental evidence.

(3) Dr. Johnson claims, further, that our "data do not support our reasoning;" wherein they fail he does not say. Except for this dogmatic statement, he does not again touch upon our results or upon our problem, but turns to an exposition of a method for obtaining, from our data, the correlation between the individual measurements of reaction and attributive clearness.[5] With this exposition we are in substantial agreement; but we regard it as irrelevant.

Our experiment fell into two parts. In the first part *O*, who was trained in the introspection of attributive clearness, gave, after every series of 10 reactions, an estimation, in terms of a scale of clearness-values which had been worked out in earlier studies,[6] of the clearness of the mental processes experienced during that period. The results showed that the average time of the simple sensory reactions and the degree of precision as expressed by the m.v. are both, under our conditions, inversely correlated with the introspectively by reported clearness-values. These results, however, were open (as we pointed out[7]) to the obvious objection that *O*'s reports, inasmuch as they had been delayed until after the completion of 10 reactions, were rather general impressions than true introspective descriptions, and that the figures, inasmuch as they were averages of averages, were artificial and not truly representative. *O* was therefore required during the last 700 experiments to report, immediately after every reaction, the clearness of the mental processes during the reaction. In the table in which these data are presented[8] we give (1) the different clearness values reported, (2) the frequency of their report, (3) the average reaction times for the different clearnesses, and (4) the mean variations of these averages. We computed the correlations, as we explicitly said, between clearness and average reaction time, *i.e.*, between (1) and (3), and between clearness and mean variation, *i.e.*, between (1) and (4). But Dr. Johnson objects, "Such treatment disregards the variations of time corresponding to each degree of clear-

[3]*Op. cit.*, 207, footnote 12.

[4]*Op. cit.*, 578.

[5]The attempt to measure attention in terms of the length of the sensory reaction was early proposed and early given up; *cf.* L. R. Geissler, this JOURNAL, 20, 1909, 496 ff. The coefficient that Dr. Johnson obtained from our data was small (-.25), and the negative evidence is thus further corroborated.

[6]K. M. Dallenbach, this JOURNAL, 24, 1913, 468; 27, 1916, 445.

[7]*Op. cit.*, 206. [8]*Op. cit.*, Table II, 206.

ness."[9] Surely: and if he had not been prepossessed by his own argument, he would have seen that it was just such variations that we wished to disregard,— or rather, to eliminate by averaging.

The length of the simple sensory reaction is determined by many conditions beside attributive clearness. Some of these, such as practice, habituation, and fatigue, were eliminated (so far as elimination is experimentally possible) by the conditions under which our work was done; others, such as attitude and attributive clearness, were introspectively noted and thus controlled; and still others, unknown and adventitious (Dr. Johnson will hardly maintain that the conditions named are the only ones that affect the reaction time), were beyond our control.

We based our procedure of averaging upon the assumption that the length of the reaction was a function of the chance distribution of attitude, clearness, and the unknown conditions. Of these conditions clearness alone was measured. We thought, by averaging the reaction times that were paired with the various degrees of clearness, to eliminate the effect of the other conditions and thus to discover the relations that existed between clearness and reaction. Since the correlation thus obtained was very high and the PEs were very low, since the results of the two parts of the experiment agreed, and since the relation persisted through marked shifts of attitude, we felt justified in concluding that under our conditions (we explicitly forbore generalization) "attributive clearness could be measured by the *average* duration and the *mean variation* of the simple sensory reaction."[10] Our procedure may be questioned; but we can not well be criticised for not "treating each paired measurement as a single datum";[11] we knew beforehand that that procedure would be futile.

(4) Dr. Johnson's review is not always accurate, either in quotation or in report. We have already mentioned a quotation the sense of which has been changed by omission. Again, in the last paragraph of his review[12], he draws a quotation from its context in the first part and applies it to the data of the second part. In yet another instance,[13] he credits us with a conclusion which nowhere appears in our paper.

Inaccuracy of report is shown by the following: "Two flashes of a Geissler tube serving as preparatory signals were given in succession 3″ and 1.8″ respectively, before the stimulus."[14] We used, not a Geissler tube, but a "2.9 volt electric globe" hooded "by four layers of black cloth." The preparatory signals occurred "3 sec. and . . . . . 1½ sec. before the occurrence of the stimulus." Dr. Johnson is also in error in his report of the occurrence and distribution of the control experiments among the distraction experiments.

K. M. D.

Core and Context in the Drowsy State

The experience of which I write occurred one morning in the early Spring of 1921. It had been my custom to go to bed about 11 o'clock at night, and to be waked in the morning by the light of the rising sun which flooded my room and by reflection fell upon my closed eyelids. My waking was always in concomitant variation with the rise of the sun. On winter mornings I would wake about 7 o'clock, at which time the first sunlight penetrated my eyelids. As the sun rose earlier, I woke earlier, until at the end of February I was waking between 6 and 6.30.

One morning early in March, between 3 and 4 o'clock as I later observed, I was still in the drowsy state when the following vivid dream occurred to me. 'I was awake and in bed. The room was dark, save for a huge tongue of flames which would intermittently leap from the open door of a large,

[9] *Op. cit.*, 570. [10] *Op. cit.*, 207. Italics are mine. [11] *Op. cit.*, 579.
[12] *Op. cit.*, 580. [13] *Op. cit.*, 578. [14] *Op. cit.*, 577.

red-hot, pot-bellied stove in the center of the floor and illumine the room. Fear was aroused in me less the flames might catch the bed-clothing'.

The fear mentioned at the end of the dream was the direct cause of my sudden waking to discover that the first electric storm of the spring was threatening. There was no thunder, but merely the intermittent flashes of lightning far off. The sky was dark. I observed the time to be 3.30.

It became at once apparent to me what, in a psychological sense, had occurred to me. The intermittent visual stimulation of my retina through my closed eyelids by the flashes of lightning had been necessarily vague, in fact, too vague to arouse centrally the entirely appropriate context. In place of such a context, this vague core of pure sense-perception aroused in my drowsy consciousness the context given above as my dream, in which the flames intermittently belched forth from a red-hot, pot-bellied stove. In reality there was no such stove in my bed-room.

The striking features of the context are as follows. (1) The context in my drowsy state was composed of two elements: ideas and feeling. (2) The ideas were visual concrete. (3) The feeling-element was complex in character, *i.e.*, it was the emotion of fear. The significance of fear in the context becomes intelligible either in the sense of my instinctive reaction to sudden stimuli or in the sense of my life-long emotional reaction to lightning as well as to fire. (4) Although the context was not entirely appropriate to the actual core of pure sense-perception, it clearly represented the attempt of the drowsy mind to arrive at the appropriate one.

West Virginia Collegiate Institute F. C. Sumner

A Blower for the Galton Whistle

In an experiment conducted in the Harvard Psychological Laboratory it is necessary to produce a continuous Galton whistle tone for several minutes. To meet this requirement a blower was devised which blows the whistle for about 25 min. at a constant air pressure of 100 mm. of water.

The blower is made of three five-gallon glass bottles, such as are used in the sale of spring water. The lowest bottle, hereinafter called No. 1, stands on the floor; the next, No. 2, is on a low table (21 in. high); No. 3 is on a table of ordinary height (32 in.). No. 2 is filled with water, which is siphoned into No. 1 through tubes (glass with some rubber sections). The incoming water forces air from No. 1 to the whistle. The glass tube leading into No. 1 should not extend to the bottom, as the rise of the water in the bottle on the floor would decrease the hydraulic head, which determines the pressure, if the head were to be measured to a changing level instead of to a fixed position at the neck of the bottle; hence the inlet should deliver the water only to the neck of the bottle, and, to insure silence, should divert the water to the side of the bottle instead of letting it drop straight down. The level of the water in No. 2 must not be allowed to fall, since any change in it also changes the effective head. To keep it constant, water is siphoned into No. 2 from No. 3. The level in No. 2 should be marked at the beginning of an experiment and kept as near as possible to that mark if constant pressure is desired. The tube from No. 3 to No. 2 should be somewhat larger than that from No. 2 to No. 1, and one of the rubber connectors should be fitted with a clamp, which is adjusted until the right amount of water is flowing into the middle bottle to keep the level at the mark. If the whistle is blown as long as 15 or 20 min. and one wishes to have the air pressure so nearly constant that no change in the height of the water in a manometer is perceptible, it may be necessary to adjust this clamp from No. 3 to No. 2 once or twice during that time. A little deviation, say, half an inch either above or below the mark on No. 2, does not produce any perceptible change in the height of the water column of the manometer. When No. 1 is full, No. 3 is empty; to blow the whistle for

another period it is necessary to interchange these two bottles. The bottles are, of course, equipped where necessary with rubber stoppers with holes to admit glass tubes, and at appropriate places there are sections of rubber tubing with clamps to regulate the flow of water or of air.

The advantages of this blower are its cheapness, silence, simplicity of construction, and the absolute constancy of the air pressure.

Harvard University FRANK A. PATTIE, JR.

A NEW PSYCHOLOGICAL SPECTRUM

The Cornell demonstrational laboratory possesses a psychological spectrum,[1] which Mr. L. A. Fuertes painted for Professor Titchener in 1903. The spectrum is painted in oils upon a canvas strip of approximately 4½ by 72½ in.; the strip is set in a black frame 14 by 84 in.; and it is faced with plate glass, in front of which are four flaps of black cardboard, hinged below, which can be turned down to show any or all of the four qualitative series. This spectrum has served its purpose very well; the one serious objection to it is that the four qualitative series, copied too closely from the Prang chart of the physical spectrum, are of uneven length.

We have lately been trying out, for demonstrational purposes, the colours of wall calcimine. These colours have a hard brilliance which, as compared with the soft depth of oil colours, seems almost luminous. A very effective psychological spectrum may be made as follows.

The spectrum is 5 in. high and 80 in. long; each one of the four series R-Y, Y-G, G-B, B-R is 20 in. in length. The colours are mixed by weight to give 40 units to a series, so that every unit covers half an inch of the full 20 in. laid off for the series. The 40 sensory steps, even where they are most evident, are only slightly above the limen.

For every series—we take the R-Y series as illustrative of all—three pairs of values must be found: (1) the amount of Y which, added to a known amount of R, will furnish a hue just perceptibly yellower than pure R; (2) the amount of R which, added to a known amount of Y, will furnish a hue just redder than pure Y; and (3) the proportion of R to Y which will give the sensory middle term between pure R and pure Y, *i.e.*, a hue which looks as much like R as like Y. These data, when the relative amounts have been converted into suitable absolute amounts, give three points on a curve of diminishing amounts of R and on a curve of increasing amounts of Y, as one passes from pure R to pure Y. More points may, of course, be determined, if one is not sure of the bisection of the qualitative line; we have found, however, that these three are enough. When the curves are plotted, the units of ordinates and abscissae should be chosen of such relative magnitudes that interpolation will be easy, since it is from these curves that the amounts of the colours are to be read. In our instance the axis of abscissae represents the sensory length of the R-Y series. If it is divided into steps just less than the DL, there will be many members to mix, and the mixing takes a great deal of time. If it is divided into steps just larger than the DL, there are fewer members to mix, but the spreading of the pigment and its blending under the brush are not easy, at any rate for an unskilled workman. Whichever plan one adopts, the chosen steps are laid off on the axis of abscissae, and ordinates are erected upon them. The curves cut the ordinates at the points representing the absolute amounts of R and Y needed for the corresponding sensory member. It is worth while to scrutinise the amounts read from the curves, and to make sure that they are sufficiently accurate, before mixtures are made according to them; thus, the values as read might be 35, 33, 32, 30, 29, etc., which should be corrected by inspection to read 35, 33, 31, 30, 29, etc.

[1]E. B. Titchener, Class Experiments and Demonstration Apparatus, this JOURNAL, 14, 1903, 178.

In the spectrum which we have already made the sensory borders of the colour units are not eliminated. In our lecture room, students even in the front row are far enough away to see the spectrum, thus prepared, as a smooth line of hues. We are now making another spectrum with the border lines eliminated, so that there shall be a uniform transition along the colour series even under the conditions of near observation. The blending of unit with unit is not easy to execute; but it is possible to spray the surface of the calcimine first laid, and so to keep it in the form of a paste which may be blended by the brush for the removal of the boundary lines.

Cornell University H. G. Bishop

The Annual Meeting of the American Psychological Association

The thirty-second annual meeting of the American Psychological Association was held at the University of Wisconsin, Dec. 27th to 29th 1923. There were about one hundred and twenty-five present at the banquet, which figures give an idea of the size of the attendance at the various sessions. Naturally the proportion of members from the Middle West was greater than when the Association meets on the Atlantic Coast; there were, however, a considerable number of members from the eastern colleges as well as from the Far West. As abstracts of the papers have already been published, it does not seem necessary to give a summary of the addresses in this place. In making this report I desire merely to point out some of the features which differentiate this meeting from the previous ones.

The programme was very well arranged in that the parallel sessions only partially overlapped. This plan enabled one, as a rule, to hear all the papers one was particularly interested in. Further, there were as a rule only four or five papers in each session, so that there was plenty of time for discussion, of which there seemed to me to be more than the usual amount. I think this was one of the most encouraging features of the meeting. Many of us have felt for some time that unless some means were found for stimulating more animated discussions, the formal side of the meeting could not be considered very successful.

There were a large number of papers on General Psychology, so that two sessions were devoted to this subject. Behaviorism still seems to arouse the greatest interest, if one can judge from the liveliness of the discussions. The *Gestalt*-theory made its first appearance on the programme as a tendency in psychological thinking. Without doubt, it will appear more frequently in the papers of subsequent meetings.

The experimental papers also occupied two sessions. The reports were very encouraging to laboratory psychologists, who hope that these sessions will grow in number and importance. There were, on the other hand, fewer papers than usual on mental measurement.

The three outstanding innovations in the programme were a session for Industrial Psychology, a session for papers presented by graduate students, and an informal conference of experimentalists. The industrial group, which was presided over by Professor Bingham, was chiefly concerned with vocational guidance and selection, and the study of personality. The session was considered very successful by those present and it was voted to arrange for a similar session next year.

According to the opinion of some of the members, who were present at the session for graduate students, this new plan of allowing the younger men to take an active part in the meeting proved to be an extremely good one. It was much more than a market for young instructors. The papers, for the most part, were very interesting and aroused animated and somewhat informal discussions. No doubt, the men gained valuable experience in defending their views, and the older men were able to obtain some idea of the general tendency among the younger generation of psychologists.

At the informal meeting of experimentalists about thirty-five members were present. Professor Raymond Dodge presided and almost all the large laboratories were represented. The general discussion was on experimental problems of *Gestalt*. There were also several brief reports from the various laboratories. The members considered the meeting very well worth while and voted to continue it next year. Professor Boring was elected chairman of the next meeting.

The symposium on the Contribution of Freudianism to Psychology did not seem to me to be very successful. I do not mean to reflect upon the addresses of the participants, but upon the general nature of symposia as they are at present conducted. I have sat through a number of them during the last year, and, so far as I could perceive, they might as well have been termed a special session devoted to papers on a particular topic. The main difference seems to me to be that the symposia are more general in nature and for that very reason, as a rule, less valuable than special sessions. For a symposium to have a right on the programme there should be a central theme which is supported against attacks from opponents. Such an arrangement requires considerable time and preparation; and it is to be hoped that when a symposium is again planned selections will be made and a pivotal paper commenced at least eight months before the meeting.

Professor L. M. Terman, in his presidential address, defended research in the field of mental testing as a legitimate part of experimental psychology. His paper was a pleasing combination of serious discussion and delightful witticism. In his clever defence of his position he showed much tact and dignity, and the audience must have been impressed by his sincerity and deep-rooted faith in the value of the work in which he is engaged.

Harvard University H. S. Langfeld

Toronto Meeting of the British Association

A meeting of the British Association for the Advancement of Science will be held at Toronto, from August 6th to 13th, 1924.

The Association has recently added to its various Sections a Section of Psychology. The Organising Committee of the Psychological Section believes that the meeting will afford an unique opportunity for intercourse between psychologists of Canada, the United States, and Great Britain; and accordingly issues an invitation to any psychologist in the United States or Canada, who may desire to do so, to join the Association as a member for the Meeting, and to contribute a paper or attend the discussions.

Professor McDougall, F.R.S., of Harvard University, has accepted appointment as President of the Section; and it is hoped that he will deliver the presidential address. Other papers have been promised by Dr. Morton Prince, Dr. Brett, Dr. Bott, Dr. Bridges, Dr. Sandiford, Dr. Tait, Dr. Myers, Dr. Burt, Dr. Drever, Mr. Flügel, Dr. Miles, Professor Pear, and other leading American and British psychologists.

Joint Meetings have been arranged with the following Sections:

Section H (Anthropology), to discuss "Racial Mental Differences."

Section I (Physiology), to discuss "Application of Physiology and Psychology to Industrial Efficiency."

Section L (Education), to discuss "Tests for Scholarships and Promotion."

Papers should not require more than 20-30 minutes to read; and intending contributors should send a notice of their papers before the end of March to the Recorder of the Psychological Section (British Association), The Psychological Laboratory, The University, Leeds, England. Further

particulars as to membership for the meeting can be had from the Secretary, British Association, Burlington House, Piccadilly, London, W.1, England; and notices of the general arrangements will be issued in due course by him and by the Hon. Local Secretary in Toronto.

LL. WYNN JONES, Recorder
CYRIL BURT, President (1923)
W. McDOUGALL, President (1924)

PUBLICATION IN GERMANY

Shortly after sending to the JOURNAL the Note concerning the serious curtailment of German publication, I received information from various men in Germany to the effect that the situation is not as serious as Professor Stern's words seemed to indicate. The representative of a Leipzig bookseller gave it as his 'unofficial' opinion that there is at present about a twenty percent. curtailment in the publication of books and periodicals.

Harvard University H. S. LANGFELD

INTERNATIONAL CONGRESS OF PHILOSOPHY

The fifth International Congress of Philosophy will be held in Naples, on the occasion of the seventh centenary of the foundation of the Royal University of Naples, May 5-9, 1924. Two anniversaries will be commemorated: the six hundred and fiftieth anniversary of the death of St. Thomas Aquinas (with Cardinal Mercier as official speaker) and the two hundredth anniversary of the birth of Kant (Professor A. Liebert). The Congress will be divided into ten sections: (1) metaphysics, logic, gnoseology; (2) aesthetics; (3) ethics; (4) history and philosophy of religion; (5) philosophy of law; (6) history and philosophy of science; (7) psychology; (8) pedagogy; (9) sociology; and (10) history of philosophy. The general secretary is Professor Guido della Valle, 259 Piazza Salvator Rosa, Naples 35.

ARBEITEN ZUR ENTWICKLUNGSPSYCHOLOGIE

Professor Krueger's series of *Arbeiten* seems to be firmly established as the successor of Wundt's *Psychologische Studien.* The numbers so far published are as follows:

(1) F. Krueger, Ueber Entwicklungspsychologie, ihre sachliche und geschichtliche Notwendigkeit, pp. x + 232.

(2) H. Volkelt, Ueber die Vorstellungen der Tiere, pp. iv. + 126.

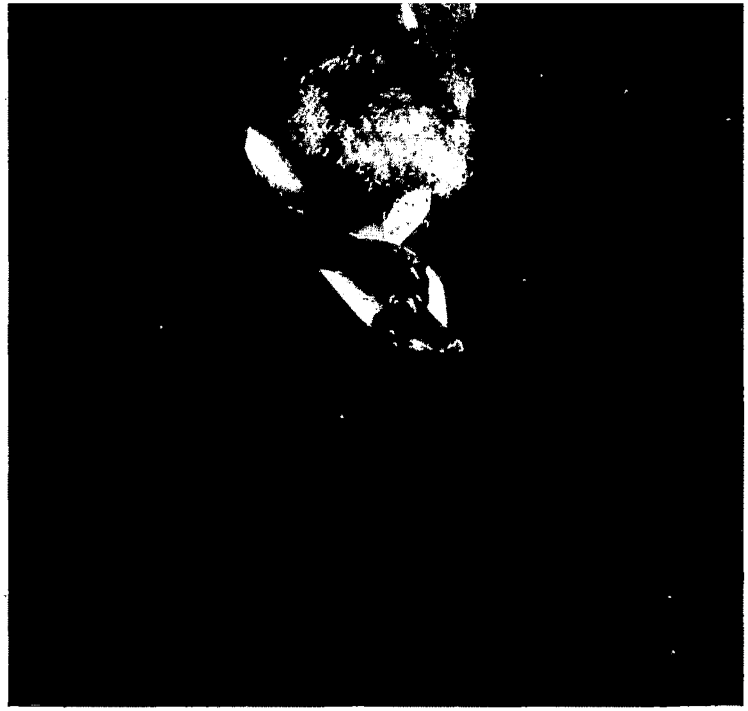
(3) H. Werner, Die Ursprünge der Metapher, pp. viii. + 238.

(4) B. Golz, Wandlungen literarischer Motive: i. Hebbels Agnes Bernauer; ii. Die Legenden von den Altvätern, pp. 94.

(5) H. Freyer, Die Bewertung der Wirtschaft im philosophischen Denken des 19. Jahrhunderts, pp. 174.

ARCHIV F. D. G. PSYCHOLOGIE

The *Archiv* passes, in its forty-sixth volume, from W. Engelmann to the Akademische Verlagsgesellschaft, Leipzig. The review-section will be resumed, and Professor Wirth hopes to publish four volumes yearly.



G. Stanley Hall.

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Granville Stanley Hall
1846-1924

Editor 1887-1895 Senior Editor 1896-1920 Cooperating Editor 1921-1924

Dr. G. Stanley Hall, the founder and for eight years the sole responsible editor of The American Journal of Psychology, died at his home in Worcester on April 24th in his seventy-ninth year. His death, in spite of his retirement from the presidency of Clark University four years ago, removes the most conspicuous figure in American psychology: witness his election last winter for a second time as President of the American Psychological Association.

The main events of his life may be briefly stated. He was born of old New England stock and spent his boyhood on a farm in western Massachusetts. His parents had secured something better than a common school education, and had both taught school. After fitting at Williston Seminary, he entered Williams College with an indefinite purpose of fitting himself for the ministry, and graduated in 1867 with Phi Beta Kappa rank and other honors. The next year he spent in Union Theological Seminary, finding himself, as time passed, in lessening sympathy with current orthodoxy and yet not seeing his way clear to any other calling. Most of the next three years (1868-1871) he spent in study in Germany, going over on borrowed money, secured through the friendship of Henry Ward Beecher. For a short time after the outbreak of the Franco-Prussian War he served also as a war-correspondent. His taste of university life in these years determined him definitely against the ministry

and in favor of an academic career; but on his return to the United States he found it impossible, on account of his heterodoxy and his supposed Teutonization, to secure a teaching position even in a state university, and was compelled to fall back once more on Union Seminary, where he took the B. D. in 1871. For a few weeks he was in charge of a mission church in Pennsylvania, but gladly gave it over to become a private tutor in the family of Jesse Seligman of New York. A year later came his first real academic opportunity, a chair of English Literature in Antioch College, Ohio. At Antioch he remained four years, exchanging his chair in English first for one in Modern Languages, and that later for one in Philosophy, doing incidental work also with the college choir, the library, student dramatics and the like, and finding time for a good bit of serious reading. In 1876 he resigned at Antioch with the intention of studying again in Germany; but before he sailed he was induced to accept a subordinate position in English at Harvard. The work, which was in elementary composition and rhetoric, proved a dreary task, but it gave him a chance to follow certain courses in philosophy, to do something in physiology with Bowditch at the Medical School, and to make the acquaintance of the younger men in philosophy at Harvard, especially that of James, whom he came to know intimately. At the end of his second year he took his Ph.D. at Harvard with a dissertation on the Muscular Perception of Space. With his degree behind him he now put into execution his plan for further study abroad, enrolling first at Berlin, but changing later to Leipzig where he was the first American student in Wundt's laboratory. In his graduate studying, both at home and abroad, Dr. Hall was least of all a student of the sessile and routine type; on the contrary he was like a boy at a banquet curious and eager to taste everything. While at Union Seminary he gladly made use of the opportunities of a great city, frequented the theater and opera, heard all the best orators, and engaged in mission-work of a sort to bring him in touch with poverty and crime. While in Germany he attended lectures all along the line from theology to anatomy and psychiatry. During his two visits there he took courses under, or came in touch with, Dorner, Trendelenburg, Pfleiderer, Lazarus, Leuckart, Du Bois-Reymond, Helmholtz, Ludwig,

His, Flechsig, Westphal, Wundt, Fechner, Paulsen, von Hartmann and many other leaders in different fields. With von Kries and Kronecker he published joint researches.

On his return in 1880 he began in Boston the studies later embodied in his paper on the Contents of Children's Minds on Entering School (published in 1883), and under the patronage of Harvard offered a short course of lectures on the Leading German Philosophers and Psychologists, and another course of Saturday lectures for teachers. The latter won him prompt recognition, led to a call for a similar course next year, and ultimately to an invitation to Johns Hopkins University, first as non-resident lecturer, then as permanent lecturer, and finally in 1884 as Professor of Psychology and Pedagogics. The next four years were spent as a brilliant member of the galaxy of brilliant men whom President Gilman had gathered about him as the first faculty of that new and significant institution.

In 1888 came an offer of the presidency of Clark University,—ominous in view of later developments, but seeming then to promise a fresh start with the highest ideals, vast resources and limitless freedom. Dr. Hall accepted, of course; and at the end of the academic year at Hopkins went abroad once more to learn the latest that the world had to teach on university administration. In the spring of 1889 he took up his residence in Worcester, and in the fall of the same year the first students were received. Of the history of Clark University, of the disappointment and thwarted hopes which marked its earlier years, and of the victory which Dr. Hall won out of defeat, this is not the place to speak. Suffice it to say that in the thirty-one years of his presidency he succeeded in realizing in essence, though in much restricted scope, the high purposes with which he began and in creating a place of research upon which many workers look back as to a beatific vision.

Dr. Hall married twice. In 1878 he married, in Berlin, Miss Cornelia Fisher, an acquaintance of his Antioch days, and of this marriage two children were born: a son, Robert G. Hall, now a physician in Portland, Oregon, and a daughter, who with her mother met a tragic death in 1891 by accidental gas-poison-

ing. In 1900 he married, as his second wife, Miss Florence E. Smith of Newton Centre, Mass., who survives him. Of this marriage there were no children.[1]

Dr. Hall's published work consists of some fourteen volumes (by far the greater portion of them, both in number and importance, published in the last twenty years of his life) and of more than 350 shorter papers, articles and addresses, nearly half of which belong to the same period. This relatively late productivity is doubtless due in part to the hindrances which prevented Dr. Hall's securing a settled and congenial academic position until late in his thirties, and to the administrative harassments of the earlier years at Clark. That he was able to ripen a harvest after the age at which most men begin to feel the burden of years, and to produce, as he did with almost unabated vigor, until the beginning of his last brief illness, was due to his great physical vitality and to his perennial youthfulness of spirit. A second remarkable characteristic of Dr. Hall's work is the relative absence from it of anything like systematization. His own thought was too fresh, too full of nascent possibilities, to encourage or permit him to lay it out in regularly concatenated sequences. For the same reason he rarely had the patience to disentangle complicated matters of theory and to assign to each conflicting factor its precise or probable value. His natural bent was creative rather than critical. This, of course, did not prevent his dissenting from the views of others and, upon occasion, giving vigorous expression to that dissent. He attacked, indeed, at times with vehemence the positions and tendencies of which he disapproved, particularly when they bore upon the teaching of young people; but of judicial decisions, with reasons attached, there is small trace in all his published writings.

His gradual shift away from the laboratory toward "genetic psychology" and the larger social and religious questions is well indicated by the bibliography of his contributions to the JOURNAL which follows this notice. It should be recalled also

[1]Fuller accounts of Dr. Hall's life, together with bibliographies, complete to their dates of issue, will be found in L. N. Wilson's *G. Stanley Hall* (New York, Stechert, 1914), and in Dr. Hall's own *Life and Confessions of a Psychologist* (New York, Appleton, 1923). Wilson's book contains some details of personal history not taken up in the autobiography.

that after his founding of the *Pedagogical Seminary* in 1891 much of his work on the educational aspects of psychology appeared in that journal, and that after the beginning of the *Journal of Religious Psychology and Education* in 1904 a portion of his work in that field was turned in its direction.

The death of Dr. Hall marks not only the end of his own distinguished career, but also the final close of the pioneer period in American psychology. Of the three outstanding figures of the first few years, Ladd, James and Hall, none remains. The workers, the problems, the methods have all changed. Of the three, each served the new movement in a unique and indispensable way. Ladd, by producing the first authoritative and reasonably complete text-book of the new science in English, made it accessible to a rising body of students who could not readily make use of works in German. James, by his studies of the feeling of effort, of emotion, and of vision, by his attractive literary style, and finally by his two-volume Principles of Psychology, advanced the theory of the new science, gave it standing in academic and literary circles and won for it friends and supporters. Hall, by opening the Hopkins laboratory, by championing the genetic point of view, by enthusiastic teaching, both at Hopkins and at Clark, by founding the American Journal, and by organizing the American Psychological Association, multiplied workers, made publication easy, and gave to those busy with psychology a corporate unity. There is honor enough for all; no history of psychology in America can ever be written without honorable mention of all three; but it seems that in the end the greatest total service, measured by the extent to which the science has been helped forward, will be found to have been rendered by the last to go. After completing the Principles and the smaller books which were in a sense by-products of that work James promptly, and with a certain satisfaction, turned away from psychology to other and more congenial fields. Ladd, who had all along been two-thirds philosopher, published nothing of psychological importance after 1894 except the revision of his Elements of Physiological Psychology, and in that he was assisted by Woodworth. Hall alone maintained a creative interest in the science and its applications until the end.

Of the three it is fair to say that Ladd, at least in psychology, showed no more than high talent intelligently and industriously applied, while James and Hall both had, in differing fashion, the unmistakable marks of genius. Of genius in science Ostwald has distinguished two types: the Romantic and the Classical. In science all geniuses work hard, but the *Romantiker* works also swiftly. His mental machinery is labile; his ideas bud forth in all directions; his interests are wide; he reads everything; he talks readily and interestingly; his enthusiasm is contagious. As a consequence he draws about him, without effort, eager collaborators, and becomes, almost in spite of himself, an ideal director of advanced students. Problems, in which he himself is keenly interested, are ready in abundance for their study; he gives to them lavishly of his time and attention; he rejoices in their success, and they, in turn, work as never before, accomplish results seemingly beyond their powers, and make return in affectionate admiration. But the need of communication is intense in the *Romantiker;* the university lecture-room and seminar rarely suffice for it; he reaches out for a larger audience through publication. When he writes, he works under high pressure; he is impatient to get this piece of writing out of the way in order to come at the next; he is careless of details and even of minor errors; to correct them would be to delay other things of greater import. Is it not enough that he has furnished a new insight and opened up new territory? Let after-comers make their own corrections! With the material coming thus hot from his heart and brain, it is inevitable that his work should bear a strongly personal character; no one else could have done it just as he has; and if it is good work of its kind, it affects powerfully his own day and generaiton.

The qualities of the *Klassiker* contrast with these at nearly every point. His mental machinery functions more slowly, more ponderously and with a greater concertration. His flow of fresh ideas and new problems is but moderate, and all are closely germane to the central topic of his interest. He feels less need of communication and much more need for treating problems systematically, exhaustively and for all time. Teaching, if it falls to him, he will carry through conscientiously; but he takes no especial pleasure in it; he kindles small enthusiasm

and attaches few students by personal ties. When he writes he composes methodically, publishes sparingly and often with hesitation; completeness is hard to attain, and no error is minor. As the quality of his work approaches perfection, as it gains in universality and becomes independent of time and place, it loses its personal flavor and takes on something of the impersonality of mathematical demonstration. The work of both *Klassiker* and *Romantiker*, if it has been worthy, ends by disappearing in the general and nameless current of science; but the work of the *Klassiker*, paradoxically, just because it is less personal, bears up his name for a relatively longer time upon the stream.

To workers of either type the methods and outlooks of the other are largely incomprehensible. To the *Klassiker* the *Romantiker* is anathema: he is hasty, he is careless, his methods are unsound, he is in palpable error. To the *Romantiker* the *Klassiker* is a pedant, a tither of mint and anise and cummin, a stickler for orthodoxy, a devotee of meaningless decimals, caring more for system than for science. To a less passionate judgment both sorts of workers and each sort of work, in its particular time and place, are essential: that of the *Romantiker* when new fields are to be cleared or old ones ploughed up anew; that of the *Klassiker* in fields already reduced to normal tillage. A *Klassiker* in sociology is as rare as a *Romantiker* in mathematics.

Dr. Hall was a typical *Romantiker*; and he found in the nascent psychology of forty years ago and in the applications of psychology to education (a technology which is ever in need of reconstruction) matters to which he could devote himself with full abandon. The speed at which he worked, especially the amount of scientific literature which he covered—often by the slow help of a reader—was a marvel to all who knew him. The range of his interest, if not quite universal, included at least all of those branches of knowledge which deal with life, even in its sordid and repellent aspects, and of him it can be more truly said than of most that to him nothing human was alien. His teachir ;, his public lecturing, the variety of new problems which he cr dered, his output of papers, his carelessness of details an' ıor errors, which even the care of his devoted assistants

could not wholly eliminate, the unmistakable character of his writing (the "Hall mark" so easily recognized), the extent of his early influence upon the development of psychology in America and throughout his career upon education,—one and all run true to type. It is as a pioneer and propagandist in these fields that he must be judged. The attempt to appraise him by other standards misses the essential meaning and purpose of his life.

It is also likely that his scientific contributions will suffer the typical fate of the work of the *Romantiker*. In psychology the specific things with which his name has been associated (his early work in the psychological laboratory, his studies of children and adolescents, the "recapitulation theory" in education, the progress in muscular control "from fundamental to accessory", the emphasis on sex in normal life and education), in all these his first reconnoissances have been revised or superseded by the more accurate surveys of later comers, or seem likely to be so superseded, a result which Dr. Hall, again like a true *Romantiker*, would have looked forward to with entire equanimity. But the larger principles which underlay his work—none of them indeed original with him nor exclusively his, yet all held enthusiastically by him and greatly advanced by his support—the belief in research, the genetic point of view in psychology, the psychological point of view in education and religion, the importance of studying children and young people as the key to teaching them, the passionate reverence for youth, health, freedom, and self-realization—his contribution to the effective recognition of all these and to their progressive realization in action nothing can ever lessen or gainsay; it has already passed into the vital current of the time. It was these fundamental trends, so characteristic of Dr. Hall, that, in comparison with the less certain values of his specific suggestions, gave to some who knew him the constant feeling that the man was at all points greater than what he had published.

To preach and to practice such principles as these is the work of a great teacher; and it is as a great teacher—not merely of graduate students or of the general public—but as the inspired seer and prophet of a genuine psychological and educational gospel that his place and reputation are secure. To those

who were his pupils, the inspiration, the illumination, the friendliness are unforgettable. While these pupils live his memory will live also. When they are gone, and those also who found inspiration in his more publicly spoken word, his books may lose in power and the lustre of his name grow dim, but none the less his nameless influence will still be making powerfully everywhere for the advantage of the things which he most loved: youth, freedom, new knowledge.

E. C. S.

Articles contributed by Dr. G. Stanley Hall to this JOURNAL

1887. Dermal Sensitiveness to Gradual Pressure Changes (with Yujero Motora), I, 72-98.
Psychical Research (a review of the work of the English Society for Psychical Research, 1882-87, and of Gurney's *Phantasms of the Living*), I, 128-146.
Reviews of the Psychologies of McCosh, Bowne, Dewey and Ladd, I, 146-164.
1890. Children's Lies, III, 59-70.
A Sketch of the History of Reflex Action, III, 71-86.
1891. Review of James' Principles of Psychology, III, 578-591.
Contemporary Psychologists: E. Zeller, IV, 156-175.
(First issue of the *Pedagogical Seminary*, 1891)
1895. Experimental Psychology in America, VII, 3-8.
Psychic Research, VII, 135-142.
1897. A Study of Fears, VIII, 147-249.
The Psychology of Tickling, Laughing and the Comic (with Arthur Allin), IX, 1-41.
1898. Some Aspects of the Early Sense of Self, IX, 351-395.
1899. A Study of Anger, X, 516-591.
1900. Pity (with F. H. Saunders), XI, 534-591.
1903. Reactions to Light and Darkness (with Theodate L. Smith), XIV, 21-83.
Notes on Moon Fancies, XIV, 88-91.
Child Study at Clark University, XIV, 96-106.
(First Issue of the *Journal of Religious Psychology and Education*, 1904)
1908. A Glance at the Phyletic Background of Genetic Psychology, XIX, 149-212.
1912. Why Kant is Passing, XXIII, 370-426.
1914. A Synthetic Genetic Study of Fear, XXV, 149-200; 321-392.
1915. The Freudian Methods applied to Anger, XXVI, 438-443.
Thanatophobia and Immortality, XXVI, 550-613.
1917. A Reminiscence (for 25th meeting of Amer. Psychol. Assoc., Dec., 1916), XXVIII, 297-300.
1918. A Medium in the Bud, XXIX, 144-158.
1919. Some Relations between the War and Psychology, XXX, 211-223.
1921. The American Journal of Psychology, XXXII, 1-3.

For many years Dr. Hall was accustomed to furnish to the JOURNAL, under the title of Book Notes, groups of short unsigned reviews, sometimes quite numerous, and covering books upon all the topics in which he was interested. In the last three volumes these were continued, under his name, but were confined to books on psychoanalysis and related matters.

ON THE THEORY OF ERRORS OF OBSERVATION

By F. M. Urban

Physical measurement is never exact, owing to errors some of which are systematic and some accidental. Systematic errors must be avoided or, should this not be possible, eliminated by appropriately arranging the course of observations. Accidental errors are unavoidable. Results of repeated determinations of the same quantity do not agree. This disagreement amounts to a contradiction, for it is clearly meaningless to assign different values to a well-defined quantity. These contradictions must be removed and the data must be made consistent by eliminating the accidental errors. This process is called adjustment of observations.

The method of least squares is most frequently used for this purpose. It was applied first in astronomy and geodesy. Here measurement soon reached such a degree of precision as to warrant adjustment. Today this method is used wherever exact measurement is aimed at. It rarely happens that other methods of adjustment are used. When speaking of the theory of errors of observation one usually means the method of least squares. These terms are, however, not identical. There exist other processes of adjustment, *e. g.*, graphic methods, but they are rarely used. The method of least squares has such a dominating position in practical work as to make all other processes of adjustment appear unimportant. The situation is still more pronounced in regard to theory.

The immediate purpose of adjustment is to remove the contradictions between the results. To do this a satisfactory process of adjustment must fulfill certain requirements. The first is that the process must be well defined, *i. e.*, that it must lead by a series of well-defined steps to one uniquely determined value of every unknown quantity. The second requisite is that the process must be general, *i. e.*, that it must be applicable to any set of measurements, no matter what numerical values may have been obtained by measurement.

These requisites are indispensable, but they are not sufficient actually to determine the process of adjustment. There exist an indefinite number of processes satisfying these two conditions, and we have nothing to go upon in selecting the one to be used in actual work.

Legendre tried to justify his particular choice of the method of least squares by claiming that "there exists no principle more

general or more exact, nor one of easier application than the rule to adjust the data of observation so as to make the sum of the squares of the deviations as small as possible." This view is untenable. Simplicity of computation is certainly very desirable, but it is not a cogent reason for selecting a method of scientific research. The amount of work is of no importance here. We gladly accept any method, no matter how laborious it may be, provided that it gives the correct result. Tait illustrates this point by the example of a scientific man who would put attraction inversely proportional to distance, and support this assumption by the argument that it gives a simple and easy solution of the problem of three bodies. Such a procedure is sure to be laughed out of court. Even for practical work simplicity of computation is of minor importance. The main point is to obtain the correct solution. Should the work require too much labour, shortcuts of sufficient accuracy may be devised, provided that the value one is seeking is well defined.

Legendre's claim that there exists no method more general or more exact is entirely gratuitous. Any well defined procedure leading to a uniquely determined value of the unknown quantities is equally general. The question, how the exactness of two processes is to be compared, can not even be raised before the definition of exactness is agreed upon.

The practical need of some method of adjustment is quite plain. There exist an indefinite number of possible methods, and our trouble begins when we have to select one of them. Our choice, not to be arbitrary, must be supported by argument. It is one thing to deduce a set of formulae from given suppositions and it is an entirely different thing to justify one's choice of the particular principles from which the conclusions are drawn. There is nothing wrong with the deduction of the method of least squares and its practical success must impress anybody interested in exact science. The trouble is with the principles on which it is based. Why should we follow the rule to adjust the results so as to make the sum of the squares of the deviations a minimum, while there are an indefinite number of equally plausible hypotheses at hand?

Gauss' first deduction of the method of least squares is based on two suppositions, which are called the principle of the arithmetic mean and the principle of the equiprobability of positive and negative errors. The first implies that the average of a set of direct measurements is the most probable value of the observed quantity. The second means that positive and negative errors of given size have the same probabilities. A series of strict conclusions shows that the method of least squares gives the most probable values of the unknown quantities in the case of indirect observations too. The principle of the

arithmetic mean is supported by the fact that it is customary in practical work to take the mean as the best value. This implies that the mean is regarded as superior to any other value, *i. e.*, that it is more likely to be correct than any other determination. This, however, is but another way of saying that the mean is the most probable value of the unknown quantity.

This argument may be expressed shortly in this way. The method of least squares is derived as the process of calculation corresponding to the customary procedure in taking measurements. Practical work is the primary datum, and calculation is arranged to suit it. The rules followed in practical work are analysed as to their logical significance, and are used as foundations of the mathematical theory. Practical work is justified by its success, and theory is based on it,—or rather, theory is nothing but practical work refined.

It must be granted that this is an excellent justification and one that cuts deep to the very foundations of science. The primary datum of science is the collection of data. To be consistent, science must accept as principles the rules followed in collecting the data, or reject them both. Science may generalise or refine the methods of practical work, but the rules implied in it must be followed in theoretical research too.

At first sight the principle of the arithmetic mean impresses one as a very natural supposition. It seems that most investigators hoped in the beginning of their studies to lay the foundations of the theory of errors of observation by proving this proposition from general principles. The actual attempt only proved the difficulty of the task. As long as one has not given much thought to it, this proposition appears clear and not open to doubt. It is after one has recognised its implications that one feels the necessity of elucidating this principle or of finding a more secure basis for the theory of errors of observation. There exists, in this respect, a certain similarity between the principle of the arithmetic mean and the parallel axiom of geometry. Both are borne out by experience, and impress the beginner as perfectly straightforward. Only the serious student realises the necessity of further investigation. The difference is that nobody doubts that there exist an indefinite number of possible methods of adjustment, while the parallel axiom was, and is, regarded by many as an intuitive necessity.

Some recent developments of the calculus of probabilities make it all the more important to elucidate this problem.[1] It has been shown that all the problems of the method of least squares may be stated and solved in connection with the theorem of Bernoulli. Certain problems relating to chance events exhibit

[1]Cf. my book *Die Grundlagen der Wahrscheinlichkeitsrechnung und der Theorie der Beobachtungsfehler*, 1923, 223-238.

all the features of those treated in the theory of errors of observation. The quantities to be determined are the unknown probabilities of certain events, and the observed relative frequencies are the empirical data. The solution of these problems is given by the method of least squares, which is derived as a direct consequence of Bernoulli's theorem. In this case there is nothing arbitrary about the proposition that the arithmetic mean of direct observations is the most probable value of the unknown quantity. The solution of these problems by the method of least squares is as necessary a consequence of first principles as any deduction in mathematics, and there is no way of contradicting it. One has no choice but to adjust the data by the method of least squares, and there exist definite and cogent reasons for using it.

Where is the corresponding justification of the principle of the arithmetic mean in the theory of errors of observation? Here this proposition is postulated as a principle without advancing abstract reasons in its support. There is a difference between proving a proposition from abstract mathematical principles and justifying it by its success in practice. Gauss felt dissatisfied with his deduction in the *Theoria Motus*, and tried to find a better starting point. His object was to find a set of general principles from which a method of adjustment might be derived. It may be that his research was restricted to sets from which the principle of the arithmetic mean for direct observations would follow. His argument is typical of all the later demonstrations, although it surpasses them all by its precision and elegance. For this reason we will briefly review it.

Measurement is regarded as a kind of game in which one can but lose. Be an error positive or negative, it is a deviation from truth. It is necessary to find some means to estimate the loss one is likely to suffer in this game. Gauss chooses the mean error, and defines the most advantageous determination as the one which makes the mean error as small as possible. These suppositions make the problem definite and lead to the method of least squares. The arbitrariness lies in the definition of the mean error, and Gauss was fully aware of it. Any function of the errors which remains always positive may be taken as representing the mean error, and Gauss' definition has only the advantage of being the simplest possible assumption.

Few problems of science are as distinguished by contributions of the keenest investigators as the theory of errors of observation. We now know that the method of least squares may be derived from very different suppositions. Perhaps the best known is the deduction from the hypothesis of elementary errors. The favor which this demonstration has found may be

due to the fact that the hypothesis of elementary errors corresponds to our ideas about the origin of accidental errors. Symmetry of distribution, however, is obtained only by specialising this assumption, and to obtain the method of least squares one has to make assumptions corresponding to those in Gauss' first demonstration.

There is one weak point in all these demonstrations. They contain among their fundamental suppositions one or more propositions which can not be deduced from general principles. It is not exactly the principle of the arithmetic mean which is needed, but some proposition corresponding to it. Since all these sets of suppositions lead to the same result, namely, to the method of least squares, one must conclude that these different principles are logically equivalent. The theory of errors of observation requires one or more propositions which can not be deduced from general principles and which, therefore, are of non-mathematical character. We may guess that these propositions must refer to the general character of our methods of measurement.

It is easy to see that the idea of precision of measurement is foreign to the principles of abstract mathematics. Mathematics deals with abstract quantities, precisely defined and absolutely exact. It does not consider the process of measurement by which we assign numerical values to empirical quantities. Precision can not be measured unless it is agreed upon how this is to be done.

Precision of measurement means limited precision. This implies that the observed quantity may be increased or diminished in its value, without this change being detected by the instruments used. The theory of errors of observation does not use this simple notion, which is closely connected with the idea of accuracy of sense-perception, but defines precision of measurement indirectly in a rather artificial way. The better our instruments of measurement are, the better do the results of repeated measurements of the same quantity agree among themselves and the more closely are they clustered around the value which is to be assigned to the unknown quantity. Precision, therefore, is defined indirectly by the greater or smaller agreement between the results. Theory has no means to determine the precision of a single observation directly. This can be done indirectly only by comparing the results of repeated measurements.

It is customary to define precision by the deviations from the mean. It is, however, not necessary to take the deviations from the mean or, as a matter of fact, from any other value. Andrae and Helmert have shown that precision may be defined just as well by starting from the discrepancies among the observations.

This proves that the customary definition of precision of measurement is based on the agreement between the results of repeated measurements.

It requires a little more care to see that the method of measurement defines the value which is to be assigned to the unknown quantity. The method depends on the instruments and on the character of the quantity to be measured. The final value is never obtained directly, but is the result of a more or less complicated, systematic procedure. The first thing in actual measurement is to define the conditions under which one intends to observe. One then compares the unknown quantity with a series of known quantities, until one is found which is judged equal. The application of a method of measurement requires a rule for selecting the values which must be compared to the unknown quantities. The rule for selecting these comparison values implies a definition of the value which we assign to the unknown quantity.

It is important to show that the final value of an individual measurement is always the result of a compound process. In some cases the individual comparisons take place so rapidly that one might easily be misled into believing that one had found the final value directly. As a matter of fact every process of measurement consists of a series of comparisons, and the value of the unknown quantity is found after establishing its inequality with a series of other values.

The compound nature of this process is easily recognised in the case of transit observations. The event in question consists in the coincidence of the star with the meridian. The observer follows the course of the star and marks, *e. g.*, by releasing a key, the moment when the star coincides with the thread. The fact that the observer does not release the key is equivalent to the statement that in this moment the coincidence of the star with the thread has not yet taken place.

It is particularly easy to recognise the compound nature of the process of measurement in the case of weighing. The body of unknown weight is successively compared with known weights, until a weight is found which the balance indicates as equal to that of the body of unknown weight.

There is one more point to be settled. Empirical measurement never gives only one exactly determined numerical value; but, owing to the limited accuracy of our instruments and of our sense-perception, all the values of a certain interval appear equal to the unknown quantity. The size of this interval depends on the quality of the instruments used, but it never can be reduced to one single point. Results of measurements are

determined within a certain interval only, the size of which depends on the accuracy of our sense-organs and on the quality of our instruments of measurement.

The notion of the margin inside of which we can determine empirical quantities is very well known since Felix Klein. It may be illustrated by the following example. We have at our disposal a balance sensitive to centigrams and a set of weights graded in milligrams. In determining the weight of a body we may change the comparison weights within the limits given by the accuracy of the balance. In so far as our observations go, any point of this interval may be chosen as the weight of the body. The interval is reduced in size if a better balance is used, but, owing to the limited accuracy of the instrument, it never can vanish. Any value inside the margin might be assigned to the quantity measured, to which it is, as far as our perception goes, equal.

The existence of the margin makes it necessary to define the point of this interval which must be taken as the value of the unknown quantity. It is customary to take the centre of this interval, *i. e.*, the mean of the upper and the lower limit. These limits are found by starting from equality and increasing the known quantity until the difference is perceived. One also may start from values which are clearly greater or smaller than the unknown quantity and reduce the difference by small steps, until the quantities seem to be equal.

Every individual result is the mean of the upper and the lower limit of the margin. This definition serves the practical purpose of making the individual results numerically definite. Here again we have to raise the question whether this value defined as the mean of the lower and the upper limit of the interval possesses any distinguishing qualities. It will be seen that this question is closely connected with the definition of the point of subjective equality in psychophysics.

The process by which we determine the upper and the lower limit of the margin is identical with the one followed in the method of just perceptible differences. From this it follows that the margin is identical with the interval of uncertainty. Its size is given by the difference of the quantities S_2 and S_1 known in the theory of the method of just perceptible differences and of the psychometric functions. In this way the notion of the margin in the determination of physical quantities is made definite. It was a great step forward when Felix Klein recognised the existence of the margin. The ideal objects, of which pure mathematics treats, never correspond exactly to their empirical representatives, which we perceive by our senses. The margin in the determination of physical quantities is due to the limited

accuracy of our sense-perceptions. This word must be taken in its technical meaning, and includes perceptions in which the senses are aided by instruments.

We will now consider the significance of the rule to take the centre of the interval of uncertainty as the final determination of the unknown quantity. In psychophysics the point of subjective equality is defined as the comparison stimulus for which the probabilities of the judgments "greater" and "less" are equal. This is the abscissa of the point of intersection of the curves representing the psychometric functions of the judgments "greater" and "less". One sees at once that this point must always be within the interval of uncertainty, since outside of it the probabilities of the extreme judgments are greater than one-half. It has been found that the point of intersection of the curves representing the psychometric functions of the extreme judgments very nearly coincides with the centre of the interval of uncertainty. This means that the average of the upper and the lower limit of the interval of uncertainty is, or very nearly is, the value for which the probabilities of the judgments "greater" and "less" are equal.

The theory of empirical measurement defines for its purpose the value assigned to a quantity determined by measurement in the same way as psychophysics defines the point of subjective equality. Two quantities A and B are equal if the chances are equal that A or B will be judged greater. This coincidence is, after all, no wonder. Psychophysics deals with the accuracy of sense-perceptions, and these are the primary data of empirical measurement. It is rather surprising that this connection between psychophysics and the theory of errors of observation was not discovered before. Had Felix Klein, or one of his many followers, tried to make the notion of a margin definite, he would have been compelled to measure the accuracy of sense-perception, and this would have led to the analysis of psychophysical methods.

The necessity of defining the conditions, under which two quantities must be regarded as equal, exists as well in the theory of errors of observation as in psychophysics. The coincidence of the point of subjective equality with the centre of the interval of uncertainty is so remarkable a fact that it is worth while to state the general conditions under which this coincidence must take place. Let Ψ be a function which constantly increases from the value $-\frac{1}{2}$ to $\frac{1}{2}$. Ψ approaches the values $-\frac{1}{2}$ and $\frac{1}{2}$ asymptotically. We also suppose that the first derivative of this function has a maximum at the point $x-S$, on both sides of which it decreases symmetrically. The expressions

$$f(x)=\tfrac{1}{2}+\Psi[h_1(x-S_1)]$$
$$g(x)=\tfrac{1}{2}+\Psi[h_2(x-S_2)]$$

may represent the psychometric functions of the judgments "less" and "greater", since they always remain positive and constantly decrease, or increase respectively, between the values 0 and 1. The curves representing these functions consist of two parts which coincide after double mirroring at the straight lines $y=\frac{1}{2}$ and $x=S$. The points $(x=S_1,\ y=\frac{1}{2})$ and $(x=S_2,\ y=\frac{1}{2})$ are points of inflection.

The point of subjective equality is given by

$$\xi=\frac{h_1S_1+h_2S_2}{h_1+h_2}.$$

If h_1 and h_2 are equal, or very nearly equal, the point of subjective equality must coincide, or very nearly coincide with the centre of the interval of uncertainty.

It may be mentioned that both suppositions are suggested by experience; but it is worth while fully to expound the complicated conditions of the coincidence of the point of subjective equality with the centre of the interval of uncertainty. The rapidity of the increase and decrease of the psychometric functions depends on the quantities h_1 and h_2. Experience shows that the probabilities of the "greater" judgments increase about as rapidly as those of the "less" judgments decrease. Experience further shows that the values of h_1 and h_2 never differ very much. Similar things may be said about the formulae for the psychometric functions. These formulae are certainly hypothetical; but the hypothesis seems natural, and is one that agrees well with the results of experimentation.

The coincidence of the point of subjective equality with the centre of the interval of uncertainty was found empirically, and the hypothesis about the form of the psychometric functions was suggested by experience. We do not want to lay too much stress on this fact. We merely mention it in explanation of the way in which these propositions were found. It must not be regarded as a general justification of the use of the method of least squares in the theory of errors of observation. The psychometric functions depend on the conditions of experimentation and results obtained under one set of conditions do not necessarily apply to other conditions which are perhaps entirely different. To this must be added that until now no data are at hand which were obtained under conditions directly comparable to those obtaining in actual measurement. The success of adjusting certain measurements by the method of least squares is a practical, but strong reason for assuming that the conditions of its application are given in these fields; but a really satisfactory proof can be obtained only by the investigation of the psychometric functions.

We wish to insist on the complex nature of the conditions involved in the fundamental notions of the theory of errors of observation. We also wish to lay stress on the difficulties which are due to the fact that the psychophysical nature of the observer enters into the conditions of physical measurement.

The further course of the analysis is a little too technical to be presented here, and the reader may be referred to the last chapter of the writer's book on the calculus of probabilities. It is shown that there exists a close relation between the formulae of the theory of errors of observation and those of psychophysics. We just mention the most curious one. The Gaussian measure of precision, h, as defined in the theory of errors of observation is related to the interval of uncertainty by the formula

$$h=\frac{1}{\sqrt{\pi}(S_2-S_1)}.$$

The coefficient of precision is inversely proportional to the interval of uncertainty. In psychophysics the accuracy of sense-perception is put inversely proportional to this interval; from which it follows that the notion of accuracy as used in psychophysics is identical with the notion of precision used in the theory of errors of observation. Measurements of different precision are related to one another in the same way as if they were made by sense-organs of different sensitivity. The coefficient of precision as determined by the formula of Gauss is in the same sense a measure of this sensitivity as the interval of uncertainty.

THE RANGE FOR VISUAL ATTENTION, COGNITION AND APPREHENSION

By H. Sherman Oberly, University of Pennsylvania

The range of visual attention has been given in various textbooks as the grasping of from 4 to 8 simultaneously presented objects.[1] Fernberger concludes his study on the range of visual apprehension by stating that an experiment offering but one type of verbal report to the successive exposure of visual stimuli calls for the term "range of apprehension" to cover or identify the results. He states that it would be apparently impossible to assume the threshold for apprehension at more than 6 or 8 objects. He goes further and points out a possible difference between the range of attention and the range of apprehension. It was first suggested in this paper that such results would be more reliable and practical if the statistical limen or threshold were found; the limen being the point where the stimuli were correctly apprehended with a relative frequency of 0.5.

Titchener[2] defines attention as the increase of clearness for all objects within the range—an equal clearness, or the clearness of the "upper level." The "lower level" includes those objects which are not as clear to the observer, and those which are recognized and may be included in a cognitive process. Bain[3] stated that some of the things which pass before the eye can be observed by concentrating upon one object, with the exclusion of the others. This he refers to as the "preference of attention." Sully[4] has also mentioned that "psychical phenomena present themselves in unequal degrees of definiteness or distinctness." Further on: "attention may be defined as mental activity immediately resulting in a raising in point of intensity, completeness and definiteness of certain sensations. . . .and a corresponding lowering of any other simultaneously presented sensation." These definitions apparently seem to fall in the same category as that of Titchener, emphasizing clearness and distinctness.

Dallenbach[5] came to the same conclusion that Fernberger reached after experimentation: that the experiment heretofore

[1]Cf. S. W. Fernberger, A Preliminary Study of the Range of Visual Apprehension, this Journal, 32, 1921, 121-133. The statistical results of former experiments have been summarized in this paper.

[2]E. B. Titchener, *The Psychology of Feeling and Attention*, 1908, 220 ff.

[3]A. Bain, *The Senses and the Intellect*, 1868, 558.

[4]J. Sully, *Outlines of Psychology*, 1902, 84 f.

[5]K. M. Dallenbach, Attributive vs. Cognitive Clearness, *Journ. Exp. Psychol.*, 3, 1920, 183-230.

designed to measure the range of attention has been measuring the range of apprehension. Dallenbach associates the "lower level" of clearness (which is the same as expressed by Titchener) with cognition; the "upper level" being associated with straight attention. If one should then consider the results from the point of view of only the "upper level" of clearness, one would have the true range of attention.

The purpose of this paper is to add introspective reports to an experiment which heretofore has been called a "range of attention" experiment, and by so doing, to attempt to differentiate the thresholds of attention, cognition and apprehension as defined above. The procedure follows that of Fernberger in his study of the range of visual apprehension.[6]

Preliminary Series

The preliminary series were begun in the Fall of 1922. White cards with black dots, varying in number from 4 to 15, were exposed in a Whipple Tachistoscope for an exposure approximating 40σ. This procedure was similar to that employed by Fernberger; but the subjects were required to give an introspective report of the mental processes involved in the formation of their judgments.

The introspective reports of the *O*s indicate that there are possible, and of very frequent occurrence, three conscious patterns which form the basis of the verbal report of the stimuli perceived. These three patterns were described as follows. (1) *Immediate.* All the dots were perceived in a single flash of consciousness with an equal and high degree of clearness. (2) *Grouping.* All the dots were seen in a single flash of consciousness, but, owing to different degrees of clearness, there was an attentional grouping of the stimulus dots. Both of the two conscious patterns characterized above, however, gave rise at once to the verbal report without any subsequent mediating conscious processes. (3) *Counting.* This process was different from either of the other two, inasmuch as the report was always made on the basis of some subsequent re-imaging of the stimulus material. The mediating process varied from *O* to *O* and for the same *O* from time to time. The processes consisted frequently in a visual image, the dots of which were then counted verbally; or of a verbal counting with ocular-motor fixation of the position of the dots or other similar sorts of processes. Also, all of the *O*s reported wide differences in the degree of subjective assurance with which the judgments were reported.

Arrangement of the Experiment

The experiment was conducted in the Psychological Laboratory of the University of Pennsylvania. Six *O*s were chosen from the staff of the department.

[6] *Op. cit.*, 124.

All of these *Os* had taken part in the preliminary experiments and so were familiar with the materials and methods of exposure. The *Os* were Samuel W. Fernberger, Ph.D., Karl G. Miller, Ph.D., Assistant Professors of Psychology; Sidney Sanderson, M. A., Malcolm K. Macdonald, M. A., Stanley F. Cooper, B. S., Instructors of Psychology; and John W. Cooper, A. B., Assistant in Psychology. They are hereinafter referred to as F, M, S, Mc, SC and JC respectively. F, M and Mc were highly trained in giving introspective reports; S had but little and SC and JC practically no systematic introspective experience.[7]

Five series of white cards, 87 mm. square were prepared. From 2 to 15 black dots,[8] 6.5 mm. in diam. were pasted in haphazard arrangements on the white backgrounds. All of these were exposed in rotation by means of a Whipple Tachistoscope,[9] with an exposure of 37.5σ calibrated by the tuning fork method. The photographic dark-room of the laboratory was used in order to keep the illumination on the stimulus card and the general illumination constant, the only source of illumination being that of the exposure lamp. The *O* was seated 1.9 m. from the point of fixation.

The order of the stimulus cards was different in each of the five series and in each case was arranged in haphazard fashion. After the exposure of all the cards, they were turned 90° so that the right edge of each card was now the bottom of the card for the next group of exposures. This method of rotating the cards was continued throughout the experiment. Thus, any stimulus card was seen in the same (identical) position only after each 280th exposure. In this manner, and also by starting at different places in the series, the probability of the *O* memorizing the order of the series or of his recognizing any particular card was very much reduced.

For F, M and S the stimuli of from 11 to 15 dots were eliminated after some practice, inasmuch as judgments of "I do not know" were invariably given. For F, the cards were resumed for the last 50 judgments of each stimulus, for the reason that improvement was very evident.

The final experiment was begun in May 1923, with the *Os* sitting not more than an hour for any one day. Not more than 4 sittings were arranged for any one week. The *O* was allowed to rest for a period of several minutes whenever he chose. Two hundred and fifty judgments for each number of the stimulus dots were obtained from each *O*. The observations were completed by November 1923.

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[7]I am greatly indebted to Professor Fernberger for the suggestion of this problem, for his guidance and valuable assistance in the preparation of this paper. My thanks are also due the subjects who made the experiment possible.

[8]Dennison's No. 12 Gummed Signal Marks were used.

[9]For a description of this apparatus of G. M. Whipple, *Manual of Mental and Physical Tests*, i., 1924, 264 f.

The directions which were read to the *O* at the beginning of each experimental sitting were as follows:

"You will be shown successively for a very short exposure, cards which contain a varying number of black dots on a white background. Immediately after the exposure you will report verbally:

1. the number of dots which you have apprehended;
2. the degree of assurance or certainty of your judgment, using this scale for your report:

5—if it is a 100 to 1 bet,
4—if it is a 5 to 1 bet,
3—if it is a 1 to 1 bet,
2—if it is a 1 to 5 bet,
1—guess or do not know;

3. the method of determining your report; whether by Immediate judgment, *i.e.*, ungrouped, Grouping of dots, or Counting."

Each *O* was given some practice under the new directions before any record was taken of the judgments.

It is obvious that the introspective characterizations do not correspond to the systematic categories that have appeared in the literature. It was necessary therefore to transform the results obtained into some form of systematic categories for the purpose of study and of calculation. From a study of the literature it seems that the introspective characterization which has been described above and which appears in the instructions as *immediate* corresponds exactly to one level—*Attention,* as described by Titchener. These results have therefore been calculated under this heading.

The term *Cognition* as it appears in the literature seems to be a process which is immediate but which does not imply a single level of clearness. The principal aspect which characterizes cognition is its immediacy. It will be observed that the introspective categories of *immediate* and *grouping* are both characterized by their immediacy, although the second is further characterized by different degrees of clearness which lead to an attentional grouping. The results of these two introspective categories have therefore been combined under the title of *Cognition.*

The term *Apprehension* as it appears in the literature signifies the obtaining of a knowledge of the stimulus without restriction to the mental processes underlying such knowledge. All three of the introspective categories have therefore been combined for purposes of calculation for this systematic concept. In other words, the results for both *immediate* and *grouping,* as the two ways of getting knowledge without subsequent process, have been combined with those for *counting,* which involves such a mediating process.

The results for Attention consist solely of those which were characterized as immediate by the *O*s. The results for Cognition are those which the *O* characterized as immediate *plus*

those characterized as grouping; and finally, the results for Apprehension are a combination of the results which were characterized by the *O*s under all three introspective headings. The schema given below will perhaps still further clarify this transformation of introspective characterizations into systematic categories.

Introspective Characterization	Systematic Characterization
Immediate	Attention
Grouped	Cognition
Counted	Apprehension

It was arbitrarily determined to use for purposes of calculation only those reports which were correct and which had an assurance of 5, 4 or 3. This means that the *O* was sure enough of his report to put even money on it, and this procedure eliminates the doubtful judgments. Therefore, judgments with the assurance of 2 or 1 were eliminated from the calculations. All of the *O*s (except F) worked in entire ignorance of this elimination.

The results for each of the three systematic categories were fractionated into five groups of 50 judgments for each value of the stimulus in the order taken. The calculations were made in accordance with the phi-gamma hypothesis and these calculations were simplified by the use of Urban's Tables.[10] Such a calculation yields a statistical threshold for each of our three systematic categories as well as a statistical index of precision of the results.

TABLE I—OBSERVER F

	2	3	4	5	6	7	8	9	10	11	12	13	14
ATTENTION													
I	0.78	0.62	0.38	0.14	0.04								
II	0.72	0.48	0.24	0.08	0.00								
III	0.84	0.64	0.36	0.24	0.06								
IV	0.94	0.76	0.68	0.30	0.08								
V	0.90	0.68	0.46	0.14	0.00								
AVE.	0.84	0.64	0.42	0.18	0.04								
COGNITION													
I	1.00	1.00	0.90	0.74	0.36	0.08	0.06						
II	1.00	1.00	0.96	0.66	0.34	0.02	0.00						
III	1.00	1.00	0.96	0.84	0.36	0.02	0.00						
IV	1.00	1.00	0.98	0.92	0.52	0.22	0.06						
V	1.00	1.00	1.00	0.82	0.36	0.08	0.02						
AVE.	1.00	1.00	0.96	0.80	0.39	0.08	0.03						
APPREHENSION													
I	1.00	1.00	0.94	0.92	0.66	0.52	0.32	0.06	0.04				
II	1.00	1.00	0.98	0.92	0.82	0.52	0.42	0.18	0.08				
III	1.00	1.00	1.00	0.96	0.82	0.66	0.52	0.26	0.14				
IV	1.00	1.00	1.00	0.96	1.00	0.76	0.76	0.52	0.30				
V	1.00	1.00	1.00	1.00	0.98	0.98	0.84	0.56	0.44	0.24	0.16		
AVE.	1.00	1.00	0.98	0.95	0.86	0.69	0.57	0.32	0.20				

[10]F. M. Urban, Hilfstabellen für die Konstanzmethode, *Arch. f. d. ges. Psychol.*, 24, 1912, 236-243.

TABLE II—OBSERVER M

	2	3	4	5	6	7	8	9	10	11	12	13	14
ATTENTION													
I	1.00	0.66	0.30	0.02									
II	1.00	0.74	0.36	0.00									
III	1.00	0.80	0.42	0.00									
IV	1.00	0.88	0.48	0.00									
V	1.00	0.94	0.82	0.18									
Ave.	**1.00**	**0.80**	**0.49**	**0.04**									
COGNITION													
I	1.00	1.00	0.88	0.52	0.48	0.06	0.02	0.00					
II	1.00	1.00	0.92	0.74	0.42	0.10	0.08	0.00					
III	1.00	1.00	1.00	0.98	0.64	0.14	0.18	0.04					
IV	1.00	1.00	1.00	0.98	0.72	0.16	0.22	0.04					
V	1.00	1.00	1.00	0.96	0.88	0.32	0.20	0.02					
Ave.	**1.00**	**1.00**	**0.96**	**0.84**	**0.63**	**0.16**	**0.14**	**0.02**					
APPREHENSION													
I	1.00	1.00	1.00	1.00	0.94	0.64	0.34	0.04	0.00				
II	1.00	1.00	0.98	0.96	0.82	0.70	0.60	0.14	0.00				
III	1.00	1.00	1.00	1.00	1.00	0.90	0.62	0.14	0.00				
IV	1.00	1.00	1.00	1.00	0.96	0.82	0.64	0.10	0.00				
V	1.00	1.00	1.00	0.98	0.98	0.80	0.70	0.08	0.00				
Ave.	**1.00**	**1.00**	**1.00**	**0.99**	**0.94**	**0.77**	**0.58**	**0.10**	**0.00**				

TABLE III—OBSERVER S

	2	3	4	5	6	7	8	9	10	11	12	13	14
ATTENTION													
I	1.00	0.92	0.46	0.06	0.00								
II	1.00	0.90	0.38	0.00	0.00								
III	1.00	0.92	0.50	0.04	0.00								
IV	1.00	1.00	0.76	0.10	0.06								
V	1.00	1.00	0.86	0.26	0.04								
Ave.	**1.00**	**0.95**	**0.59**	**0.09**	**0.02**								
COGNITION													
I	1.00	1.00	0.96	0.68	0.62	0.42	0.44	0.20	0.02				
II	1.00	1.00	0.94	0.64	0.66	0.58	0.52	0.04	0.00				
III	1.00	1.00	0.94	0.68	0.64	0.38	0.36	0.10	0.00				
IV	1.00	1.00	0.96	0.84	0.86	0.34	0.42	0.12	0.00				
V	1.00	1.00	1.00	0.92	0.86	0.54	0.50	0.16	0.00				
Ave.	**1.00**	**1.00**	**0.96**	**0.75**	**0.73**	**0.45**	**0.45**	**0.12**	**0.00**				
APPREHENSION													
I	1.00	1.00	1.00	0.92	0.96	0.62	0.64	0.28	0.06				
II	1.00	1.00	1.00	1.00	0.90	0.76	0.62	0.08	0.00				
III	1.00	1.00	1.00	0.98	0.92	0.68	0.50	0.12	0.00				
IV	1.00	1.00	1.00	1.00	0.96	0.56	0.58	0.14	0.02				
V	1.00	1.00	1.00	1.00	0.96	0.76	0.62	0.18	0.00				
Ave.	**1.00**	**1.00**	**1.00**	**0.98**	**0.94**	**0.68**	**0.59**	**0.16**	**0.02**				

TABLE IV—OBSERVER Mc

	2	3	4	5	6	7	8	9	10	11	12	13	14
ATTENTION													
I	0.76	0.72	0.22	0.00									
II	0.76	0.74	0.24	0.06									
III	0.72	0.80	0.34	0.20									
IV	0.72	0.82	0.28	0.20									
V	0.70	0.82	0.52	0.20									
Ave.	**0.73**	**0.78**	**0.32**	**0.13**									
COGNITION													
I	1.00	0.88	0.96	0.68	0.72	0.54	0.32	0.26	0.10	0.02	0.00		
II	1.00	0.92	0.98	0.82	0.66	0.54	0.52	0.28	0.16	0.04	0.00		
III	1.00	1.00	1.00	0.90	.076	0.54	0.58	0.16	0.14	0.02	0.00		
IV	1.00	1.00	1.00	0.98	0.76	0.56	0.60	0.12	0.14	0.00	0.00		
V	1.00	1.00	1.00	0.96	0.74	0.58	0.72	0.18	0.06	0.00	0.00		
Ave.	**1.00**	**0.96**	**0.99**	**0.87**	**0.73**	**0.55**	**0.55**	**0.20**	**0.12**	**0.02**	**0.00**		
APPREHENSION													
I	1.00	0.98	1.00	0.94	0.84	0.60	0.44	0.26	0.12	0.02	0.00		
II	1.00	1.00	0.98	0.98	0.72	0.58	0.54	0.30	0.16	0.04	0.00		
III	1.00	1.00	1.00	0.96	0.84	0.70	0.72	0.20	0.14	0.02	0.00		
IV	1.00	1.00	1.00	1.00	0.84	0.68	0.66	0.16	0.14	0.00	0.00		
V	1.00	1.00	1.00	0.98	0.86	0.70	0.80	0.24	0.08	0.02	0.00		
Ave.	**1.00**	**1.00**	**1.00**	**0.97**	**0.82**	**0.65**	**0.63**	**0.23**	**0.13**	**0.02**	**0.00**		

TABLE V—OBSERVER SC

ATTENTION

	2	3	4	5	6	7	8	9	10	11	12	13	14
I	1.00	0.98	0.94	0.58	0.10								
II	1.00	0.98	0.92	0.34	0.06								
III	1.00	1.00	0.96	0.48	0.10								
IV	1.00	1.00	1.00	0.62	0.16								
V			1.00	0.68	0.20								
AVE.	**1.00**	**0.99**	**0.96**	**0.54**	**0.12**								

COGNITION

	2	3	4	5	6	7	8	9	10	11	12	13	14
I	1.00	1.00	1.00	0.96	0.82	0.52	0.54	0.16	0.06	0.02	0.00		
II	1.00	1.00	1.00	0.96	0.92	0.74	0.82	0.52	0.34	0.14	0.08		
III	1.00	1.00	1.00	1.00	0.96	0.92	0.80	0.56	0.22	0.28	0.16		
IV	1.00	1.00	1.00	1.00	1.00	0.88	0.76	0.56	0.60	0.50	0.40		
V			1.00	1.00	0.98	0.94	0.84	0.76	0.48	0.40	0.20	0.04	
AVE.	**1.00**	**1.00**	**1.00**	**0.98**	**0.94**	**0.80**	**0.75**	**0.51**	**0.34**	**0.27**	**0.17**		

APPREHENSION

	2	3	4	5	6	7	8	9	10	11	12	13	14
I	1.00	1.00	1.00	0.98	0.94	0.68	0.70	0.16	0.08	0.02	0.00		
II	1.00	1.00	1.00	1.00	0.96	0.78	0.86	0.52	0.34	0.14	0.08		
III	1.00	1.00	1.00	1.00	0.96	0.92	0.80	0.56	0.22	0.28	0.16		
IV	1.00	1.00	1.00	1.00	1.00	0.88	0.76	0.56	0.60	0.50	0.40		
V			1.00	1.00	0.98	0.94	0.84	0.76	0.48	0.40	0.20	0.04	
AVE.	**1.00**	**1.00**	**1.00**	**1.00**	**0.97**	**0.84**	**0.79**	**0.51**	**0.34**	**0.27**	**0.17**		

TABLE VI—OBSERVER JC

ATTENTION

	2	3	4	5	6	7	8	9	10	11	12	13	14
I	0.60	0.64	0.22	0.08	0.00	0.02							
II	0.64	0.58	0.14	0.02	0.00	0.00							
III	0.76	0.62	0.38	0.00	0.00	0.00							
IV	0.80	0.68	0.60	0.00	0.02	0.00							
V	0.86	0.90	0.84	0.14	0.00	0.00							
AVE.	**0.73**	**0.68**	**0.44**	**0.05**	**0.00**	**0.00**							

COGNITION

	2	3	4	5	6	7	8	9	10	11	12	13	14
I	1.00	1.00	0.66	0.66	0.58	0.34	0.12	0.00	0.00	0.00	0.00		
II	1.00	0.98	0.54	0.54	0.58	0.14	0.00	0.00	0.00	0.00	0.00		
III	1.00	0.98	0.54	0.28	0.26	0.02	0.02	0.00	0.00	0.00	0.00		
IV	1.00	1.00	0.62	0.18	0.14	0.00	0.04	0.00	0.00	0.02	0.00		
V	1.00	1.00	0.84	0.24	0.02	0.00	0.02	0.00	0.00	0.00	0.00		
AVE.	**1.00**	**0.99**	**0.64**	**0.38**	**0.32**	**0.10**	**0.04**	**0.00**	**0.00**	**0.00**	**0.00**		

APPREHENSION

	2	3	4	5	6	7	8	9	10	11	12	13	14
I	1.00	1.00	1.00	0.98	0.86	0.80	0.52	0.04	0.04	0.00	0.00		
II	1.00	1.00	0.98	1.00	0.86	0.84	0.54	0.16	0.08	0.00	0.00		
III	1.00	0.98	0.94	0.94	0.96	0.92	0.44	0.08	0.00	0.00	0.00		
IV	1.00	1.00	1.00	0.98	0.88	0.94	0.52	0.06	0.00	0.02	0.02		
V	1.00	1.00	1.00	1.00	0.94	0.92	0.42	0.08	0.04	0.00	0.00		
AVE.	**1.00**	**1.00**	**0.98**	**0.98**	**0.90**	**0.88**	**0.49**	**0.08**	**0.03**	**0.00**	**0.00**		

Results

The observed relative frequencies for each *O* appear in Tables I to VI, one table given to the results of each subject. In the first columns are the series into which the results were fractionated. In the successive columns are the observed relative frequencies of judgments for the different stimulus values. Each table is divided into three parts,—the upper third giving the values for Attention, the middle the values for Cognition, and the lower third the values for Apprehension. The averages are given in the bottom row of each section.

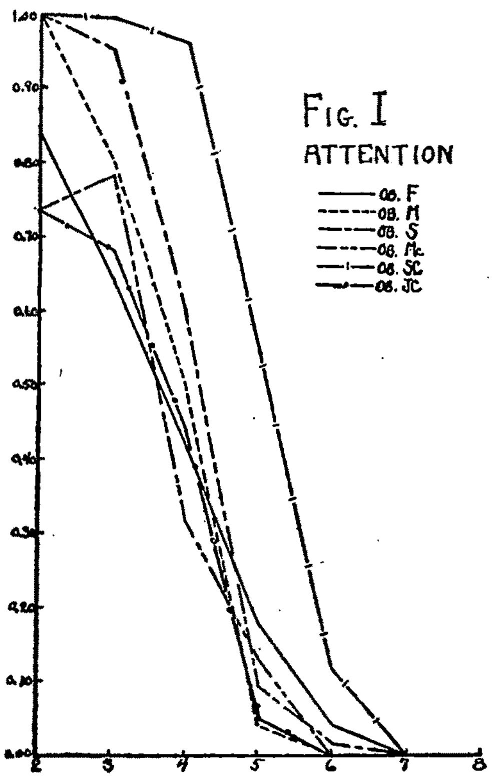


FIG. I
ATTENTION
OB. F
OB. M
OB. S
OB. Mc
OB. SC
OB. JC
1.00
0.90
0.80
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
2
3
4
5
6
7
8

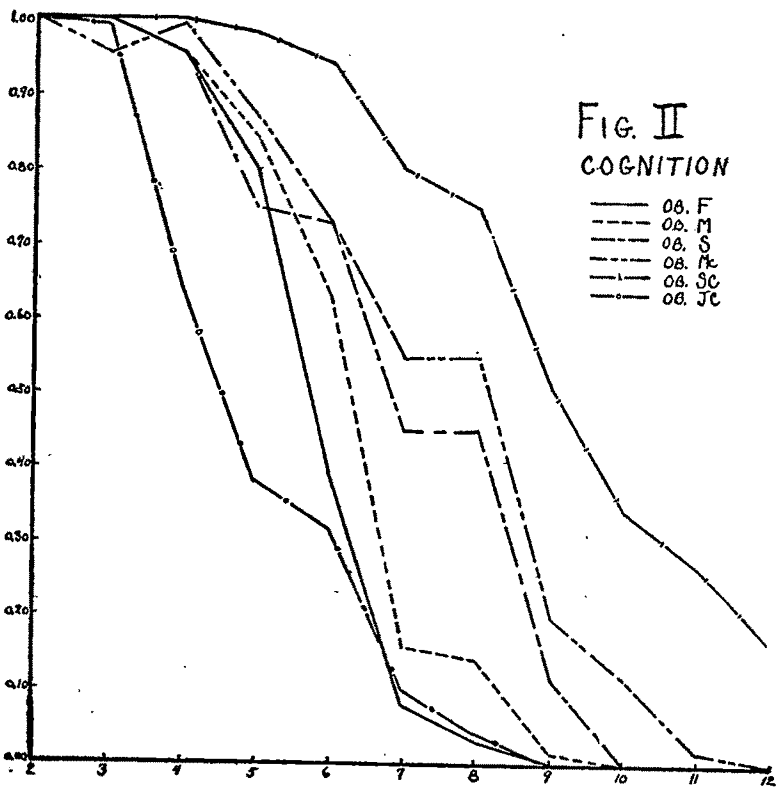


FIG. II
COGNITION
OB. F
OB. M
OB. S
OB. Mc
OB. SC
OB. JC
1.00
0.90
0.80
0.70
0.60
0.50
0.40
0.30
0.20
0.10
0.00
2
3
4
5
6
7
8
9
10
11
12

These average results are shown in graphic form in Figg. I to III. In Fig. I the curves of the psychometric function for Attention for all 6 *O*s are superimposed. Figg. II and III are similarly constructed for Cognition and Apprehension respectively. By inspection of these curves it can readily be noted that the attention judgments are relatively similar for all of the *O*s, tending to group together, with the curves of the psychometric functions relatively steep and very similar in both form and position. In the cognition process their results are widely distributed and the curves are relatively less steep. In the third or apprehension series the results for the different *O*s tend to group together again and the curves drop more rapidly than those for cognition, once they start, but not so rapidly as the ones for attention.

In Fig. IV the curves are based on the averages of the observed relative frequencies for each *O*. In other words, the curves which are relatively similar in form are widely separated

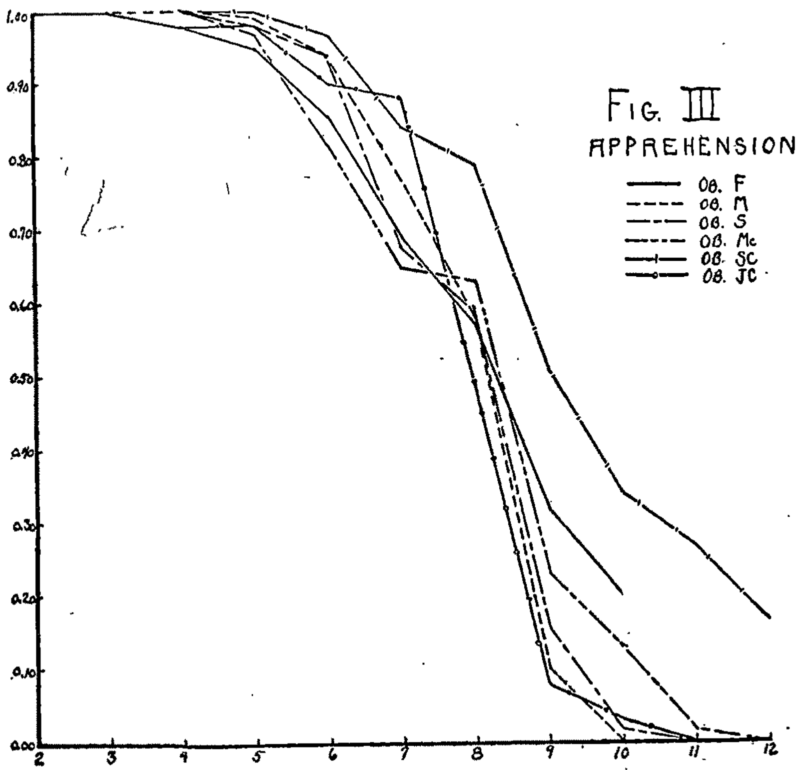
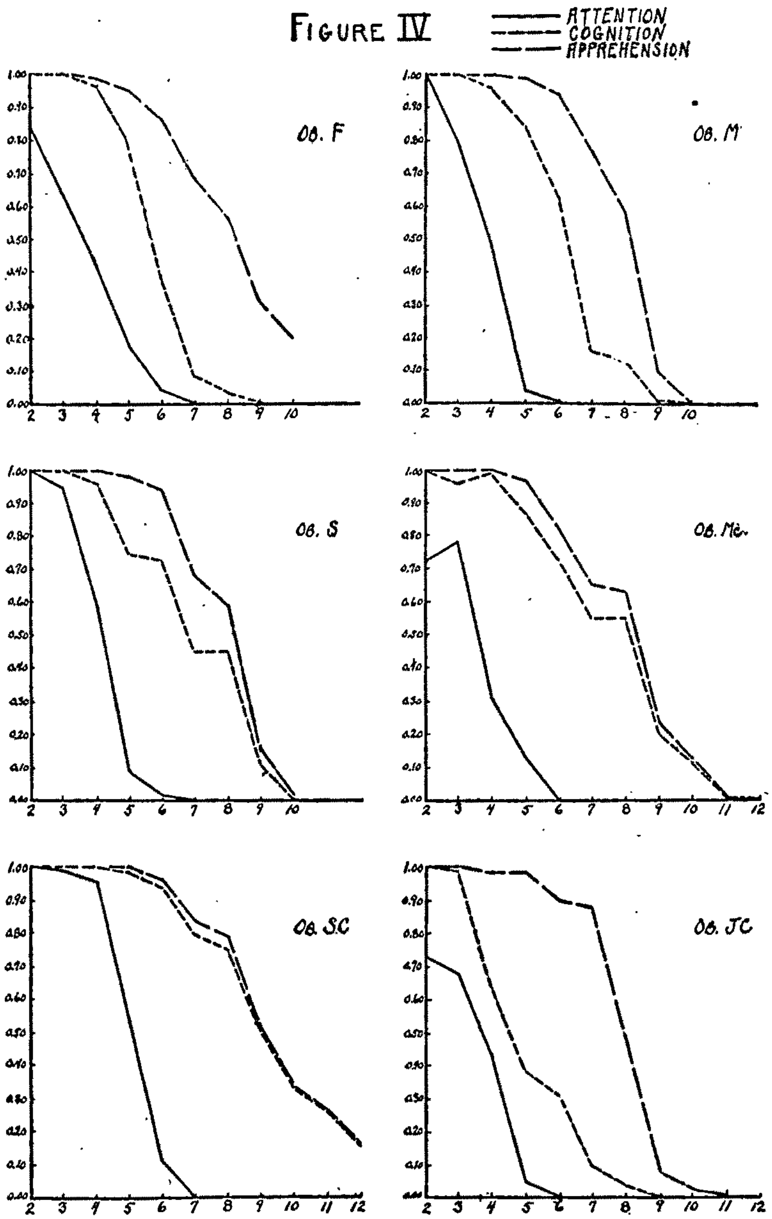


FIGURE IV


ATTENTION
COGNITION
APPREHENSION
OB. F
OB. M
OB. S
OB. Mc
OB. SC
OB. JC

each from the other with relation to the abscissa. For Mc and SC the curves for cognition and for apprehension are relatively similar with regard to both form and position in relation to the abscissa. For these *Os*, however, both curves are exceedingly shifted toward the higher values from the curves for attention. The curves for S and JC have an intermediate relationship. In both cases the curves for attention and apprehension are widely separated. For JC the curve for cognition, although quite different from either of the other curves, tends more nearly to approach that of attention; for S the curve for cognition tends to approach that for apprehension.

This indicates that for all *Os* a marked difference in results was obtained between attention and apprehension. For F and M cognition was markedly different from either. For Mc and SC, cognition was markedly different from attention but not from apprehension. For S cognition was markedly different from attention, but less markedly different from apprehension; and for JC the opposite relations seem to hold.

TABLE VII—OB. F

Series	Attention		Cognition		Apprehenison	
	s	h	s	h	s	h
1	3.381	0.4461	5.642	0.56011	6.957	0.41432
2	2.9028	0.46657	5.453	0.7798	7.430	0.40075
3	3.5936	0.43048	5.6712	0.87154	7.907	0.37708
4	4.2617	0.49782	6.2104	0.67397	9.0396	0.30712
5	3.7028	0.53656	6.2385	0.74927	9.753	0.37373
Ave.	**3.5684**	**0.47551**	**5.8430**	**0.72694**	**8.2173**	**0.37460**

TABLE VIII—OB. M

Series	Attention		Cognition		Apprehension	
	s	h	s	h	s	h
1	3.2175	0.8165	6.5679	0.54419	7.1625	0.63119
2	3.6548	0.6374	5.7312	0.53775	7.8265	0.41616
3	3.8242	0.6921	6.4668	0.54839	8.1632	0.82908
4	3.9643	0.84696	6.7033	0.59919	8.0343	0.67657
5	4.4256	0.95254	6.8862	0.66309	7.8942	0.60288
Ave.	**3.8173**	**0.78910**	**6.4711**	**0.57852**	**7.8161**	**0.63118**

TABLE IX—OB. S

Series	Attention		Cognition		Apprehension	
	s	h	s	h	s	h
1	3.9356	1.00562	6.8166	0.28966	7.9785	0.40806
2	3.7866	1.27356	7.1027	0.2595	7.898	0.55466
3	3.952	1.0608	6.612	0.31975	7.7342	0.55304
4	4.4299	0.89562	7.0935	0.38865	7.7152	0.55151
5	4.6765	1.10118	7.5633	0.4103	8.0746	0.57484
Ave.	**4.1561**	**1.06736**	**7.0376**	**0.33357**	**7.8801**	**0.52842**

TABLE X—OB. Mc

Series	Attention		Cognition		Apprehension	
	s	h	s	h	s	h
1	3.239	0.53834	6.926	0.26690	7.656	0.36796
2	3.2463	0.56564	7.416	0.26844	7.8378	0.31566
3	3.5433	0.38213	7.5652	0.34898	8.0724	0.40855
4	3.486	0.4038	7.6728	0.36966	7.9762	0.39329
5	3.823	0.33432	7.6633	0.37664	8.1148	0.44663
Ave.	**3.4675**	**0.44485**	**7.4487**	**0.32612**	**7.9314**	**0.38642**

TABLE XI—OB. SC

SERIES	ATTENTION		COGNITION		APPREHENSION	
	S	h	S	S	S	h
1	5.0705	0.85512	7.7375	0.42829	8.066	0.49981
2	4.7907	0.9503	9.026	0.32466	9.1652	0.36107
3	4.9495	1.09075	9.4826	0.3426	9.4826	0.3426
4	5.2291	0.90422	10.8025	0.17593	10.8025	0.17593
5	5.3538	0.87194	10.174	0.35036	10.174	0.35036
AVE.	**5.0787**	**0.93447**	**9.4445**	**0.32437**	**9.5381**	**0.34595**

TABLE XII—OB. JC

SERIES	ATTENTION		COGNITION		APPREHENSION	
	S	h	S	h	S	h
1	2.866	0.38522	5.775	0.26642	7.7536	0.54889
2	2.7664	0.52973	7.5609	0.28349	7.9504	0.40353
3	3.4722	0.3566	4.4821	0.44878	7.793	0.38594
4	3.868	0.39123	3.718	0.26116	8.0354	0.47734
5	4.3564	0.46676	4.6121	0.6023	7.8932	0.69568
AVE.	**3.4658**	**0.42591**	**5.2296**	**0.26402**	**7.8851**	**0.50228**

The calculated results for all the *O*s appear in Tables VII to XII. The first columns show the fractionated series for the results of the *O*s. The tables are divided next into three groups which indicate the systematic characterizations, Attention, Cognition and Apprehension, and these are each divided into two columns showing the value of the threshold (S) and the value of *h*, which is considered as the index of precision, for each series. The bottom row of figures in each table shows the averages for the values of S and *h* for each of the three systematic categories.

The Limens

The average value of S in the case of F (Table VII) shows a wide difference for each of the characterizations, ranging from 3.6 for attention to 5.8 for cognition to 8.2 for apprehension. JC (Table XII) is the next in rank order to show such a variation, with the changes in the value of S from 3.5 to 5.2 to 7.9. M (Table VIII) shows the variation from 3.8 to 6.5 to 7.8. Mc (Table X) shows a similar wide range of difference, from 3.5 to 7.4 to 7.9; the difference between cognition and apprehension is only 0.5. The results of S (Table IX) are very similar to those of Mc in this respect, for his value of S for attention is 4.2, for cognition is 7.0 and for apprehension 7.8; the difference between the threshold value for cognition and apprehension is but 0.8. SC (Table XI), as may be noted in the charts above, has the highest range of any of the *O*s, and his results follow the same general trend; from the value of 5.1 for attention to 9.4 for cognition, and 9.5 for apprehension. As was stated above, concerning the curves drawn from the averages of the observed relative frequencies which are shown in Fig. IV, the difference between attention and apprehension in all cases of the *O*s is marked and striking. The results of F, M and JC show

a difference in the values of S between attention and cognition, and also between cognition and apprehension. With S, Mc, and SC the difference between cognition and apprehension is much less marked.

An examination of the threshold values for the individual fractions shows that for F, M, S and Mc the relations which have been described for the averages hold throughout. There is one exception in the results for JC,—in fraction 4 the threshold for cognition is very slightly less than that for attention. For SC the values of S for fractions 3, 4 and 5 are the same for the categories of cognition and apprehension, a result which indicates that for these 3 fractions the *O* made not one single report which was characterized as counting.

Progressive Practice

An analysis of the results of the value of S for the different fractions of our *O*s from the point of view of progressive practice shows the following for attention. All of the *O*s show an increase in the size of the threshold as experimentation progressed. For M and Mc the increase was steady from fraction to fraction, amounting in the case of Mc only to 0.58, while in the case of M the increase is 1.2. For S, SC and JC there is only one inversion in the size of the threshold, and in every one of these cases the inversion occurs by the threshold in the first series being larger than that for the second (and for SC for the third also). The amount of increase for these *O*s from the first to the last series is: S, 0.74; SC, 0.28; JC, 1.49. For F an increase in the size of the S from the first to the last series is also apparent, amounting to 0.32. Two inversions are to be found in this series, however, the order of the fractions with regard to magnitude of the S being 2, 1, 3, 5, 4.

For 5 of the *O*s the thresholds of cognition also show an increase with progressive practice, the difference in the size of the first and the last thresholds being for F, 0.59; M, 0.30; S, 0.74; Mc, 0.73; SC, 3.06. The value of S for none of these *O*s shows a steady increase from fraction to fraction. The values for JC are anomalous, inasmuch as there is a decrease in the size of the threshold from the first to the last fraction of 1.17, and the order of the series with regard to magnitude is 4, 3, 5, 1, 2.

A similar increase with regard to magnitude of the threshold values for all *O*s is to be found for progressive practice in apprehension. F alone shows a systematic increase for the 5 fractions, the total amount of increase being 2.8. The amount of increase for the other *O*s is: M, 0.73; S, 0.09; Mc, 0.45; SC, 2.73; JC, 0.14. For Mc and SC only one inversion is found for the 5 individual fractions, while for the other three *O*s (M, S and JC) the order is quite irregular.

It would seem then that progressive practice has an effect of varying degree upon the different systematic categories for the *O*s who have been studied. In the case of F a large effect of progressive practice is to be found for apprehension, a considerably smaller effect for attention and cognition, although the increase for cognition is slightly greater than that for attention. For M the greatest amount of progressive practice is to be found for attention; slightly less for apprehension, and considerably less for cognition. For S there is an equally marked effect for attention and cognition, with an almost negligible effect for apprehension. For Mc the magnitude of the effect is not very different for the three categories, the order being cognition, attention and apprehension. For SC a very marked effect is found for cognition and also for apprehension, while a negligible effect only is found for attention. For JC a large effect is found for attention, a negligible effect for apprehension, and a negative effect for cognition. It seems obvious, as was also borne out by the introspection of this *O*, that the apparent negative effect of progressive practice upon cognition was really an artifact. This was brought about by the increase in training in introspection which the *O* obtained during experimentation, which led him to classify in the later experiments many of the processes under counting which had formerly been classified under grouping.

The following Table indicates the number of *O*s who showed gains in the value of S for the systematic characterizations Attention, Cognition and Apprehension. The first column shows the number of the series, and the number of *observers* listed under the following three columns indicates those whose results show an increase over the preceding series.

Series	Attention	Cognition	Apprehension	Total
2	2	4	5	11
3	6	4	4	14
4	5	5	3	13
5	5	4	3	12
Total	18	17	15	50

The possible totals under attention, cognition and apprehension are 24 each.[11] This would indicate that there are more cases of gains in attention than for cognition or apprehension. The possible totals in the last column for each series are 18 each. This indicates that the second series was the poorest for the *O*s, and it may be stated here that the longest period between any two series was between the first and the second. The third and fourth fractions were run through within a short period of time; but there is still a decrease evident. There are more variations

[11]Four series, six *O*s.

in the results for cognition and apprehension, which may account for the falling off of gains—while attention seemed to be more constant and steady. For the totals of neither columns or rows are the differences great.

Index of Precision

When one considers the average values of the index of precision (h) for the three systematic categories, the following relations seem to hold.

For M, S, Mc and SC the average value of h for attention is higher than for either of the other two; the value for apprehension is next, and that for cognition is lowest. This means that for these four *O*s the curves for the psychometric functions for attention were steepest, while those for cognition were least steep.

These relations do not hold when one considers the individual fractions into which the results of these *O*s were divided, with the exception of S, where the relations described above hold throughout. For M three exceptions are to be noted: in the second fraction the relations for cognition and apprehension are reversed; for the third fraction the h for cognition is highest; in the fifth fraction the order is: cognition highest, attention and apprehension. Two exceptions are to be noted in the values of h for Mc. In the third fraction the order is: apprehension highest, then attention and cognition. In the fifth fraction the order is: apprehension, cognition and attention. For SC in the last three fractions, attention is always highest, and the values for h for cognition and apprehension are exactly the same.

F gives values for the coefficient of precision which do not agree with the average values of the four *O*s whose results have just been analyzed. For this *O* the average values of h for cognition are highest, attention second, and apprehension lowest; and these relations hold true for every one of the five fractions into which the results were fractionated.

JC has average results which are also at variance with the first four *O*s analyzed. For him the value of h is largest for apprehension, attention second, and cognition smallest. Only two of his fractions agree with his average results, however,—namely, 1 and 4. For fraction 2 the order is attention, apprehension, cognition; for fraction 3, cognition, apprehension, attention; for fraction 5, apprehension, cognition, attention.

For the majority of the *O*s, therefore, the least precision is found with cognition. F, who has had the most experience with introspective methods, ranks high with cognition, while JC, who has had the least amount of training in the introspective method, has the lowest value of h for cognition.

Progressive Practice

The effect of progressive practice on the index of precision (h) seems to be variable from *O* to *O*. Perhaps one can best illustrate by using the following diagram; + indicating an increased effect; o, no effect; − a negative effect.

Observer	Attention	Cognition	Apprehension
F	+	o	−
M	+	+	o
S	o	+	o
Mc	−	+	+
SC	−	−	−
JC	o	o	o

Taking the algebraic sum of these symbols no effect is found for attention, a slight positive effect for cognition (+2), and a negative effect (−1) for apprehension.

Discussion

I. *Are the three systematic categories properly chosen and properly defined experimentally?* In attention, according to Titchener, the organism is interested in the object at hand. There are not active and passive states of attention, but there is one state of attention when all the organism is directed to the exposed stimulus with the exclusion of other material. In this experiment the *O*s were instructed to make a verbal report of the number of stimulus dots apprehended, and to give an introspective report as to how they determined the number of dots apprehended. The report "immediate" was to be given in this experimental procedure when there were no subsequent mental processes involved and only when all parts of the process were of an equal degree of clearness.

As this method of judging is limited in regard to the mental processes involved, there appears to be justification in calling the first of our systematic categories *attention*. It obviously agrees with the definition of attention as given by Titchener, and experimentally it has been found that the *O*s indicate this in their introspections at the completion of the experiment.

M reports: "Where an immediate judgment was reported, the dots seemed of equal clearness and definition. Introspection shows no interval between the exposure of the card and the number perception. In every case 2 dots were apprehended immediately. For the 3-dot stimulus the condition for immediate judgment demanded approximately equal spacing and not great extension. In many cases the difference between immediate and grouping judgments was determined not so much by the number of dots exposed as by the spatial arrangement.[12] Invariably immediate judgments were given with the greatest assurance. The verbal report followed the exposure with almost the speed of a simple reaction."

[12] Cf. A. M. Bowman, Size vs. Intensity as a Determinant of Attention, this JOURNAL, 31, 1920, 87-90.

S reports: "An immediate judgment was given without attention to the perseverating image, and a high degree of confidence in its correctness always accompanied it. [A "5" judgment.] This applied to numbers 2 and 3, and usually 4 which could be apprehended in a single group."

Mc reports: "In judging the number of dots a definite effort is made to group them. This makes the estimation of their number easier and more accurate. In cases where there is only one group I make a report of 'immediate'."

SC reports: "With immediate judgments I find, with the smaller groups (3, 4 and occasionally 5) there is a simultaneous and equal clarity, *i.e.*, all dots stand out clear cut and of equal blackness at one time; with the larger groups (most of the 5s, and all of the 6s) there is an *immediate consciousness* of 6—or 5, with a slightly greater clarity to a certain component (2 or 3) than to the rest, which elicits a re-reading of the memorial image—this seems to be due to the point of fixation, and varies from time to time with the same card. All immediate judgments were attended by a high degree of assurance."

JC reports: "At first the judgments reported as immediate were so in a process quite analogous to a reflex. There was no recognition in consciousness of any process except the visual, and the report was not elicited by any other element than the stimulation. That is to say, no sooner was the stimulus presented than the report was made, and an actual consciousness of the report itself, or of its nature, was not experienced until it was given."

These introspections indicate that the verbal reports of "immediate" are definitely linked with the single level of clearness which Titchener mentions. SC was one of the *O*s with practically no training in the introspective method, and his results show that he has the highest range of attention of the *O*s—and he uses the method of rechecking his results *after* he has given his report on the number of stimulus dots apprehended. Here the organism is limited, and it is noticeable in our results that there is a corresponding limited range of attention for the *O*s. A report of immediate judgment is similarly given with the grouping process, but the *O* is not limited to the single level of clearness as described above. The *O* now is permitted to use other immediate means of apprehending the number of stimulus-dots on the cards, and he brings into operation another level of clearness or mental process. The organism practically doubles the process involved in attention, and this judgment is called *cognition*, whether it consists of one level or two levels of clearness. Dallenbach[13] found in an experiment on sensory clearness that two levels were possible, and as mentioned above in the introduction, the experiment dealing with two such levels is an experiment in cognition, and not one in attention as heretofore described in the literature. That is, this dual level of clearness permits the *O* to report a greater number of stimulus-dots than he does in attention proper. Consequently, there are several attentional processes which are grouped and which are reported verbally as "grouping," and there is a corresponding increase in the range of cognition over

[13] *Op. cit.*, 230.

the range of attention. This range of *cognition* is defined experimentally in the fact that the *O*s report the grouping of the stimulus dots. Further, examine the introspection of each *O*.

M reports: "The group judgments seem to be a combination of two or more immediate judgments. The group of 3 or 4, and very rarely 5 dots was apprehended as a unit and combined with another similar group. Again the spatial arrangement of the dots seemed of primary importance; some stimulus cards which readily permitted a group judgment in one position offered a decidedly different problem when rotated through 90° or 180°. In every case where the group judgment was given it was arrived at following a definite act of addition. The two or more groups, although exposed for so short a time that eye movement was impossible, registered in a distinctly temporal series. The degree of assurance was frequently, but not necessarily great."

S: "A grouped judgment implied that it was on the basis of more than one group, *e.g.*, a 5 was apprehended as a group of 3 and a group of 2, or of 4 and 1. A larger number was often apprehended in terms of similar groups of familiar number arrangements."

Mc: "The groupings are made in a vertical direction, *i.e.*, the dots which lie under one another are put in the same group. An attempt was made to make vertical groups, and if only one group resulted an 'immediate' report was given; if several groups resulted, a 'grouped' report."

SC: "I found that, on three or four occasions, 5s were apprehended by grouping instead of immediate judgment as they usually come. One of these 5s consisted of an arrangement which grouped 3 and 2. Another came immediately in whatever arrangement or position it was exposed. This one first associated itself in my mind as a funnel, and thereafter appeared as a funnel in the other three positions." He further states: "With grouped judgments I am conscious of exploring the memorial image, each successive group being a trifle less clear than the preceding one; the degree of assurance depending, it seems, upon the clarity of the latter groups—or group."

JC: "As regards grouping judgments it can only be said that they have served as a makeshift in an attempt to observe a number of dots too great for immediate judgment. Consciously no attempt was made to group the dots; and where a grouping judgment was made, it was where the dots fell into natural groups themselves. These judgments, therefore, took on somewhat the same characteristic of the immediate judgments, except that the units were consciously compound, instead of single dots. As the experiment progressed there developed a tendency to count, even those cards on which dots were naturally grouped; and accordingly, as the *O* became efficient with practice, the number and importance of the grouping judgments were greatly decreased."

The calculation of the cognition or grouping results includes those for attention, for the organism is increasing the number of methods for determining the number of stimulus-dots. Only the correct results are calculated; hence, those immediate judgments, or attention, which have been considered, are included in the results for grouping, which is a more complex process for the organism.

The most complex method is one which permits greater freedom on the part of the *O*, and is that of *counting*. The instructions call for the number of dots which can be apprehended; therefore, the judgment in every case need not be necessarily an immediate one. There need not be equal levels of clearness—the

organism is "let go" and "runs wild"—the more that can be apprehended, the larger the range for apprehension. Fernberger[14] has so defined the range of apprehension—that there is not necessarily an immediate response on the part of the *O*, but some subsequent process may be used. The method permits any attentional report, any grouping report, and any counting—any method whatsoever in order correctly to gain a knowledge of the number of stimulus-dots. The introspections also bear this out for apprehension, as they do for attention and for cognition.

M reports: "The judgment of counting was usually based upon a rather indistinct but nevertheless usable immediate memorial image. One group of dots was apprehended immediately. During an intervening period the remaining dots were definitely counted with the assistance of implicit eye-movements of exploration. Since there was no clear image, the assurance of the counted judgments was rarely, if ever, high."

S: ". . . . in counting, either all the dots by the perseverating image are counted one by one, or a group was apprehended as a whole, and then the remaining dots were counted. For example, 8 might be taken as 5 dots, and 1, 2, 3 other dots!"

Mc: "When there are groups, but several single dots left over around the edges which do not fall into groups, I report counting."

SC, who did not use the "counting" report after the third fractionated series, reports as follows: "With counting judgments there seem to appear isolated dots that occur as images which pop out after the grouping is complete—these single dots were of less clarity of outline and blackness than the groups whose apprehension came earlier. Assurance is markedly less in these cases; the greater the number of single dots appearing after the groups, the greater the uncertainty, not only of the dots themselves, but of the group which came earlier—a sort of retroactive loss of recall."

JC: "Introspectively the general habit of observation which repetition forces upon the *O* was one of counting. Resulting in immediate judgments, a secondary process, counting, set in. The grouping judgments became broken up, and a great preponderance of counting judgments was given. In the earlier part of the experiment the attention was concentrated not deliberately, yet consciously, upon the stimulus upon which the eye first fell. Counting was impossible then, where the number of dots was at all large. Accordingly, unconsciously the habit of counting was fixed, and became first noticed when approximately half the judgments were made. The most significant introspection in this process was the original appearance of, and later loss of, negative after-images of the dots themselves. . . . . as the habit of counting became more fixed, the dependence on the after-images decreased, until no after-images at all were recognized in consciousness."

It has been shown experimentally that the systematic category of apprehension has also been properly chosen. The introspections of the *O*s indicate various methods of apprehending the number of stimulus-dots.

II. *Increase in S from attention to cognition to apprehension may be expected, as by definition they are progressively inclusive.* The foregoing definitions of *attention*, *cognition* and *apprehension* indicate that the organism has more liberty in the method of

[14] *Op. cit.*, 133.

determining the number of stimulus-dots: that is, attention permits an immediate report,—one level of clearness and distinctness; cognition may involve several levels of clearness which are also immediate; and apprehension permits the organism to go the limit insofar as methods are concerned,—any combination may be used. The definitions are progressively inclusive and a progression for the values of the thresholds is anticipated from attention to cognition to apprehension. Such is the case of all the *O*s in the experiment. Fig. IV serves to demonstrate this result graphically. It will be observed, as indicated before, that cognition and apprehension are the same for SC for the higher values; he learned to eliminate the counting reports, which accounts for this similarity.

III. *Size of h seems to decrease from attention to apprehension to cognition, on the whole.* An examination of the values of *h* for the three categories for four *O*s (except F and JC) shows the highest values for attention, intermediate values for apprehension, and smallest values for cognition. The order for F is cognition highest, then attention and then apprehension. For JC the order is apprehension, attention, cognition. In accordance with Boring's[15] paper on the stimulus-error, one would have expected that the order would have been attention, cognition, apprehension—inasmuch as introspectively attention is the most univocal process, while apprehension is the process most equivocally determined. The process of apprehension is a uniquely good example of the stimulus-attitude, while both attention and cognition, as defined above, may be classed under process attitude. Hence, insofar as in the case of five *O*s (F, M, S, Mc and SC) the value of *h* for attention is considerably larger than the similar value for apprehension, this hypothesis seems to be correct. The values for cognition, however, do not correspond so well with our general assumptions. In the case of F the value of *h* for cognition is higher than for the other two processes. For M, S, Mc and SC it is the smallest of the *h*-values. For the case of these last four *O*s, it may well be believed that the grouping process, which added to the immediate judgment really determines the form of the curve for the cognition category, was an intermediate sort of process on which the *O*s fell back when attention proper was not sufficient. A proper explanation for the high cognition value of F cannot be offered.

In the case of JC the size of the values of *h* for attention and for cognition corresponds exactly with the relations for the four *O*s discussed above. The high value for apprehension for this *O*, however, cannot be given adequate explanation on the basis

[15]E. G. Boring, The Stimulus-Error, this JOURNAL, 32, 1921, 449-471.

of this hypothesis. A comparison of his curves will indicate for cognition and apprehension (Figg. II and III) that, although the increase in the number of correct judgments is considerable for values up to 8 dots inclusive, there was markedly little increase above 8 dots, and hence the upper tail of his curve falls off very rapidly, giving a relatively steeper inflection to the curve as a whole.

Unfortunately there is a situation similar to, but much less equivocal than, that found by Fernberger with lifted weights.[16]

The results seem to show definitely that the curves for apprehension, which are definitely stimulus-attitude, show considerably less precision than those for attention, the most univocally determined judgments.

Conclusions

(1) The systematic categories "attention," "cognition" and "apprehension," as they appear in the literature, do not correspond with the introspective characterizations of the *O*s in this experiment.

(2) Attention may be characterized as an *immediate* process in which all sensory elements have an *equal* and high degree of clearness.

(3) Cognition may be characterized as an *immediate* process in which *either* the sensory elements have an *equal* clearness value *or* in which the sensory elements appear with *different* degrees of clearness; thus leading to an attentional grouping.

(4) Apprehension may be characterized as the obtaining of a knowledge of the stimulus in terms of *any* sort of mental process by either of the two processes described as attention or cognition, or by a *mediating* process of re-imaging the impression and of counting the elements in this image.

(5) The values of the threshold for our *O*s are greatest in magnitude for apprehension, intermediate for cognition, and smallest for attention.

(6) The values of the coefficient of precision (h) for most of the *O*s are largest for attention, intermediate for apprehension, and smallest for cognition.

(7) Progressive practice seems to affect all three systematic categories, and shows itself as an increase of the value of the threshold as practice continues. Both the absolute and relative increase varies from *O* to *O*, however, as one compares the three systematic categories.

(8) Progressive practice seems to have a variable effect on the magnitude of the coefficient of precision as one compares the values for the different categories for the different *O*s.

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[16] S. W. Fernberger, An Experimental Study of the 'Stimulus Error,' *Journ. Exp. Psychol.*, 4, 1921, 63-76.

THE THEORY OF EINSTEIN AND THE *GESTALT-PSYCHOLOGIE*: A PARALLEL

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The Psychological School of the *Gestalt*, of which the protagonists are Wertheimer, Köhler and Koffka, originated historically with von Ehrenfels,[1] article on *Gestaltqualitäten*, written in 1890, though twenty-two years passed before the appearance of Wertheimer's[2] paper, which seems to have been the scientific starting point of the theory. In this connection it is interesting to note that in 1898 the Erdmann-Dodge monograph on reading anticipated the fundamental contention of the school.[3]

Wertheimer claimed that his experiments with successively exposed visual stimuli, and in particular the occurrence of what he termed the simultaneous phi-phenomenon, which came into being when the time-interval was shortened beyond the limits for phenomenal movement, pointed to the existence of a unified physiological process,[4] which resulted as a specific whole from the excitation of local regions. This cross or total-process Wertheimer insists, is not simply the sum of the local excitations,[5] but is a structure having these as its basis. Following this thought, there has developed the concept of the psychic structure or *Gestalt*, which conforms with von Ehrenfels' criteria, that such a structure cannot be built up out of elements, and that it can be transposed like a melody.[6] Such a Gestalt is not merely a discrete sum,—in Wertheimer's words an "Und-Verbindung",[7] a neologism which I venture to translate as "enumerative-combination." The component parts are not additive, like the soldiers in the Trojan horse, to use a phrase which Plato has made famous in another connection. Köhler has developed

[1] *Vjs. f. wiss. Phil.*, 14, 1890, 248-292. In answer to the objection that the doctrine of the *Gestalt* is nothing but the rather shop-worn theory of *Gestaltqualitäten*, as set forth in this paper of v. Ehrenfels, it is sufficient here to adduce the disclaimers of members of the school, as Koffka, *Psych. Bull.*, 1922, 536; Köhler, *Phys. Gestalten*, 1920, 35 ff. See also the interesting note on the *Gestaltqualitäten* in C. Spearman, *Nature of Intelligence*, 1923, 116, which gives the names involved.

[2] Experimentelle Studien über das Sehen von Bewegung, *Zts. f. Psych.*, 1912, 161-265.

[3] B. Erdmann and R. Dodge, *Untersuchungen über das Lesen*, 1898, Ch. 6.

[4] *Op. cit.*, 251f.

[5] "Sich auf der Grundlage der Einzelerregungen. . . . . .aufbauen."

[6] Von Ehrenfels states that he took the hint from Mach. See E. Mach, *Analyse der Empfindungen*, 1906, 85, for an example.

[7] Untersuchungen zur Lehre von der Gestalt, *Psych. Forschung*, 1921, 47-58.

the conception of non-additiveness by adding the further postulate that the parts of a true structure are actually altered individually when they are combined.[8] Thus the typical form of the stimulus-experience relation is not an elemental sensation,[9] but a structure which is, hypothetically and actually, as immediate as its parts, which latter are not necessarily prior to the whole process. The theory of mental elements or psychological atomism, to use a perhaps dangerous term which is now becoming fashionable, is consequently an unnecessary assumption, depending on a spurious application of the methods of physical science.[10] In contrast to the idea of the mental element, the component members of a psychic structure are really interdependent, each one of them being what it is by virtue of its place in the whole.[11] To reduce consciousness to a single sensation, then, even though this single sensation be the product of analysis, is to adduce a limiting case[12], Wertheimer would argue, and we have no right to draw conclusions hence as to normal states of consciousness. The sensation is then really an abstract artifact, which not only has no basis in observation, but which leads to subtly false conclusions as a product and an instrument of analysis. It is indeed, a false analysis[13] that has set it up as primitive datum, as experience reduced to its lowest terms.[14] Similar criticisms are made against the current use of the terms 'association' and to some extent 'attention', these concepts being supplanted by an ingenious use of the single concept of the unified whole process.[15]

Let us now turn to the other member of the parallel which it is the purpose of this paper to bring out. According to Lord Haldane, "stated generally, the teaching of Einstein is that absolute rest and motion are meaningless for physical science, and that motion can signify only the changing position of bodies relatively to each other."[16] Thus, to take the simplest possible case, that of two isolated bodies in motion, all that we can observe is a change in distance of the two bodies in mutual relation. We have then a dynamic whole progress, with respect to which it would be distorting the facts to claim, for the purpose of calculation or description, that "either of the reference

[8] *Die physischen Gestalten*, 1920, 42. One may compare Aristotle's definition of the 'whole' in the Poetics.

[9] Koffka, Zur Grundlegung der Wahrnehmungspsychologie, *Zts. f. Psych.*, 73, 1915, 57.

[10] Wertheimer, *Psych. Forschung*, 47.

[11] Koffka, *Psych. Bull.*, 543.

[12] *Op. cit.*, 52.

[13] *Ibid.*, 53, note 3.

[14] Compare E. B. Titchener, A Textbook of Psychology, 1915, 50, and also his article, Sensation and System, this JOURNAL, 1915.

[15] See Koffka, *Psych. Bull.*, *passim*; and E. Rubin, *Visuell wahrgenommene Figuren*, 1921, 101.

[16] *The Reign of Relativity*, 55.

bodies is 'unique' as compared with the other."[17] The special theory of Einstein makes this negative postulate for bodies in uniform rectilinear motion, the general theory for bodies in motion of any description.[18] We have for simplicity taken the case of two bodies. The argument can of course be extended to three or more, the differences thus introduced being, for our purposes, of complexity only. Clearly, therefore, it is illogical to argue as to the behavior of these bodies in any way that presupposes a space or a time dependent on any one of the moving bodies, which is what Newton does when he refers the constituent events to a fixed frame of reference and a fixed time system.[18a] And in any case, space, in the ordinary sense of the word, meaning a universally valid space, and time, in the same sense, are not the products of experience. They are "really abstractions from reality." If, then, we cannot hold the assumptions of a single space and time, which is the framework of reference for the whole system, how is it possible to describe the unified whole of nature, of which we are aware? At first sight, the removal of the assumption of a fixed framework seems to lead to chaos. Here was a task for mathematical analysis, to determine whether the events of the system could somehow be described in terms independent of space and time. To his surprise, Lorentz tells us, Einstein succeeded in developing an entirely adequate calculus which "deals with change in which space and time have not yet been discriminated," and by means of this transcendental instrument he recalculated the observable facts of the solar system. The results, according to the relativists, approximate more nearly the actual facts of experience than do the results obtained as a result of the Newtonian assumptions. One may note that, in spite of the apparent novelty of this wide embracing hypothesis, it is said to be only a rediscovery "of ideas which were promulgated by the philosophers of the seventeenth century,"[19] and which were eclipsed by Newton's discovery of the law of gravitation.

[17] A. Einstein, *Theory of Relativity*, 60. English Edition.

[18] *Ibid.*, 61.

[18a] Objection has been made to the statement that Einstein does not make use of a fixed frame of reference. In his *Theory of Relativity* Einstein says (95): "Thus in reality, the description of the time-space continuum by means of Gauss coordinates completely replaces the description with the aid of a body of reference." *Ibid.*, 99: "This non-rigid reference body... might appropriately be termed a 'reference-mollusc'." "The general principle of relativity requires that all these molluscs can be used as reference bodies with equal right and success." And again, 94: "we refer the four dimensional space-time continuum in an arbitrary manner to Gauss coordinates." And further, 73: "[the general principle of relativity] ....the equations of such a theory hold for every body of reference, whatever may be its state of motion." Ipse dixit.

[19] H. Wildon Carr, *Time Space and Material*, 106, in *Problems of Science and Philosophy* (*Aristotelian Society*), 1919.

Between these two theories, that of the German physicist and the German psychologist, there stands out an immediate parallel. Each strikes at the discreteness of the cosmos, at the local autonomy respectively of the physical and the psychic. Each begins with a negative, destructive attack,—sensations are not independent, velocities and spaces and times are not absolute. True it is that, as already indicated, the notion of a cosmic interdependence was not new. But the two theories claim to have carried the idea much further than ever before.[20] Thus, while we have Newton claiming the gravitational interdependence of physical matter, we find also a school of philosophers declaring an associational interdependence of psychic material, and indeed the parallel between these two modes of thought was brought out by Hume in a famous passage. "Here is a kind of Attraction," he says, "which in the mental field will be found to have as extraordinary effects as in the natural."[21] It is not without significance to find the father of the British Association school, Hartley, acknowledging his indebtedness to the great physicist of his day.[22] And exactly as the results of Newton are now said to be only approximations which modern technique has transcended, so we find Wertheimer saying that "only seldom,. . . . only within narrow limits and perhaps in any case *only in approximation*, does the principle of enumerative combination hold in psychology."[23]

Things that are not independent cannot be added together arithmetically.[24] For addition we must posit a discreteness which has been denied to its data by each of the two theories under discussion. A father and a son, as such, cannot be added together to make two of anything. Only when they are considered outside of their relationship, that is as men or as human beings, can we say they make two. If two drops of water are placed in sufficiently close contact, they lose their twoness. This fact has been exemplified in each of the two theories under discussion where, in each case, the non-independence of the data has been found to imply a non-additiveness. "The theorem of the addition of velocities employed in classical mechanics cannot be maintained," declares Einstein.[25] In exactly the same way, the parts of the *Gestalt* are declared not to be summative. They cannot be added together to make the *Gestalt*.[26]

[20]Compare Köhler, *Physische Gestalten*, 153 on the danger of likening the *Gestalt* with the philosophic unity of the world.

[21]*Treatise on Human Nature*, Part 1, sec. 1.

[22]D. Hartley, *Observations on Man*, ch. 1, Introduction.

[23]*Unt. z. Lehre v. d. Gestalt*, 52.

[24]*Reservatis reservandis*.

[25]*Theory of Relativity*, 16.

[26]Köhler, *Phys. Gestalten*, Introduction, p. xviii; and cf. Wertheimer and Koffka, *passim*. The thought originated with von Ehrenfels.

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While the idea is central for the *Gestalt*-theory, yet it has remained at the negative stage longer than in the theory of Einstein. The latter has adopted the Lorentz transformation to explain the combination of velocities; but the advocates of the *Gestalt* do not seem to have developed any such specific law for the combination of mental data.[27]

The antithesis of this concept of the additive element, which is denied so vigorously by both the theory of the *Gestalt* and the theory of Einstein, is a whole which cannot be built up out of its parts. A theory which stresses the inapplicability of the conception of the arithmetic sum as the mode of combination of its data is therefore likely to emphasise the fact that it treats wholes of one kind or another. This can be seen to be a part of the relativist's theory; for, to go back to our fundamental illustration, Einstein states that the sole dynamical datum is two or more bodies moving relatively to each other.[28] Put in another way, this means that the original datum is two moving bodies, to take the simplest case, considered together, not either one considered by itself or absolutely. After all, that is what we mean by the statement that motion is relative. Thus we find Whitehead, in a remarkable passage,[29] asserting in criticism of the older physical theory that "awareness of nature begins in awareness of a whole which is present", and "this awareness of the whole is directly sensed, and is not a detailed discrimination of its parts." Could one ask for a statement better illustrating the fundamental similarity of the two theories? It would, indeed, be difficult to decide at a guess from which of the two it comes. Certain it is that the sentence could be matched many times over by almost precisely similar words from the school of Köhler. As an example on the psychological side one may take the statement of Wertheimer that "what is given consists of more or less fundamentally structural, more or less definite wholes and whole processes."[30] Almost equally remarkable in its parallelism is the statement of Koffka that "the members of the structure are what they are by virtue of their place in the whole."[31] Could a better statement be found than this of a psychological theory of relativity?

An excellent example of the idea of the "whole-process" as used by the school of the *Gestalt* is to be found in the work on

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[27]There are the laws of leveling and assimilation, mentioned by Koffka, *Psych. Bull.*, 545 (*Prazisierung* and *Nivellierung:* F. Wulf, *Psych. Forschung*, 1922, 341). These are considered special cases of Köhler's law of pregnancy; see Koffka, *l.c.*, and Köhler, *Phys. Gestalten*, 348 ff. There are also hints of laws developed and expounded in Wertheimer's academic lectures.

[28]Haldane, *l.c.*

[29]*Time, Space and Material*, 46f.

[30]*Untersuchungen*, 52.

[31]*Psych. Bull.*, 543.

the psychology of comparison.[32] Pikler found when two successive stimuli were being compared for intensity, the judgement of comparison never followed the apprehension of the second stimulus but was always simultaneous with it. This points the way to the position taken by Köhler, who maintains that we cannot properly speak of two sensations which are experienced and compared, but that our comparing experience is the apprehension of two members of a whole with a certain tension between them.[33] This writer has, in fact, devoted a whole monograph to showing that hens and chimpanzees react to pairs of colours as to whole structures and not as to two, individual, separable sensations, and has repeated his experiments on a three-year-old child.[34] Throughout the monograph he attempts to disprove the theory of what he calls the "absolute" sense of the stimulus in favour of the structural, relative sense.

There are other parallels between the two theories. Of these, perhaps one may mention that both theories claim that the results of their predecessors have been built up by a postulate of abstractions, the sensation and space and time respectively. Each claims that its analysis conforms better with what is actually observed. Each claims to have united under one common explanation facts that previous theories regarded as disconnected anomalies, and thus to have made necessary fewer laws and fewer postulates. Other points of similarity could be brought up, but they would be, as are these, merely symptoms of a general agreement, which springs from the fact that the two theories, in essence, show a fundamentally similar attack by the human mind upon two radically different fields of experience.

One may thus sum up what seems to the writer a remarkable chapter in the history of science. In the fields of physical and psychological science there have lately appeared, under the names of the Einstein and the *Gestalt* theories, innovations which, unnoticed, at least in print, by both sets of workers, appear to lead by fundamentally similar methods of attack to fundamentally similar conclusions. In each case the attack is headed against an older theory whose postulate is discreteness of data, and in each case there is substituted a postulate of the primary interdependence of data, implying the necessity of a relative rather than an absolute treatment. This common point of departure results in parallelisms, of which examples are the non-additiveness of data and the insistence upon whole processes rather than parts.

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[32] J. Pikler, *Zts. f. Psych.*, 67, 1913, 277.

[33] Nachweis einfacher Structurfunktionen beim Schimpansen und beim Haushuhn, *Abh. d. Preuss. Akad. d. Wiss.*, 1918, 14.

[34] *Ibid.*, 45.

Although, and this is perhaps inevitable from the nature of the subject matter, psychology lags infinitely behind the exact and powerful mathematical technique of the physical sciences, yet innovations in the two branches of knowledge seem to be moving along parallel paths.

TONAL VOLUME AS A FUNCTION OF INTENSITY

By H. M. Halverson, University of Maine

Diotically perceived tones vary in their volumic aspects with changes in pitch. Rich[1] has determined the precise nature of these variations, and concludes that within the limits of 100 vs. to 800 vs. "the relative difference limen of tonal volume is approximately constant." In a recent paper[2] the writer found that diotic tones as perceived through closed tubes vary in volume as the phase relationship between the tones is altered. Tones of median and extreme lateral localization have greater volume than tones in intermediate positions, when no differences of pitch are discernible. It is generally believed[3] that intensive changes accompany changes of phase; therefore the question which naturally arises concerns itself with the relation of volume to intensity, and it is the purpose of this paper to discuss this relationship from the evidence obtained from the following experiments.

The apparatus consisted of a telephone receiver which was introduced into the secondary circuit of an audio-oscillator of 1000 cycles, enclosed in an approximately sound-proof box, which served as the source of sound. The oscillator was energized by a 6-volt storage battery which was recharged at regular intervals as long as the experimentation continued. A telegraph key and slide rheostat, upon which had been pasted a mm. scale, were inserted into the secondary circuit. A shift of the rheostat slide increased or decreased the current through the receiver and resulted in corresponding alternations in the intensity of the tone of the receiver. The telegraph key served to open and close the secondary circuit. Zero on the rheostat scale corresponded with maximal intensity, and 233 on the scale with minimal intensity, of the stimulus.

The *Os* were Dr. W. S. Taylor (T), a member of the faculty of the University of Maine, and Mr. H. E. Pressey (P) and Miss Helen C. Burton (B), students of the same university. T and P were trained musicians; B

[1]G. J. Rich, *J. Exp. Psychol.*, 1, 1916, 22.

[2]H. M. Halverson, this Journal, 33, 1922, 526-534.

[3]See A. H. Pierce, *Studies in Auditory and Visual Space Perception*, 1901, 127; C. E. Ferree and R. Collins, this Journal, 22, 1911, 250-297; and Halverson, *ibid.*, 33, 1922, 178-212.

had had little musical training and was not particularly musical. None of the *Os* had had experience in auditory experimentation, and under the conditions of the experiment this lack of experience might be considered an advantage. T gave some trouble in the first few settings. Differences of volume for all these *Os* were dependent upon differences of pitch. T, however, was unable to say just what he meant by volume, except that he had always believed that changes of pitch of tones connoted changes of volume. P and B gave little or no trouble throughout the entire series of observations.

The first step in the experimentation[4] was to train the *Os* to make volumic judgments. In order to accomplish this purpose the *Os* were required to make volumic judgments after the manner described by the writer in an earlier paper.[5] The average limen for the *Os* at 396 dv. was 18.1 dv., with m.v. of 1.8 dv. T, who was not on the ground at the time of this preliminary experimentation, was required to make volumic judgments with piano tones under the method of constant stimuli. C (256 dv.) was used as the standard tone, and the five next lower tones, B, B♭, A, A♭, and G were the comparison stimuli. P and B took part in this preliminary experiment also. This work came to a close when the *O's* judgments became consistently uniform. The average limen was less than the difference between the standard and the first comparison stimulus; hence no attempt was made to compute it.

In the principal experiment the *O* was seated so that the receiver was about 2 ft. behind him in the median plane at ear level. The purpose was to determine precisely the direction of the changes in volume as the tone was altered from maximal intensity (position 0 on rheostat scale) to minimal intensity. With the maximal intensity as standard, the procedure consisted in determining the limen for the category "smaller" by the method of constant stimuli. The instructions were to report "larger", "equal", or "smaller". The first set of limens for the three *Os* was averaged and used as the new standard, and in this way the entire volumic range was covered. The first standard and the four resulting limens are indicated in Fig. 1. An effort was made to find a fifth limen; but the sound at this position of the scale was too weak, and the outside auditory distractions were relatively too great, to permit of dependable judgments; hence the experimentation for volume was brought to a close.

The limens shown in Table I were computed by linear interpolation between the two relative frequencies adjacent to the 50% point. It is apparent that the change of volume for change

[4]The Stern variators used in this experiment were lent by the Clark Psychological Laboratory. We take this opportunity to thank Dr. E. C. Sanford for this courtesy. We wish also to express our thanks to Dr. A. L. Fitch of the University of Maine for extending to us the use of the physical laboratory.

[5]*Op. cit.*, 527.

TABLE I

Psychometric functions of volumic differences for intensive differences of diotically presented tones of 1000 dv. Relative frequencies based on 120 judgments for each comparison stimulus

Series and standard stimulus	Os	Range of 5 equally separated comparison stimuli	Relative frequencies of volumic differences under category "smaller" Successive comparison stimuli in increasing order of intensive differences					Limen by linear interpolation	Average limen
mm.	Os	mm.	1	2	3	4	5	mm.	mm.
I	T	20–100	15	53	87	111	118	44	
0.0	P	30–150	12	29	50	69	97	106	77
	B	30–150	20	41	67	92	112	82	
II	T	89–137	24	33	66	89	119	111	
77	P	97–177	3	23	47	95	106	141	131
	B	97–177	3	26	53	100	117	140	
III	T	140–176	18	33	58	107	113	158	
131	P	146–206	4	18	61	116	118	176	172
	B	146–206	3	12	39	101	117	181	
IV	T	179–207	12	31	60	116	120	193	
172	P	179–207	4	11	46	104	118	195	198
	B	182–222	6	17	44	106	119	205	

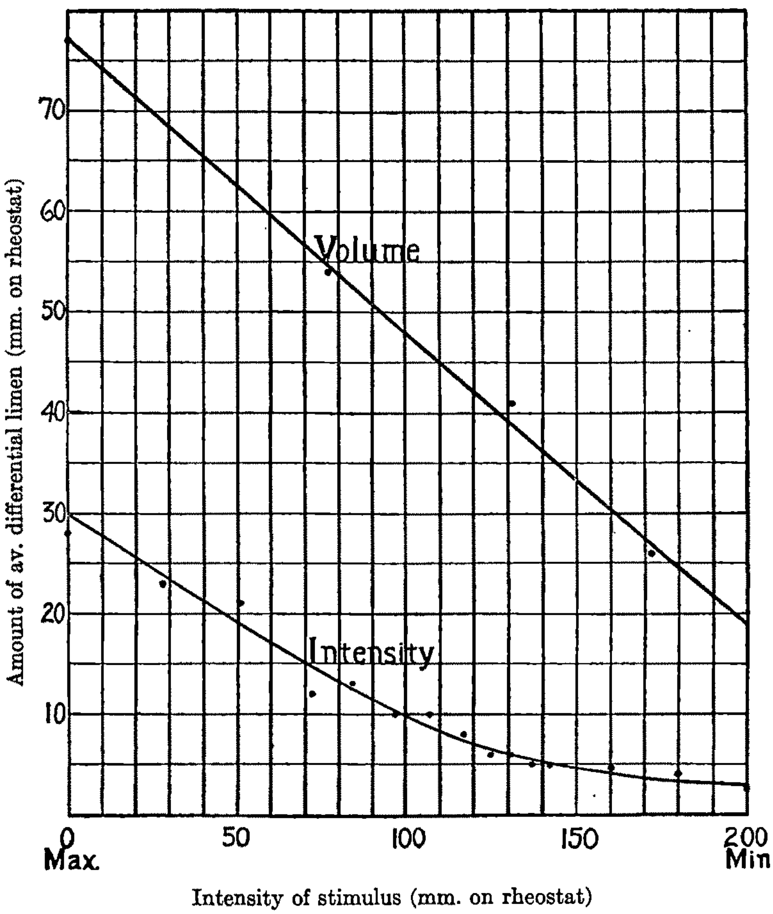
of intensity of stimulus is regular. While no limen was determined at 198 on the mm. scale of the rheostat, the limen at this position can be fairly accurately obtained by plotting the value of the other limens against the volume on the rheostat scale, and drawing in by eye the best fitting curve.

Fig. 1 presents graphically the data of Table I. The upper dots and curve are the limens and the best fitting line of the limens for volume. The limens are plotted on the ordinates against volume which is represented by the abscissa. If we assume continuity of function, the limen for volume by linear interpolation lies between those stimulus values for the relative frequencies that include 50%. The successive liminal values for volume thus obtained are read from the ordinate scale. For example, the limen at 0, maximal volume, is 77, and the other limens in order are 54, 41, 26 and 20; the last limen is read from the curve at 198, which is the fifth and last standard used in the method of constant stimuli. The limens herein indicated are the average limens for the three *O*s, and each was in turn taken as the standard for the succeeding series. On the basis of these results we may accept the smoothed curve as representing in general the form and the amount of the volumic function.

Inspection of Table I reveals that T's limens for volume are throughout the entire series generally smaller than those of either P or B, and that the increments of his comparison stimuli are less than those of P and B, except in the case of the

last series. The results for P and B compare favorably throughout the experiment, except in the series where 172 served as the standard stimulus, in which case the sound was relatively weak and B was especially susceptible to auditory distractions.

FIG. I



Average liminal values of the three *Os* for volume and for intensity with decrease in tonal energy from 0 to 200 on the rheostat scale. The dots represent the values obtained through experimentation and the curves are approximately the best fitting lines of the liminal values. The upper line is the curve of volumic limens; the lower line the curve of intensive limens.

All *O*s agree that there is a gradual diminution in the volume of the tone as its intensity is reduced. For T greater volume means that the tone "looms up more in consciousness like something which is expanding rapidly," that the tone is "bigger," "occupies more of my field of consciousness or mental horizon." For P greater volume means "larger in height, width and breadth," that the tone "felt larger in the ears and spread out more over the temples," and that it "covers more area on the head." B made her judgments in terms of visual imagery. All tones appear as funnel-shaped clouds emanating from a point in the space before her; the wider the mouth of the funnel, the greater the volume of the tone. Tones of least volume are mere streaks of smoke. Of these *O*s T was the only one who was at all influenced by the presence of intensive differences of the tones. Upon one occasion, after a judgment on volume had been rendered by B, she was asked which of the two tones just presented had the greater intensity. Her naive response was, "I didn't notice."

The next step was to determine the function for intensity for the intensive range just covered for volumic differences, and to note just how this function compares with the volumic function. The method of procedure was similar to that just described for finding volumic limens. With the stimulus zero on the rheostat scale as the first standard, the limen for the category "weaker" was determined by the method of constant stimuli. The instructions were to report "stronger", "equal", or "weaker," although, of course, as for volume, no stronger (greater) could be obtained under the conditions of the experiment. When the limens were averaged for the three *O*s, this average was used as the standard for finding the next limen, and in this way the range for intensity was stepped off to 147 mm. of the rheostat scale (see Table II). At this point another method of procedure was adopted. Instead of finding the successive limens in regular order, *E* found limens at three points about equally spaced on the scale, namely, at 160, 180 and at 200. These limens with those already determined were then plotted, and the best fitting curve was applied to the entire constellation of limens (see Fig. 1, the lower dots and curve). The limens between 147 and 200 were now interpolated from the curve in the following manner. The last limen (5) of the series of successive "weaker" limens was obtained at 142. The new standard for the next series would then be 147. According to the curve the limen at 147 is 5. If we add this number to 147, the next standard is 152. The limen at 152 as shown by the

TABLE II

Psychometric functions of intensive differences of diotically presented tones of 1000 dv. Relative frequencies based on 120 judgments for each comparison stimulus

Series and standard stimulus		Range of 5 equally separated comparison stimuli	Relative frequencies of volumic differences under category "less" Successive comparison stimuli in increasing order of intensive differences					Limen by linear interpolation	Average limen
mm.	Os	mm.	1	2	3	4	5	mm.	mm.
I	T	10–50	15	40	76	107	114	25	
0.0	P	10–50	10	34	67	82	96	28	28
	B	12–60	21	36	70	86	99	32	
II	T	38–78	17	36	75	97	119	54	
28	P	35–63	17	30	61	91	101	49	51
	B	35–63	27	42	53	92	97	50	
III	T	56–76	14	39	73	78	95	64	
51	P	58–86	15	31	52	71	102	75	72
	B	58–86	16	28	44	70	90	76	
IV	T	77–97	19	50	85	98	109	83	
72	P	77–97	13	43	84	102	110	84	84
	B	77–97	32	37	84	94	103	84	
V	T	89–109	23	42	76	103	106	96	
84	P	89–109	13	25	70	101	106	98	97
	B	89–109	24	36	71	81	99	97	
VI	T	102–122	30	53	96	108	118	108	
97	P	101–117	24	52	75	98	116	106	107
	B	101–117	15	41	62	90	110	107	
VII	T	111–127	27	47	82	93	112	117	
107	P	111–127	27	53	91	107	118	116	117
	B	111–127	31	41	71	90	108	117	
VIII	T	121–137	32	59	89	104	116	125	
117	P	120–132	19	49	79	102	116	124	125
	B	121–137	34	52	89	97	112	126	
IX	T	128–140	26	45	78	96	104	133	
125	P	127–135	27	56	81	101	115	129	131
	B	127–135	34	42	56	78	89	131	
X	T	134–146	35	55	80	93	109	137	
131	P	132–136	20	32	46	58	91	135	137
	B	133–141	22	27	42	87	102	138	
XI	T	139–147	25	42	72	92	110	142	
137	P	138–142	35	40	50	82	99	140	142
	B	139–147	25	26	38	94	94	144	
XII	T	144–152	26	47	70	76	93	147	
142	P	143–147	34	43	58	83	102	145	147
	B	144–152	29	32	42	58	85	150	
XIII	T	162–170	29	44	72	92	102	165	
160	P	161–165	22	47	68	77	97	163	165
	B	162–170	40	41	54	74	92	167	
XIV	T	182–190	31	58	80	94	110	184	
180	P	181–185	50	47	57	88	98	183	184
	B	182–190	34	47	71	77	92	185	
XV	T	201–205	29	42	59	85	104	203	
200	P	201–205	34	54	76	94	109	202	202.7
	B	201–205	25	44	57	75	95	203	

curve was found, and in this way all the remaining standards and limens were estimated. The interpolated standards and limens follow.

Standard	Limen	Standard	Limen
147	5	177.2	3.6
152	4.8	180.8	3.5
156.8	4.5	184.3	3.4
161.3	4.2	187.7	3.4
165.5	4.0	191.1	3.3
169.5	3.9	194.4	3.2
173.4	3.8	197.6	3.2

The number of intensive limens for the entire range of intensity totals 26 as against 5 limens for volume over the same range.

The lower function of Fig. 1 shows the limens for intensity, and the smooth curve approximately is the best fitting line of the dots. By limen is here meant the average limen for all three *O*s. A study of the curve reveals that, as the stimulus diminishes from zero to 100 on the arbitrary scale, the regularity in decrease of intensive liminal values gives a straight line, a relation which indicates a logarithmic relation between psychological intensity and the degree of the stimulus. From 100 to 200 liminal values no longer yield a straight line, but lie relatively closer together as the stimulus decreases to the minimum. The dots represent those liminal values which were actually obtained by experiment.

The Bernstein theory of localization as extended by Boring,[6] which suggested the writer's earlier experiment[7] on diotic tonal volumes, implies that intensity and volume are covariants; *i.e.*, an increase or decrease in the intensity of a tone would be accompanied by a corresponding increase or decrease in tonal volume. The results of the present study indicate that such relationship holds.

Guernsey[8] found that Weber's Law obtains "with a fraction of about one-third throughout the middle range of intensities." Our figures show that Weber's Law applies for tones of ordinary intensities (0 to 100 on the scale) with a fraction of about one-fifth, but that the law does not hold for weak tones. In the case of volume Weber's Law apparently obtains with a fraction of about one-fourth, provided we assume that the best fitting curve of our volumic limens is a straight line.

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[6]E. G. Boring, *Quart. J. Exp. Physiol.*, 10, 1916, 86-94.

[7]*Op. cit.*

[8]M. Guernsey, this Journal, 33, 1922, 567f.

Rich[9] has shown that Weber's Law holds for volume as a function of vibration-rate, and our results indicate that the same law holds apparently for volume as a function of intensity.

The curves and introspective reports for volume both demonstrate that volume is dispersion. In saying that volume is a function of frequency we assume constancy of intensity; otherwise intensive differences might have accounted for volumic differences. It is probable, however, that intensity did not vary greatly in the experiments upon frequency and volume. If we follow the extension of the Bernstein theory, volume of necessity varies directly as intensity varies. The intensity of a tone depends essentially on the energy of the stimulus; and it is readily intelligible, if we keep in mind the above theory, that the stronger the impulse, the greater will be the nervous 'displacement', and therefore the greater the central dispersion.

Conclusion

Diotically perceived tones vary directly in their volumic aspect with differences of intensity. These volumic changes are of the same nature as the volumic changes which accompany changes of pitch. Since the volumic limen is much larger than the intensive limen, the two aspects, thus determined, are independently variable and must represent separate attributive dimensions. At medium intensities changes of intensity occur at a relatively slow rate and are accompanied by very much slower changes of volume. With weak tones intensive changes occur at a gradually increasing rate, whereas there is no apparent increase in the rate of change of volume.

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[9] *Op. cit.*, 22.

A STUDY OF THE "CAUTION" FACTOR AND ITS IMPORTANCE IN INTELLIGENCE TEST PERFORMANCE

By WILLIAM M. BROWN, Washington and Lee University

A careful study of the various kinds of scores given in the Thorndike "Intelligence Examination for High School Graduates" (which was in the present instance taken as a sample of current intelligence tests) would seem to lead to the conclusion that much more significance is to be attached to the wrong answers made by an individual, who is tested, than would at first sight appear. Heretofore, the *plus* scores have been most emphasized in all examinations of this character, and present practice in scoring leads in most cases to the utter disregard of *minus* scores. Where chance seems to play a large part (as in "plus-minus" or "true-false" tests) the common method of computing the score is to subtract the number wrong from the number right (R−W).

Thorndike seems to have been among the first to recognize that this formula does not completely evaluate the wrong answers, and he accordingly provides for a more elaborate method of scoring his intelligence examination and a corresponding variety of situations with which the individual is confronted. Other devisers of tests have yet to follow his example and, for the most part, the question of the scoring and interpretation of wrong answers in intelligence examinations remains unanswered. Certain it is, however, that these reactions of an individual possess as much significance in their way as do the answers which are scored correct. The difficulty lies in finding the objective means for placing an estimate on these various types of response.

In making the present study the writer lays no claim to originality of method or of interpretation. The attempt has been made merely to substantiate the hypothesis above mentioned and to discover at least some of the ways in which wrong answers on intelligence tests may be evaluated. The question of the further interpretation of correct answers and of items not tried has not been undertaken, but it is hoped that this too will be studied in the not too distant future by others who are interested in the matter.

Character and the "Caution" Factor

Ordinarily the terms "cautious" and "rash" are used to designate persons of more or less opposite types. If we assume the presence of "caution" as a trait of human personality, we may say that a person, in whom this factor is dominant, is of the "cautious", "careful", or "conservative" type, while one who tends to the opposite extreme is commonly called "rash"

or "careless." There seems to be no reason to believe that these are not entirely valid distinctions, and, while they differ from each other rather in degree than in kind, they nevertheless represent more or less definite concepts in each individual mind. It may also be added that these concepts are usually thought of as belonging to the domain of character rather than to that of intelligence.

Granted the existence of such a trait as we have described, we can, at least from an abstract point of view, treat it quantitatively by measuring its presence or absence in a given individual by the use of a percentage scale. On this scale the 100 per cent. point may be taken to represent that point at which would be placed that member of a given group showing the greatest tendency to be "cautious", *i. e.*, the most conservative member of the group. The most "rash" (or least conservative) individual would then be given a rating of zero, and the remaining members of the group would be distributed in their proper order between these two extremes. With such a distribution, those persons at the upper end of the scale would be designated as "cautious" or "careful", or at least fairly so, while those at the lower end would comprise the individuals whom we ordinarily know as "rash". It is altogether probable that, if a large enough group were taken as a sample, our distribution would follow that of the "normal curve". The majority of the group would then be expected to fall within the middle range of the scale, *i. e.*, between the 25 per cent. and the 75 per cent. points, and to be more nearly typical of the average person, who is, in the long run, neither over-cautious nor exceedingly rash.

Our present limited means for the investigation of character traits on a scientific basis make it almost an impossibility to undertake a further analysis of the trait which we have just described. It may or may not be related to intelligence, though the former is most probably the case, since there are times when it is necessary for us to know, not only *when*, but *how* to be cautious. Again, the caution factor may be complex or it may be comparatively simple. It may form a part of a "hierarchy"[1] of traits, or it may be comparatively isolated. In any event the "caution factor" seems to be the simplest term we can apply to it, and, though this falls somewhat short of any adequate description of the trait or its implications, it seems advisable in the present study to use this term for want of a better one.

There is probably not much difference between the "caution factor", as used here, and "sagacity", which James[2] calls the "perception of essence", "the ability to extract characters—not *any* characters, but the right characters". This he distinguishes

[1]B. Hart and C. Spearman, General Ability: Its Existence and Nature, *Brit. Journ. Psychol.*, 5, 1912, 51-79.

[2]W. James, *Principles of Psychology*, ii., 1890, 331.

from learning, which he calls "the ability to recall promptly consequences, concomitants, or implications". Following James, Hollingworth[3] in speaking of stimulus and response says:

"If the detail which occurs is what we commonly call a significant part (of the stimulus), the response is a useful perceptual reaction. The more irrelevant the detail responded to, the more lacking in sagacity, and hence the more psychoneurotic is the individual to be considered."

"Sagacity is, then, the ability to comprehend properly the part in its relation to the whole and to discriminate out of a whole the appropriate, relevant, or significant details. Failure in sagacity will thus imply a disposition to react to a present total situation by singling out *some* detail of it and reacting to this detail by *some* total reaction previously associated with a whole in which the detail figured as an item. This is the mechanism of the psychoneuroses."

One more question needs to be dealt with briefly, namely: Under what circumstances does this factor, if present in an individual, make its appearance? No well-defined situation may be described in answer to this question and regarded as a typical one. Generally speaking, lack of knowledge, either of facts or of method of procedure, coupled with other elements which make the situation an important one, such as the necessity for speed, the urgency of the occasion, and the character of the probable outcome, furnishes a sufficient stimulus for the activity of the caution factor. It will be observed, moreover, that a situation similar to the one just described tends to have exactly the opposite effect on the "careless" or "rash" individual, who either lacks the caution trait or successfully inhibits its action—it is difficult to say which of the two actually occurs. If there is such a thing as "resistance to the tendency to be cautious," this would operate parallel to the caution factor itself, both being mutually inhibitory, as two pairs of muscles, *e. g.*, the flexors and the extensors, in the human body.

It is, of course, quite possible that the caution factor is present in situations which are not at all critical. Our problem is not so much the determining of the nature of the stimulus which arouses it, as the study of its effects upon the performance of an individual in whom it is present as contrasted with that of one in whom it is either lacking or for the time being inhibited by some other factor or group of factors.

Character of the Groups Studied

In order to make the investigation as comprehensive as possible, it was thought best to select for study three groups differing considerably in respect to environment and previous training. All of the members of these groups had previously been tested by the use of one or more of the standard intelligence tests, and the test papers were in every case carefully

[3]H. L. Hollingworth, *Psychology of Functional Neuroses*, 1920, 21.

studied for evidences of the presence or absence of the caution factor in the individual's performance. In Groups I and II the attempt was made to study the effect of this factor on scholarship, while in the case of Group III the methods and results obtained from the investigation of the other groups were applied to a number of widely different types of individuals.

Group I consisted of 375 students in Columbia College, who were admitted in September, 1919, and had remained in residence for at least one semester. These students were a part of a still larger group of 505 candidates for admission, who took the Throndike intelligence examination at the same time. Some, however, failed to attain the required intelligence rating, and others could not be included in the investigation because of incomplete scholastic records or unavailability of their intelligence examination papers. No records of women students were studied, and the ages of the individuals in the group were those of the average entering class in most colleges and universities.

Group II was composed of 193 children—147 boys and 46 girls—from Public School No. 192, New York City. This school is operated in connection with the Hebrew Orphan Asylum and is attended only by the children in that institution. The individuals studied ranged in age from 10 to 14 years and had all been given the Stanford revision of the Binet-Simon test. Some of them had also been tested with the Otis group test. The results of these tests were studied in relation to both the scholastic performance and the conduct of the children composing the group.

Group III was made up of 166 men who passed through the United States Army General Hospital No. 30, at Plattsburg Barracks, New York, during the years 1918 and 1919.[4] These were all abnormal cases, ranging in chronological age from 18 to 41, and most of them had been in active service with the American army in Europe during the World War. On the basis of medical diagnosis, these individuals may be classified as follows:

Psychoneurotics (not further specified)	68
Epileptics	32
Hysterics	21
Psychoneurotics (syphilitic)	18
Mentally Deficient	17
Psychasthenics	5
Exhaustion Neurotics	5
Total	166

[4]The data for this group were made accessible to the writer by the kindness of Professor H. L. Hollingworth, of Barnard College, Columbia University.

The last four groups include all the available cases in these classifications, and hence the small number included in them. All of these individuals were required to take certain tests, of which the Trabue completion test was selected for study in the present investigation. The introduction of this group was for the purpose of testing the application of certain facts derived from the study of Groups I and II, and hence it was dealt with on a somewhat different basis from that used in the other groups.

Method and Procedure

In the case of all these groups the wrong answers made on the intelligence examination were taken as indicative of the presence or absence of the caution factor. For, the giving of an incorrect answer to any question, whatever else it may indicate, shows that the individual did *not* know the right answer or was unable to recall it at the moment, and yet he gave an answer of some sort, which in this case proved to be wrong. When we consider the fact that many of the answers, which were scored right, must have likewise involved the element of guessing, the reasonableness of our assumption regarding the interpretation of the wrong answers becomes more evident. For, on the mathematical theory of chance, in a long series of guesses just as many answers are likely to be correct as incorrect. Thus, the full significance of the caution factor and the part which it plays in any given case cannot be arrived at except by an additional investigation of the number of guesses in the correct as well as in the incorrect answers. Because of our present very limited means for this purpose the study of this phase of the matter is necessarily excluded.

In substantiation of the above observation on the presence of guesses in right answers may be mentioned the work of Fullerton and Cattell [5], who compelled their subjects to guess in deciding which was the heavier of two weights when the difference was so slight as to be imperceptible by ordinary observational processes. They found that, under these conditions, the subject achieved an average of 71 per cent. of right answers instead of the conventional 50 per cent. Similar results have been found by other investigators.[6] Hence, a comprehensive study of the caution factor, taking into account caution as displayed in the right as well as the wrong answers, would probably serve to emphasize the results which we have here obtained from an investigation of caution as connected with the wrong answers alone.

If a large number of wrong answers on the usual intelligence examination may be taken as indicative of the absence of caution, this is even more true of the *minus* and zero scores on the Thorndike intelligence examination. For those individuals who took the latter test, only those parts were studied where the

[5]G. S. Fullerton and J. McK. Cattell, On the Perception of Small Differences, *Univ. of Pa. Publications, Phil. Series,* No. 2, May, 1892, 127.

[6]E. L. Larson, in an unpublished investigation made at Teachers College, found approximately the same results.

subject was warned in the test directions that a wrong answer would count off from his score.[7] It will be seen, therefore, that in six of the tests in Part II and all of those in Part III any guessing which is indulged in must be more or less deliberate and may be taken as satisfactory evidence that the caution factor was absent in that particular case, in fact, even more so than under the usual test conditions.

It may be objected that, in many instances, the person taking the examination gives answers which he believes to be right. That is doubtless often the case; but the person in whom the caution factor is operating at its maximum would most probably refuse to write down any answer of which he was not absolutely sure. Lack of the knowledge that he is right, even when he has a high degree of confidence in his answer, would tend to make the extremely cautious individual omit the item altogether, while a less cautious person would in many cases hazard a guess. It has, of course, been found that the greater percentage of correct answers normally accompanies the higher degrees of confidence[8], but this rule is not without its exceptions.

It will be recalled that Thorndike, in the parts of his intelligence examination used for this study, provides for the giving of zero as well as *minus* scores. Any answer is scored zero when it is incorrect and yet not in the class of foolish and irrevelant answers. Since a zero answer is for all practical purposes a wrong answer, it was thought entirely proper to estimate the presence or absence of the caution factor on the basis of the total number of both *minus* and zero scores combined made by any individual in those parts of the Thorndike intelligence examination which were included in the study. Thus, if a person made 15 *minus* scores and 7 zero scores in the examination, he was credited with 22 wrong answers, and this figure was taken as the "caution index" for the members of Group I without any further attempt at analysis.

For the individuals of Group II the performance on the Binet test was studied for evidences of the operation of the caution factor. It was found that the vocabulary test alone proved satisfactory for this purpose, as it was the only test

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[7]For a detailed description of these parts of the Thorndike intelligence examination the reader is referred to B. D. Wood, *Measurement in Higher Education*, 1923.

[8]For further information on this point see Fullerton and Cattell, *op. cit.*; L. J. Martin and G. E. Mueller, *Zur Analyse der Unterschiedsempfindlichkeit*, 1899; G. F. Williamson, Individual Differences in Belief Measured and Expressed by Degrees of Confidence, *Journ. Phil. Psych. Sci. Method*, 12, 1915, 127-137; E. K. Strong, Jr., The Effect of Length of Series upon Recognition Memory, *Psych. Rev.*, 19, 1912, 447-462.

which was actually performed by every one of the 193 children tested. It was also found difficult to make any estimate as to the caution factor in the answers for many of the other tests because of the wide latitude allowable in some places (*e.g.*, in such tests as the ball in the field, drawing a diamond, etc.). Therefore, for the sake of uniformity and for other reasons, the vocabulary test alone was included in the investigation. Here, since there were specified limits for the different age levels, the number of wrong answers had to be considered in relation to the total number of words given to the child to define. Some children were taken through the entire list, others were given as few as 10 or 15 words. It is obvious from these facts that the only way of comparing the various individuals for the caution factor is in terms of the percentage of wrong answers out of the total number possible. Thus, if a child were asked to define 20 words, with a total of 10 correct, 5 wrong, and 5 not tried, his caution index was determined as 25 (*i. e.*, he made a total of 25 per cent. wrong responses). Another individual, however, making only 5 incorrect responses out of a total of 50 possibilities would be credited with a caution index of 10. By this method, of course, the person with the smaller index is regarded as having shown the larger degree of caution, and conversely.

Of the children in Group II, there were 88 who also took the Otis group test. In these cases the caution index was taken as equal to the total number of wrong answers made by the child, since the number of possibilities was the same in every case. Reducing these results to a percentage basis would not have changed the final outcome in any way.

For Group III the Trabue completion test was studied for evidences of the author's factor. Both of the preceding groups were fairly homogeneous and showed mental ages such as would put them all in the "normal" class. But in the third group, composed of men who showed various kinds of mental abnormalities, the range of the mental ages and the I. Q.'s was too great to permit of the treatment of the group as a homogeneous one. Furthermore, the variety and amount of material to be found in the Trabue completion test made it too limited in scope and prevented it from proving as satisfactory for purposes of the present study as might otherwise have been the case. But no other usable data were available, and the results obtained were all the more remarkable, confirming as they do the facts derived from the study of Groups I and II. It was thought best, therefore, all things considered, to represent the caution index in this last group in terms of the percentage of wrong

completions as compared with the number attempted[9] and not with the entire 24 items of which the test is composed. Thus, an individual attempting 10 completions with 8 right and 2 wrong would receive an index of 20, etc. Here also the larger index indicates less caution, as in the case of the Binet test for Group II.

By way of summary it may be said that for subjects taking the Thorndike test and the Otis group test the caution index was taken as equal to the number of wrong answers without further treatment; and for those taking the Binet vocabulary test and the Trabue completion test the index was expressed in terms of the following formula:

$$\text{Caution index} = \frac{\text{No. items wrong}}{\text{No. items possible}}$$

We are now ready to proceed to a consideration of the method used for determining the scholastic index of the groups studied. There were no school records available for Group III, and hence there are no scholastic indexes for these subjects. For Group I the index was computed by transmuting the letter grades—A, B, C, D, and F—into their numerical equivalents on a scale ranging from 15 to 1 (A=15 and F=1). These values were empirically assigned as a result of several years' experience on the part of the Dean's office of Columbia College.

The scholastic performance of the children in Public School No. 192 is recorded by letters ranging from A to D. Only one letter is entered on the permanent records for each child, and this represents that child's general average in his studies for an entire half-year. If he is deficient in any subjects, the names of these are entered in a space provided for this purpose. Finally the pupil is also given a term mark on conduct, likewise indicated by one of the letters, A, B, C, and D. For both scholarship and conduct, any grade above C indicates passing, while both C and D are unsatisfactory. Strangely enough, a B grade may represent anywhere from 60 to 90 on a percentage scale, and this fact made the construction of a transmutation table exceedingly difficult. However, after consultation with several of the school officials, the letter grades were assigned a numerical equivalent on the basis of 10, which represents as nearly as possible the actual value of these grades in percentage terms. The final results are shown in the Table below.

[9] I have used the word "attempted" here to express an idea for which a better expression seems entirely lacking. I hold that it is quite possible for one to "attempt" or "essay" a task mentally without writing down on paper the result of such an "attempt" or giving any other very objective indication of it. In many cases in Group III an individual wrote no answer to some of the items on the Trabue test, and yet it is quite possible that he put forth considerable mental effort in trying to discover the correct completions. All we can do in such a case, however, is to take the last item, for which there is a written answer, as the limit of the individual's endeavor, and estimate the overtly wrong answers as a percentage of this limit. A similar observation may be made with reference to the vocabulary test.

TABLE I

Showing the numerical equivalent for each letter grade as used in rating the scholastic performance and conduct of the individuals in Group II

Letter Grade	*Equivalent* *Without Deficiencies*	*With Deficiencies*
A	9	8.5
A−	8.5	8
B+	8	7.5
B	7	6.5
B−	6	5.5
C	5	4.5
D	4	3.5

Except in very few instances only the grades obtained in Public School No. 192 were used in computing the scholastic index for this group, in order that the conditions of the experiment might be kept as nearly the same as possible for all individuals. The half-dozen exceptions to this rule were made in cases where a pupil had been in the school for two or three terms only, and in such instances it was necessary to add several grades from the school last attended in order to obtain a reliable index.

After transmuting the letter grades into their numerical equivalents the total for each child was obtained and divided by the number of terms during which he had been in attendance at the school. In this way the scholastic index was made to represent the pupil's average performance during the time that he was in school. The same may be said of the Columbia College students, and a simple inspection of the scholastic index in either case tells at a glance the general calibre of the individual's school work. For example, a Columbia student whose scholastic index is 10.17, is a B− man, and a pupil in Public School No. 192, who has an index of 7.34, is a B pupil.

The intelligence rating for each of the three groups was represented as follows:

For Group I, the total score obtained on the Thorndike intelligence examination.

For Group II, the I. Q. obtained by means of the Stanford revision of the Binet test.

For Group III, the I. Q. and M. A. as obtained from the "team of tests" administered by the army psychologists.[10]

In the case of Group II a conduct index was found for each pupil by the method already described for obtaining the scholastic index, but, of course, no deductions were made in this case for special deficiencies.

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[10]These tests included the Woodworth-Wells Substitution Test, Naming Opposites Test (Pintner-Pyle-Whipple), Word Building Test (Pintner-Pyle), Memory Span for Digits, Trabue Completion Test (Scale A), and the P-N Inventory (Woodworth Personal Data Blank), as well as the Army Alpha. See H. L. Hollingworth, *Psychology of Functional Neuroses*, 1920, 210ff., and A. D. Tendler, The Mental Status of Psychoneurotics, *Archives of Psych.*, No. 60, 1923, 9.

Various other data of minor importance were collected, and all information was classified as indicated in the accompanying Tables.

TABLE II

Showing samples of the data obtained for Group I and the method of Classification

Individual No.	*Schol. Index*	*Intell. Score*	*No. Minus Scores*	*No. Zero Scores*	*Caution Index*
1	7.88	75.0	31	15	46
2	5.42	74.6	24	9	33
3	7.01	71.2	40	13	53
4	4.11	75.8	25	8	33
5	7.54	7.58	22	16	38
6	6.96	82.0	21	10	31
7	4.94	88.4	26	6	42
8	6.97	78.6	18	9	27
9	11.12	88.2	16	6	22
10	7.84	88.2	31	11	42
11	9.54	110.8	23	8	31
12	9.53	78.0	22	7	29
13	9.50	77.6	32	13	45
14	8.42	81.2	26	4	30
15	10.41	77.2	8	10	18
16	5.93	91.4	14	9	23
17	7.13	97.0	16	4	20
18	9.30	82.0	19	19	38
19	9.42	105.0	21	7	28
20	9.92	86.4	6	5	11

NOTE: It should be remembered that the caution index as noted in the last column above is taken as the equivalent of the total of the *minus* and zero scores combined.

TABLE III

Showing the method of Classification of a sample of the data obtained for Group II

Individual No.	*Conduct Index*	*Schol. Index*	*I. Q.*	*Caution Index Binet*	*Caution Index Otis*
1	8.40	5.75	96	12.5	...
2	8.33	7.67	82	9.9	52
3	8.95	7.50	102	23.5	...
4	8.75	8.00	120	16.7	...
5	9.00	6.33	98	10.0	...
6	7.88	7.63	103	17.5	94
7	8.17	7.67	106	20.0	...
8	7.88	6.44	99	15.0	...
9	8.00	7.44	81	19.2	65
10	8.00	6.75	70	13.7	...
11	7.25	5.56	71	12.5	32
12	8.00	7.13	90	15.0	...
13	8.33	7.00	81	35.7	...
14	8.57	6.93	102	20.0	...
15	8.00	7.60	110	15.0	...
16	7.15	6.65	83	3.3	99
17	6.80	7.00	97	10.0	64
18	8.83	7.42	80	5.0	...
19	7.44	7.22	104	16.0	...
20	8.67	6.58	85	14.0	...

NOTE: Where no figures are given for the caution factor on the Otis test it is to be understood that the individual in question did not take this test.

TABLE IV

Showing samples and method of classifying data for Group III

Individual No.	*Caution Index*	*C.A.*	*M.A.*	*I.Q.*	*Classification*
1	43	25	18	112	Psychoneurotic
2	22	18	11:5	72	"
3	50	29	13	81	"
4	47	27	9	75	"
5	37	27	8	50	"
6	0	22	8	81	"
7	18	28	13	81	"
8	35	21	13	44	Epileptic
9	12	23	18	112	"
10	13	20	16	87	"
11	17	26	12:6	59	"
12	22	29	7:1	106	"
13	0	36	18	81	Psychasthenic
14	17	19	11:6	72	"
15	15	29	13	50	"
16	40	25	14:1	62	"
17	5	30	9:6	82	Exhaustion Neur.
18	8	25	17	69	"
19	12	28	13	94	"
20	18	26	11:5	103	"

Discussion of Results

As a prerequisite to the making of comparisons between the various groups and of interpreting the data obtained in the course of the study, considerable statistical treatment of the material in hand was necessary. In the main this treatment consisted of (1) finding correlations between factors occurring in individuals included within the same group; (2) obtaining partial correlations of the most important factors studied in Groups I and II, these being the caution factor, the scholastic performance, and the intelligence rating; and (3) from the above, getting a regression equation from which the probable scholastic performance of an individual might be predicted on the basis of the other two variables, *i. e.*, the caution factor and the intelligence score (or I. Q.). These three steps may be discussed briefly in order.

(1) For the simple correlation between two factors the Pearson "product-moment" formula was used in every case. In order to reduce the number of errors to a minimum, the standard tables and the computing machine were used for all but the simplest calculations. These correlations are included in the summary Table below.

(2) For the partial correlations only two groups were considered, namely, Groups I and II. Three variables were used—the caution index, scholastic index, and intelligence score (Group I), or I. Q. (Group II). Each variable was eliminated or "partialed out" in turn. These results are also given in the Table following.

(3) The usual procedure was followed in finding a regression equation from which the probable scholastic performance of a given individual might be predicted on the basis of his caution index and his intelligence score (or I. Q.). This equation was determined for scholarship only, as it has no practical value for the prediction of the other variables. The equation for each group is given below.

For Group III the only correlations practicable were those between the caution factor and the M. A., on the one hand, and between the caution factor and the I. Q., on the other. There is a singular coincidence in the case of the results obtained here,

TABLE V

SUMMARY OF CORRELATIONS

Between	*Group I*	*Group II*
(1) Scholastic index and caution index	.27±.03	.15±.05
(2) Scholastic index and intelligence score	.45±.03	.40±.04
(3) Intelligence score and caution index	.40±.03	.43±.04
(4) Scholastic index and caution index with intelligence score excluded	.11±.03	.02±.05
(5) Intelligence score and caution index with scholastic index excluded	.22±.03	.41±.04
(6) Scholastic index and intelligence score with caution index excluded	.39±.03	.38±.04
(7) Caution index and time required for Parts II and III of the Thorndike intelligence examination (N=132)	.09±.06	
(8) Scholastic index and no. *minus* scores	.24±.03	
(9) Scholastic index and no. *plus* scores	.10±.04	
(10) Scholastic index and value of *minus* scores	.14±.04	
(11) Scholastic index and value of *plus* scores	.22±.03	
(12) No. of probations and caution index (N=40)	.71±.05	
(13) Conduct index and caution index		.07±.05
(14) Scholastic index and caution index (Otis) (N=88)		.49±.05

NOTE 1. All correlations and P. E.'s were carried to two decimal places only, and this accounts for the apparent lack of variation of the P. E. in some of the cases listed above.

NOTE 2. On the assumption that, in "true-false" tests, the chance element operates to make half the guesses right and half of them wrong, the number of wrong scores was doubled for Part II, test 8, in the Thorndike intelligence examination and the experiment was tried of correlating the changed caution index with the scholastic index. The result showed a correlation of .26±.03 as compared with the original .27±.03 (see above), thus making no appreciable difference in the figures already obtained.

TABLE VI

Showing the range and the average of the M. A., I. Q., and caution index for each of the sub-groups included in Group III

Classification	*No. of Individuals*	*M. A.* *Range*	*M. A.* *Avg.*	*I. Q.* *Range*	*I. Q.* *Avg.*	*Caution Index* *Range*	*Caution Index* *Avg.*
Psychoneurotics (not further specified	68	7:1-18	12:7	44-112	77.9	0-84	17.4
Epileptics	32	8 -18	12:11	50-113	79.9	0-77	17.6
Hysterics	21	8-17:5	12:7	58-109	83.1	0-76	17.4
Psychoneurotics (syphilitic)	18	8 -17	13:7	50-106	83.7	0-53	15.8
Mental Defectives	17	7 -14	8:11	42-65	52.8	5-83	32.9
Psychasthenics	5	10 -18	14:0	63-118	88.6	0-37	11.2
Exhaustion Neurotics	5	10 -18	13:4	62-113	83.2	0-45	14.8

although for the second correlation the number of individuals was only 127 on account of the incompleteness of the available records. The correlations are as follows:

Between caution index and M. A., $r = .60 \pm .03$ ($N = 165$)
Between caution index and I. Q., $r = .61 \pm .04$ ($N = 127$)

An interesting comparison might also be made between the various smaller groups comprising Group III. Because of the small number of individuals in some of these sub-groups, no extensive conclusions may be drawn, but in Table VI are presented the range and the average for each sub-group in respect to M. A., I. Q., and the caution index together with the number of individuals included in each classification. The reader's attention should again be called to the fact that a low caution index always indicates the presence of the trait to a high degree, and conversely.

Significance of Results

For the most part there is singular agreement between Groups I and II in the matter of correlations. For both groups the correlation between the scholastic index and the caution index is the lowest, being $.27 \pm .03$ and $.15 \pm .05$ respectively. Between the scholastic index and the intelligence score[11] a correlation of $.45 \pm .03$ was found for Group I and one of $.40 \pm .04$ for Group II. The same groups show a correlation of $.40 \pm .03$ and $.43 \pm .04$ respectively between the caution index and the intelligence score. While the correlations are all somewhat low, we may conclude that the caution factor seems to have little influence on an individual's scholastic performance, though it does seem to affect his score on an intelligence test. This is the premise with which we started, and there can be little doubt that the caution factor, as a rule, plays no inconsiderable part in intelligence test performance.

The correlations between the intelligence score and the scholastic index are as good as the average correlations between the same factors obtained by Wood[12]. His highest correlation (.672) was obtained with a somewhat selected group chosen from the individuals included in Group I of this study. It may be said, further, that his data for determining the scholastic performance of his group covered a period of two years instead of three, and the increase in the size of the group *plus* that of the number of scholastic grades included in the present investigation may be regarded as, in some degree, accounting for the lower correlation of $.45 \pm .03$ obtained in this case. Some

[11]The expression "intelligence score," as used here, includes also the I. Q., which was used to indicate the intelligence rating of the members of Group II.

[12]B. D. Wood, *op. cit.*

equally low correlations have been found in other studies.[13] In any event, we may say that the general intelligence of a person as measured by his I. Q. or his score on the Thorndike examination is an important factor in his scholastic performance.

As to the partial correlations, the low figures obtained with both groups between the scholastic index and the caution index, with the effect of intelligence eliminated, seem to show that the "cautiousness" or "rashness" of an individual does not affect his school standing. But note that this is true *in the long run*, and it is entirely possible that, since the normal distribution curve may be assumed for the caution factor, the "rashness" which is manifested by a person in one situation may be compensated for by considerable "caution" in another situation. In the correlation mentioned above, where the effect of intelligence was not eliminated, the same general tendency seems to be present.

With the results of schooling ruled out, the two groups show a considerable difference in the effects of the caution factor on the intelligence score. For Group I the correlation is .22 ±.03, while for Group II it is .41 ±.04. This difference is probably due to various causes, among which may be especially mentioned the wide variation between the two situations in which the individual is tested. The Thorndike test is administered as a group test and the individual in question must make the decision as to whether he will guess or not. But the Binet test is administered by an examiner, who directly confronts the subject. Hence the personal equation enters greatly into a situation of this kind. In addition, many examiners consistently urge the child to guess, even though he says "I do not know"; and he is told repeatedly to "try" or "try again", as the case may be. In such instances, therefore, the number of wrong answers would, other things being equal, in all probability be much greater as compared with the number of correct answers or with the total number of items on the test than would be the case under other circumstances.

If now we eliminate the effect of the caution factor, the correlations between the scholastic index and the intelligence score are found to be .39 ±.03 and .38 ±.04 for the two groups. Hence, while the correlations in each case are in very close agreement, the two sets of figures do not show a very appreciable

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[13]See E. Gordon and H. J. Baker, Intelligence Tests and Academic Standing, *Journ. Applied Psych.*, 4, 1920, 361-363. These writers studied the I. Q.'s of 44 students at the University of Michigan as correlated with their scholastic performance by courses. The highest correlation obtained was .55, with several others much lower. In a similar study with 48 college students Caldwell found a correlation of only .44 between I. Q.'s and scholastic performance. See H. H. Caldwell, Adult Tests of the Stanford Revision Applied to College Students, *Journ. Ed. Psych.*, 10, 1919, 477-487.

difference. This fact would seem to indicate that little would be gained by the elimination of the caution factor from intelligence test performance, at least as far as predicting probable scholarship is concerned.

The other correlations presented in Table V possess a less marked significance. For Group I there seems to be little relation between the speed with which the intelligence examination was taken and the caution factor, *i. e.*, a fast-working individual does not necesarily show any less caution than a slow-working person of the same intelligence.

If we take the number of *minus* scores alone as indicative of the presence or absence of the caution factor, we get for Group I a correlation of .24 ±.03 with scholastic performance, as contrasted with one of .27 ±.03 when the *minus* scores are combined with the zero scores to make the caution index. For a large number of cases either method of determining the caution index would probably be satisfactory.

The correlations in Group I between scholastic performance and the number of *plus* scores, the value of the *plus* scores, and the value of the *minus* scores taken in turn are all too low to enable us to draw any satisfactory conclusions.

A correlation of .71 ±.05 was obtained for 40 individuals in Group I between the caution index and the actual number of times each was placed on probation during the time in which he was in college. This would tend to show that the "rash" person is more likely to get on probation than the "cautious" one, but the number of cases examined is too small and the method of treating probations is too unsatisfactory to make any such conclusion a valid one.[14]

In Group II a correlation of .07 ±.05 seems to show no relation between conduct and the caution factor in the case of public school children. This is in agreement with the general results for the same group between scholarship and the caution factor, and may probably be explained in the same way (see above).

A correlation of .49 ±.05 for 88 persons in Group II between the caution index, as determined on the basis of the Otis group test, and the scholastic index may be explained by the fact that the number of individuals is too small for general conclusions. A larger group might be expected to reduce this correlation, causing it to conform more closely to those obtained from the use of the Thorndike and the Binet tests.

[14]This statement should be explained further by calling attention to the fact that it is practically impossible to get an adequate index of the number of times a student is placed on probation. Some students are admitted on probation, while others are not, and in no two successive years is the number of probation periods exactly the same.

The equations as determined for the purpose of predicting the probable scholastic achievement of individuals when their intelligence score and caution index are known are as follows:

For Group I we have:

$$X_1 = 2.72 + .02\,X_2 + .06\,X_3 \text{ (P. E.} = \pm 1.3)$$

For Group II:

$$X_1 = 4.04 + .002\,X_2 + .03\,X_3 \text{ (P. E.} = \pm .45)$$

In each case X_1 = scholastic index
X_2 = caution index
X_3 = intelligence score (or I. Q.)

An inspection of the formulae will show that for Group II the caution index is a negligible quantity in predicting and the equation may, therefore, be written:

$$X_1 = 4.04 + .03\,X_3 \text{ (P. E.} = \pm .45)$$

In the equation for Group I the intelligence score is three times as valuable for predictive purposes as the caution index.

In order to make the equations entirely reliable the P. E. should in each case be multiplied by 4 (assuming the normal curve). On this basis we find the range of variability to be ± 5.2 on a scale of 15 points for Group I, which is too large for practical use. For Group II the range is considerably smaller, being but ± 1.8 on a scale of 10 points. Predictive results obtained by means of these or similar formulae might be used to advantage by school authorities in determining the probable scholastic achievement of those who apply for admission, and this would be an additional factor to the information now generally employed in evaluating and rating the candidates for admission to colleges and other schools.

For Group III the correlations between the caution index and the M. A., and also between the caution index and the I. Q., are rather high ($.60 \pm .03$ and $.61 \pm 0.4$ respectively). The difference between the P. E.'s here is due to the fact that, because of defective records, only 127 cases are included in the second correlation. In general, the caution factor seems more evident in persons of high intelligence than in those lower down in the scale. This is to be expected, especially when a group test is given to individuals with a range of intelligence extending from very low to very high, as was the case with Group III. It is conceivable that, where such discrepancies between individuals exist, the less intelligent ones, in addition to whatever natural propensities toward "rashness" they may possess, are spurred on by the activity of the other individuals taking the test and hence make more guesses than they might otherwise do. In a more intellectually homogeneous group this condition would not be so emphasized as where wide variations are present in intelligence levels.

No valid comparison may be drawn between the various sub-groups of Group III, because of the small number of individuals which some of them contain. In Table VI are given the range and the average of the M. A., the I. Q., and the caution index for each sub-group, from which the reader may make his own deductions. The most interesting point seems to be the fact that, although the ranges vary widely in their limits, the average caution index shows little variation from sub-group to sub-group, with the single exception of the Mental Defectives. Here we find an average caution index of 33.9, which is approximately twice as large as that for the other sub-groups. This is still further evidence to substantiate the hypothesis put forward in the preceding paragraph.

As an illustration of the differences occurring in scholarship between persons with a high caution index and those with a low caution index the following may be mentioned: a group of 16 individuals making a low average caution index and one of 17 individuals making a high average caution index, although all were of approximately the same intelligence as evidenced by their scores on the Thorndike intelligence examination, showed a difference of 8.6 points in favor of the more cautious group as regards their average scholastic performance.

Further instances comparable to the above are presented below, all indicating that the cautious individual (*i. e.*, the one with the lower caution index) stands higher in scholarship.

TABLE VII

Showing difference in caution indexes and scholastic indexes of pairs of individuals, both with approximately the same intelligence score

Pair No.	*Intelligence Index*	*Caution Index*	*Schol. Index*	*Diff. in Schol. Index*
1	74.8	8	11.11	5.69
	74.6	33	5.42	
2	88.2	15	10.07	1.90
	88.2	29	8.17	
3	91.0	18	8.58	1.95
	91.6	30	6.63	
4	84.0	20	8.60	2.27
	84.0	40	6.33	
5	90.0	5	7.57	.37
	90.0	12	7.20	

While the correlations obtained were not high enough to show that the cautious individual invariably excelled in scholarship, they nevertheless served to indicate that the *trend* is in that general direction.

One other example of a slightly different type may be given. Two brothers made scores on the Thorndike examination of 111.8 and 104.4, with caution indexes of 16 and 12 and scholastic indexes of 12.77 and 10.90 respectively. The difference of 1.87

in scholastic performance would not be expected from an inspection of the small differences to be noted in the case of the caution index and the intelligence score. An examination of the scholastic records of the two individuals showed that the one with the lower scholastic index had been compelled, on account of sickness, to be absent from four examinations in one semester. Thus the difference in scholarship is to be explained on this basis rather than on that of the caution factor or the general intelligence level.

Conclusions

(1) The caution factor seems to have some influence on scholastic performance, though this fact is more marked in the case of Group I than in that of Group II. In all probability this difference is due to the differentiating character of the situations placed before the individual taking the Thorndike intelligence examination as contrasted with the average type of situation found in other standard tests.

(2) There seems to be a fairly well-defined relationship existing between an individual's score on an intelligence examination and the caution factor, largely because this relationship is made to exist by the technique of scoring employed in dealing with the wrong answers, which, as has been shown, form the basis for computing the caution index.

(3) A still closer relationship than the above may be noted between the scholastic performance and the intelligence rating of an individual, and this result is in accordance with the results previously obtained in this connection by other investigators. The caution factor has apparently little influence on the correlation in this case.

(4) The caution factor seems to influence conduct and the time taken for the performance of a given task only slightly, if at all.

(5) The number of the *minus* scores and the value of the *plus* scores on the Thorndike examination show some relationship to scholastic performance, but here, as well as in the case of the number of *plus* scores and the value of the *minus* scores, the correlations are too low to permit of any definite statement as to how or to what extent scholarship depends upon any of these.

(6) The regression equations for predicting scholarship on the basis of the intelligence score and the caution index have been actually used on various individuals and found to be reliable in every case within the range of the P. E. In the equation for Group II, it will be remembered that the caution factor played such a small part that it was dropped from further

consideration; but in the case of Group I it was found to be worth approximately one-third as much as the intelligence score for prediction purposes. This fact may again be due to the difference in character between the Thorndike test and other tests for intelligence.

(7) In general, the results of the study tend to show that the cautious type of person, other things being equal, is more likely to excel in scholastic performance than one of the opposite type. This statement seems to be borne out by the illustrations already given in both group and individual cases.

(8) Further investigation is needed to show the operation of the caution factor in fields other than that of scholastic performance.

(9) The combination of intelligence tests with those now used for testing character traits should be a feasible extension of the use of tests. Emotional tests, such as the Downey "Will Profile", might be combined with some one of the present well-known intelligence tests, allowing, of course, for suitable modifications in each wherever they might appear necessary. Suggestibility tests, tests of aggressiveness, etc. require the use of a certain amount of intelligence on the part of the person taking them, just as intelligence tests on their part show unmistakable evidences of certain character traits. If there is, therefore, this degree of overlapping, the possibility of devising tests which will furnish an objective basis for estimating both intelligence and the most important character traits at the same time is by no means a remote one. Entirely new tests may be devised for this purpose without specific reference to those already in use, or some more or less modified combination of the latter may be employed, or even the intelligence examinations as they now stand may be further studied for evidences of character traits, as has been done in the present instance.

(10) Following the analogy of Hart and Spearman and of Webb,[15] who have introduced the concepts of the 'g' and 'w' factors as forming the basis of intelligence and character in general, it is suggested that specific character traits, as they are recognized and studied, be denoted by letters of the Greek alphabet, and that accordingly the caution factor be called the "Zeta" factor or by some similar name.

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[15]B. Hart and C. Spearman, *op. cit.*; E. Webb, Character and Intelligence: An Attempt at an Exact Study of Character, *Brit. Jour. Psych., Mon. Suppl.*, 1, 1915, no. 3.

DOES BEHAVIORISM IMPLY MECHANISM?

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The reader will recall the instance of the German who went to Paris, and thought the French a queer people because they insisted on calling bread *du pain.* This seemed to him very foolish, for, by whatever name they might call it, was it not *Brot*? Changing one's language is not easy, and until the new language is acquired in some completeness it has meaning only when translated into the native tongue. Yet changing the language need not change the significance, however impossible as a medium of expression the new language may seem to one not versed in it. Some of the critics of Behaviorism dislike the new language, and will not accept it at the assessment which its users give. The philosophy of such critics seems to run in this wise: Only material bodies behave; therefore behaviorism must be a phase of materialism. My thesis is that the behaviorist may be as idealistic as the best—or the worst—of his critics; and that a large proportion of the misunderstanding of Behaviorism must be put down to an indisposition to accept the new tongue, and an unwillingness to take the words in the behaviorist's sense.

Behaviorism, however, means much more than attaching new words to old situations; it is in large part a new view of old situations. What it succeeds in doing, or at least aims to do, is to furnish for previously incommensurable things, objective and subjective, belief and fact, intention and conviction, a common denominator in terms of which all may be compared. A common denominator does not abolish old differences, but merely restates them in new form, in a form in which comparison is possible. Under the new form the old differences are present as truly as ever they were, but we can now state the degree of difference and not merely the fact of difference. The advantage of Behaviorism over introspection or subjectivism, for example, is not that subjective-objective differences are abolished, but that they can be adequately appreciated. To infer otherwise were as if one supposed that three-fourths lost its difference from seven-eighths as soon as the former denominator became the latter.

The latest example of this confusion of meanings—as it seems to me—is McDougall's *Outline of Psychology,* wherein Behaviorism is opposed to purpose and made almost synonomous

with mechanism. His own examples ought to furnish enlightenment. Behavior, it is true, is just behavior. Some behavior is non-purposive, some is purposive.

McDougall makes this clear, and then proceeds to identify purposé with foresight of the end on the part of the agent. This is to confuse the nature of the act with consciousness of the nature of the act. It can not be known as purpose unless some one knows it as purpose; but why insist that the agent must be the knower? Must the pigeon be aware of the purpose to propagate when he embarks on a career of courtship admirably adapted to secure the end which we foresee? Is it not accidental whether the purposive agent is aware of his purpose, and is not his awareness of it quite irrelevant to the question whether or not his activity is purposive? The life-history of every individual gives the story from another point of view and in other sequence. We strive for life long before we know that we are striving; were it not so, we should not live to know. We act purposively long before we are aware of our purpose; otherwise there would be no mature we to be aware of what we are about. Millions of individuals show a will to live, and few there be who are aware of it; nor is that will to live made more real by their becoming aware of it. Such knowledge no more changes the nature of their purpose than an acquired knowledge of anatomy changes the number of vertebrae or of endrocrinal glands. Why do the psychologists write psychologies if not to show us truths about ourselves which we did not know before? New knowledge may instil new purpose, but the converse also is true.

The behaviorist would allege that we are because we do and what we do, introspection notwithstanding. The real motive, the real force, is that which manifests itself. How otherwise we can know it, and how we can know more than that, is what puzzles him. He would look askance at McDougall's definition of instinct as "enduring innate dispositions, which generate impulses to action......the mainsprings of human conduct," if by this we mean that instinct is something over and above the organism which is acting, as McDougall again and again implies and says that it is. "Instinct makes us do this and that," he would say, as though instinct is a separable force opening floodgates or actively prodding us this way or that. This is like the earlier view of gravitation as some all-pervading force which holds the planets in place and tugs at all material particles.

Such a force of gravitation there is not, or at least we know it not. It is not gravitation which pulls bodies, but the pull of bodies is what we designate by the name of gravitation. So it is with instinct. We do not do things because we have instincts, but we have instincts because we do them; which is merely an-

other way of saying that certain prevalent modes of behavior, performed spontaneously and without learning, are assigned to the category of 'instinctive.' The reifying of instinct is but making an entity out of a class name.

We sympathise with McDougall's inability to follow the mechanists and with the extent to which he would read purpose into mental activity of almost every sort, our remembering as well as our learning. Mechanically minded behaviorists fall far behind McDougall in their effort to extract the meaning of behavior.

To view behavior as just behavior seems meaningless, and I think the behaviorists would so regard this attitude. For them, as for other people, some sorts of behavior are of more significance than are other kinds; and only purposive behavior is significant. How the behaviorist is to behave toward the phenomena of behavior is a problem as well as a fact, and all behaviorists' behavior is not of equal value.

Indeed, one behaviorist, reviewing McDougall's diatribes against Behaviorism, behaved in such a manner as almost to merit the name of misbehavior. To interpret his review of McDougall as just behavior on the part of a behaviorist might be true, but would not be enlightening. One obtains a much more enlightening insight into the action of the aforesaid critic-behaviorist by reading McDougall's purposive psychology than by reading the current behaviorist psychology.

The explanations of action which many of the behaviorists give we can no more accept than can McDougall. Suppose, for example, that the behaviorist wishes to give an account of why I proceed from the street to my house. He—or some of them—will say that it is because this foot goes before that, with certain muscular pulls and nervous stimulations which combine to plant my feet before the door. In a word, by studying the details of the action they profess to be able to explain the outcome of the action. Such an ideal is futile. A detailed study of this kind will show how the action is performed, but will not tell us why. It is not because one foot is placed before the other in alternation that I am proceeding toward the door, it is rather because I am proceeding toward the door that one foot goes before the other, so many paces to the front, so many to the left, then again forward. Only the completed action explains the details of the action, for the act is in no sense a summation of the activities which make consummation possible. The walking is the result of my going to the house; I do not go to the house because I am walking. Walking may be a necessary condition, but is not a sufficient one. To be explained, the act of going to the house must be classified with similar acts of going to the house. Even so the real signifi-

cance of it may lie in the fact not that I am going to the house, but that I am going after my revolver to blow out my brains. And this may not be its final significance, for perhaps I am irritated at my insurance company and want revenge by making them pay up to my widow. But before we have settled on this as the significance of the act of going to the house, the neurologist tells us that my mind was deranged, melancholia gripped me, and the insurance company was only the convenient present hallucination, in lieu of which some other would have served equally well. In a sense each one of these interpretations of my purpose would be true, but that interpretation is most significant which gives the surest anticipation of my conduct. That action is purposive which leads in the main, or in a certain proportion of cases, to further action or state of being which can be predicted as probability but not as certainty. One result of my footfalls upon the walk is to send tremors through every structure in the city in which I live, but this can not be called my purpose, since it happens whenever I walk and whenever any one else walks. No such invariable result of an action can be accepted as the purpose of the act, and no psychological significance attaches to an act which has such invariant consequences. This, of course, does not mean that the act must succeed in order to be purposive; its nature is determined by its class, not by its accidents. My one successful throw of the dice does not demonstrate me a skilled man, and a failure in business enterprise does not show that I lack business acumen. The character of similar acts determines the character of the one in question, and the results of similar acts determine whether the one in question is purposive or non-purposive.

This does not mean that I value idea or ideal above fact, but that fact as fact is meaningless. To one who insists that a fact is a fact, the sufficient answer is, What of it? We are sometimes as assured of inference as of fact of observation. Let me give an instance.

In my office there is a skeleton some 6 ft. high. I know it for that of a man. I examine the articular surfaces of the long bones, the mandible, and the teeth. I know that it once was part of a living human being who walked and who ate. I did not see him while alive, and so far as I am aware no one living saw him alive, and no record exists that such a person was once a walking, masticating human being. That does not diminish in the least my assurance, nor that of any one who has mastered the elements of anatomy, in the fact that he once lived and moved and ate. Suppose a few million people gave me sworn testimony that they knew the said person in the flesh, but that he never walked nor ate. This one skeleton confutes them all,

for it could not be as it is if it had not been as I describe it, witnesses notwithstanding. If this is good science, it is also good psychology.

In a similar manner, though not with the same surety, we may infer purpose where no purpose is seen save as it is inferred. Certainly it is not necessary that the agent foresee the purpose.

Behaviorism would escape, if that be possible, a discrete world of incommensurable parts by seeking, let us say, a concrete world of commensurable parts. It does not see only things to the exclusion of events, nor form to the exclusion of function. It does not proceed piecemeal, as if reality were got only by adding up the elements of which the world is made, as if the sum of the parts could give the whole, when obviously in many cases it can not do so. To understand man we must understand mankind. Mankind can be viewed as one historical development extending through thousands of years, making that development a unit as truly as if these years were so many seconds. The significance of any bit of human behavior must be determined by its relation to this larger comprehensive unity. Behaviorism needs this point of view as much as does idealism or subjectivism. Behaviorism can proceed profitably only when it moves in the light of the larger unit within which the details of behavior fall. One can not discover the significance of the behavior of the human organism, nor even its behavior, by adding up the function of the hand, that of the heart, and so on, until all the component parts have been included. The behavior of any organ must be treated in the light of its relation to the entire organism and to the respective parts of that organism. Similarly, to understand man the individual, we must start with man the species. Into the metaphysical problem how this is possible I am not now concerned to inquire.

So far as results are concerned, Behaviorism seeks a new plane of procedure quite as much as it seeks new methods of procedure. Its methods are as much open to question and to verification as are the methods employed in any field of research. How Behaviorism should behave is as much a problem as how mind should think, and one which the behaviorist can not escape by persisting in that kind of behavior which merely observes and records behavior; for, by his own showing, subjectivism is as truly behavior as is the procedure adopted by the behaviorist. His only defence would seem to be the purposive one that subjectivism is a form of behavior which does not yield as much truth or profit as his own objective method. He can defend his position only by being purposive; and one of the most important things for the behaviorist to point out is the significance of the differences which lie within the realm of behavior.

In asserting the dependence of behavior on organism there follows no inference that behavior is impelled in a mechanistic fashion by the machinery in which it is incorporated. An automobile loose and an automobile driven by a man are each of them mechanistic at any moment of the process; but in the process taken in its entirety the one acts solely according to mechanistic principles, the other as a mechanism under the control of man, illustrating a type of behavior which is supramechanistic if the man be included. If we compare the man with the automobile as to type of behavior, we find that the significant differences are not in quantity of action but in quality, in variability of reaction not in complexity of reaction. A steam-engine is much more complex than an amoeba or a paramoecium, but in the living organism we find a different kind of unit-reaction measurable by conduct leading to self-preservation. The engine has only one reaction to fuel or potential fuel, the amoeba has many toward food or potential food. From the point of view of structure and of reaction to stimulus, a stone differs from a badger not in simplicity of structure and of reaction to stimulus, but in the invariant reaction under the same conditions, and as merely enduring rather than preserving self.

The mechanist critic may here object that we are positing something, namely self-preservation. That is true. But is not 'invariance of reaction' merely an ideal and in no sense, at least in no empirical sense, a demonstrated 'fact' of science? Does science have empirical knowledge that under the same conditions the same stone will fall always with the same velocity and in the same direction—if there is a same direction—or is this not merely a postulate or concept justified by the results which follow from assuming it? Similarly, purpose, aim, self-determination, self-preservation, are postulates justified by their utility in anticipating behavior.

The living organism is mechanically constructed, but the driver of the machine is the machine itself, and its variance of reaction shows that it is not mechanistically determined. There is no vital principle, but there is a vital organism; no drive as such, but a driving organism. The organism threads its way through its environment as the pilot of an aeroplane chooses the path of the aeroplane, operating at every moment in a rigidly determined mechanistic world, but not mechanistically determined in the drive through that world. There is no independence of mechanism, but there is control of mechanism. Matter does not make mind as particles make the aggregate or even as bricks make a house or cogs a machine: the arrangement, rearrangement, the dynamic, is through material substance, but performing a non-material function; and function is not matter.

Behaviorist psychology need not dispense with function. Indeed, so far as it is profitable, it can not do so. The processes called forth in walking do not make walking; it were nearer the truth to say that it is walking which brings into play the processes, truer physiologically, biologically, psychologically. Function develops organism as truly as organism function. From first to last behaviorist psychology can be put into terms of purpose, and we venture the prediction that the time is not far distant when some psychologist will do this.

The advantage of proceeding on the solid ground of behavior is that of presenting objective and verifiable data; on such data inference must be based, whether the behavior be taken as constituting mind itself or as being only a manifestation of mind in these media.

"But," we are told, "a lobster is, after all, only a bit of chemical composition." To which one might reply: "Then why call it a lobster?"

What is meant when it is said that a lobster is merely a chemical composition? It means, we are assured, that a complete description can be given in terms of carbon, hydrogen, etc., and the arrangements and quantities of the respective chemical elements found in the lobster. We submit that such a complete description is no description of lobster but merely of the chemical composition of lobster. It tells us too little about his politics, ethics, or nature philosophy. It gives us information about his chemical composition, and that is all. No analysis of the chemical composition of lobster will enable the chemist to predict the behavior of that animal. The psychologist who studies the behavior of the creature will have a much better line on the future behavior of lobster than will the chemist who knows the elements of which it is composed. No conceivable insight into chemical composition will give us an understanding of organic activity. The chemical 'explanation' is no explanation at all; it is merely a statement after the fact of the conditions prevailing when certain organic activity takes place. The explanation of the disaster of the European war is not that a number of human organisms were in the way of guns which kept pointing at them; the explanation of the pointing of the guns lies in the human organisms which were making use of these weapons. There is a sense in which it might be said that men were on the defensive because guns were after them; but the explanation of the situation produced by the guns lies not in the guns but in the men using them.

So with lobster and its carbon and hydrogen. To be sure, there would be no lobster without these elements; neither would these elements be present in that particular time and space order except for lobsters. Lobsters did not make them, but they utilize them.

To say that a lobster is only a chemical composition is comparable to saying that a book is only a certain number of pages in a certain order. That is correct, so far as we speak of pages and of order. But the statement is meaningless to one who inquires about the respective merits of books, to one who asks about principles of library classification, to one who inquires about literary value, or about language. We state facts, but facts of an order which do not assist in answering our query.

As a philosopher I am not much enlightened by the statement that Kant and Spinoza differed little because they had the same chemical composition with slightly different arrangements in quantity and quality. Nor am I much enlightened by the statement that another chemical activity was stirring when Spinoza wrote the *Tractatus Politicus* than when he was writing his *Ethica*. I am, in fact, not much more enlightened than if I am told that when the former work was composed a whale was caught by an Eskimo in Greenland, and when the second was under way an Iroquois Indian caught three moose. I am delighted to learn the coincidence of these events, but do not feel that they shed a flood of light on the differences between the *Tractatus* and the *Ethica*. I am not even able to say when Spinoza is likely to repeat his venture, or when similar chemical activity in some other philosopher will be followed by a similar *Ethica*.

Chemical activity and organic activity are facts of different orders, and the one can not explain the other. As well say that since each individual has specific weight and unique position in the universe a combination of these two explains the uniqueness of his thought. It would do so, and does do so, in so far as that combination enables us to predict his thought, to come upon results by reason of the key. But empirical issues show us that no such key is supplied by a combination of space and of time. One must reckon with other factors if one wishes to make prediction about the language or thought of a man, and these culture-features, though in space and time, are not of them,—that is, can not be inferred, or predicted, from reckonings in these dimensions. Only through observing the behavior of men can we understand the behavior of men, not by studying their avoirdupois, stature, or chemical composition. Mathematically, every sentence can be analysed into a number of words, but number of words is not its meaning. They may be its vehicle, but they are not its meaning.

In place of 'words' and 'sentence' substitute 'chemical composition' and 'organism', and the analogy holds. The chemical elements are the vehicle, but they are in no sense the explanation of the behavior of the organism. That behavior can be understood only by reference to behavior. Even a material object

like a building is not merely the sum of the things of which it is composed. Bricks in a certain order and arrangement are at least a requisite of a building, considered as a purely material thing. A specified order is accidental, in so far as by building we mean not merely arrangement of bricks but an arrangement utilized for a specific purpose. Arrangement of bricks in the building does not constitute the purpose for which the building is used, though it may be indispensable to that purpose. Indeed, it is the purpose which explains the arrangement of the bricks, rather than the other way about. This is as true of the bodies of living organisms as of objects made by man. As J. A. Thomson says: "We cannot give any mechanical re-description of the origin of really new mutations, nor of the way in which the chick is minted and coined out of the egg, nor of the ingenuity with which animals play the hand of cards with which they have been endowed."

Some critic not much interested in the disputations between behaviorists, mechanists, and introspectionists, will say that what I have written may be about psychology but is certainly not psychology. But is this not the very point in dispute? By all means let us get on with psychology. But what is psychology? How one answers this question depends, it seems to me, and must depend, upon one's philosophy of mind and body, of life and mechanism. To close our eyes to the problem is not to answer it. To proceed with a psychology is to proceed on some implication as to method and data, and the implication may as well be made explicit.

To sum up: The manifestation of mind is mind. The behavior of living things by variant reaction to similar situations and by the fulfilling of a will to live sets them apart from machines and from material bodies which merely endure or pass through fixed cycles of activity which can be accurately foreseen, once we are acquainted with the cycle and its conditions. Purpose must be read into actions in order to get it out of them; its utility as a device in anticipating behavior is its justification. The reality of the purpose is independent of knowledge of it by the agent. The more inclusive our schemes of purposes the greater our insight, the more far-reaching our predictions, and the greater the value of our point of view. The individual must be studied as member of its class in order to understand it, and the purpose must be related to other purposes. Our final aim is significance and meaning.

IS PRIMACY A FACTOR IN ASSOCIATION-FORMATION

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The experimental problem discussed in the following pages concerns the question whether the first object in a series of sequent impressions is, solely by virtue of its priority, more readily recalled. In current terminology this problem becomes the following: Is primacy a determining condition (a "law") of association-formation?

This question is of some interest, because a few psychologists have answered it affirmatively. Seashore[2] remarks: "The answers to these questions are to be found in the secondary laws which are laws of neural action or habit. Four of these are: primacy, frequency, intensity, and recency. Other things being equal, the first or primary association will dominate." In his recently published text-book occurs the following: "Other things being equal, the stimulus of a given character having once established a circuit, this path offers less resistance and will be followed thereafter: this is the law of primacy."[3] Take the following from Hunter[4]: "These secondary laws of association are frequency, recency, vividness, primacy, emotional congruity, similarity, and cause and effect." "Primacy refers to the first neural connection made. Common opinion testifies to the ready recall to mind of first loves, first dances, first days in the army, first battles. First neural associations are evidently important." Münsterberg[5] says: "The results also indicate that the pair which is given first in the series has a certain additional chance. This points to a fourth principle. Besides frequency and importance, the *newness* of a connection, which characterizes the beginning of a series, also offers strong chances for the reproduction. In every group of life experiences that which comes early influences our memory almost as much as that which comes last."

[1]Responsibility for this work is divided as follows: Welch performed the experiment, and worked up the results. Burnett suggested the problem and prepared this report.

[2]C. E. Seashore, *Elementary Experiments in Psychology*, 1908, 123.

[3]C. E. Seashore, *Introduction to Psychology*, 1923, 183.

[4]W. S. Hunter, *General Psychology*, 1919, 287f.

[5]H. Münsterberg, *Psychology General and Applied*, 1914, 120.

None of these authors except Münsterberg makes any reference to experiments supporting these assertions, and he in a merely general way, indicating the manner of experimentation that yields the asserted results.

Chief Experiments

Materials. A disc tachistoscope, as described in G. M. Whipple's *Manual of Mental and Physical Tests,* (2nd ed.) Part 1, 265. A metronome. A series of cards containing nonsense syllables (white gummed capital letters on a black ground). The syllables were made unpronounceable in order to equalize, as far as possible, associations that might be found by the *O*s. The letters x, y, z, and q were used frequently for the same reason. The list of series follows:

1.	YJV	VXG	HTL	DVT	RBF	WQK	XSD	QWS
2.	YCQ	VDT	HKR	TMQ	FJS	QNZ	ZWH	RXH
3.	YJM	TDV	HCX	DTJ	RCF	QDR	ZRK	NSX
4.	YTJ	TGV	HZC	DGM	FHW	XFR	ZDH	DSF
5.	YVJ	QRW	SXJ	DRH	CJQ	XSN	STD	TPM
6.	YMT	WPR	TBR	FSX	CMH	GPM	JDX	PDG
7.	YGR	LPV	TMS	SQN	PRD	CKM	RMK	QWH
8.	YKZ	WMZ	JNQ	SZM	NDR	SLX	CRH	RFQ

Special Conditions of Experiment. The tachistoscope was set up in the manner described by Whipple, with the opening adjusted for an exposure of 60σ. The *O* was given the following directions:

"You will be shown a series of printed syllables through the opening before you. You may repeat in mind, or give attention to, in any way that you please, the syllable that has just been exposed. But, and this is important, as soon as the next syllable appears you must cease to give attention to the ones that have gone before. As soon as the syllable disappears you will drop your gaze to the white chalk mark at the bottom of the apparatus. The signal to raise your eyes will be 'Ready.' After a series has been shown you will print in the spaces on the blank those syllables which you can remember. The signal for this will be 'All right.' Remember, you may think about a syllable as much as you please until the next one appears. Then, you must give your *entire* attention to the new syllable."

The practice syllable (RTB) was shown to the *O* in order to accustom him to the apparatus.

The time of exposure (*i.e.,* from beginning to end) of a series of 8 syllables was 1 min. This time was kept constant, within 1 or 2 sec., by means of the metronome.

The following questions were asked at the end of the experiment. (1) With what degree of confidence did you approach the experiment, and how confident are you of results? (2) Did you use any systematic procedure for remembering? Make this answer complete and illustrate from syllable remembered. (3) Was there anything which interfered with your doing your best?

Method of Scoring. Unit credit is given for each syllable correctly remembered. On account of the frequent occurrence of x, y, z, and q it seems inadvisable to give half-credits if two out of three letters in the syllable were remembered. Simple addition of the syllables will give a reliable index regarding the factor to be determined.

Results. These are given in the following Table.

TABLE I

Name	Date 1921	(1)	(2)	(3)	(4)	(5)	(6)	(7)	(8)	total
Vose, J.	Nov. 22	0	1	1	1	2	2	0	5	12
Ham, E. B.	”	1	5	2	0	3	4	3	7	25
Davis, G. T.	”	1	3	2	1	0	1	3	6	17
Smith, D.	”	0	4	2	1	1	1	3	6	18
Watson, J.	Jan. 5, 1922	2	2	2	3	2	2	1	6	20
Cousins, F. E.	”	2	3	2	2	3	1	3	5	21
Mendelsohn, M.	”	2	2	2	2	1	1	2	3	15
Clymer, W. F.	”	0	0	0	0	1	1	3	7	12
Strout, R. S.	”	1	1	0	1	0	0	4	7	14
Cousens, T. W.	Jan. 10	0	1	0	1	0	1	2	6	11
Badger, R. G.	”	0	1	4	0	1	1	3	8	18
Averill, F. G.	”	0	0	1	0	3	0	4	3	11
Hardy, M.	”	1	0	2	2	2	2	4	7	20
Christie, A. Q.	”	0	0	2	1	0	1	1	5	10
Grenfell, E.	”	1	0	0	0	0	0	1	4	6
Gilpatrick, G. S.	Jan. 12	1	0	1	0	1	0	1	2	6
Hanscom, R. D.	”	1	1	3	0	2	3	3	5	18
Bates, R. T.	”	0	0	3	2	1	1	3	8	18
Hunt, E. W.	”	3	1	0	2	1	1	4	3	15
Blanchard, R. E.	Jan. 17	2	1	0	1	0	2	2	5	13
French, E. K.	”	0	1	2	2	3	1	3	8	20
Ricker, S. W.	”	2	1	1	1	1	1	0	3	10
Beck, H. W.	”	1	0	2	1	0	0	0	6	10
Burnell, T. C.	Jan. 19	3	2	1	1	1	0	1	6	15
Towle, C. S.	”	3	1	2	2	2	0	2	6	17
Love, R. B.	”	3	3	1	0	0	2	3	5	17
Totals		30	34	37	27	31	29	59	142	389

The graph shows combined results for all *O*s. The order of syllables is represented on the abscissae and the number correctly remembered on the ordinates.

The graph shows, allowing for individual differences, that syllables 1-6, inclusive, were remembered to about an equal extent. It is only when the 7th syllable is reached that any important change in the number remembered is apparent. With the 8th, the increase is very great.

Conclusion. These results show, in general, that the first syllable in a series is remembered less than most of the others, and that recency preponderates overwhelmingly. In this connection it should be stated that, in many cases, the *O* wrote down the last syllable immediately upon being given the signal to write.

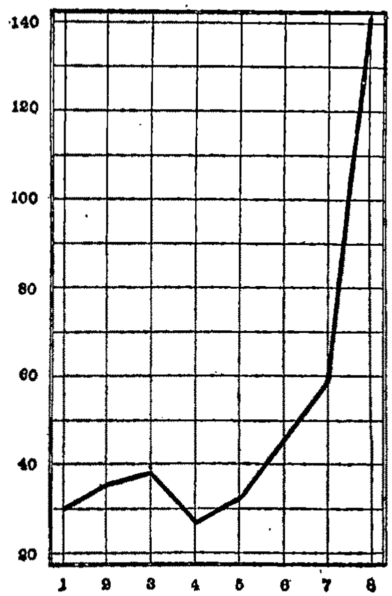
Why Primacy Should Have Appeared to be a Factor in Association

In the Bowdoin laboratory in 1919 an attempt was made by H. W. Lamb to test the validity of the four alleged laws of Primacy, Frequency, Recency, and Vividness, by use of 6 *O*s.

Material. This consisted of 8 series of nonsense syllables in white gummed letters on a black background, as follows:

1.	PEF	DAC	MOZ	HEK	DAC	LER	GIX	SEB
2.	YIB	NAS	ZAL	WOM	YIB	PEZ	RYV	HON
3.	MOF	GAH	BOP	NIM	FAL	ZED	NIM	VOT
4.	HAK	BAM	JYX	LEZ	FIP	JYX	SOJ	NIV
5.	WIV	JAF	TEG	LYG	CUK	BAZ	WIV	SIJ
6.	BAM	CES	DAV	JOS	KEC	CES	TUV	RIF
7.	NUK	WAB	PEM	ZUT	RON	KET	PEM	YIX
8.	TOL	CIB	DAG	GUX	FIK	LEP	CIB	HAR

As the syllables were shown in an order indicated to the reader by reading the series above from left to right, it is easy to see how the factors of Primacy and Recency were provided. One syllable in each series was repeated, to provide Frequency. Twice this repeated syllable coincided with the first, and any resulting preference shown by *O*s for this syllable had to be credited as divided between the two factors. Vividness was provided



variously and never twice in exactly the same way, by a strip or figure of white or colored paper placed adjacent to or behind the syllable or by a small gummed picture, embossed in colors, placed adjacent. Every syllable in turn was thus treated and in irregular order, except the last. If, then, preference of any *O* fell on the one syllable in which Primacy and Vividness coincided, the resulting credit had to be divided. That these modes of providing vividness may have provided distraction instead, in some cases, seems indicated by the second Table below.

There were, thus, on the 8 cards 29 "weighted" and 27 "unweighted" syllables. The syllables of each series were on a single cardboard strip 9 x 56 cm. sliding in a frame behind a flap-covered opening about 4 x 6 cm. Time was kept by a metronome.

Method. Starting the metronome and giving a warning signal "Ready" to bring the *O*'s gaze to the center of the flap, *E* gave each syllable an ex-

posure of about 1 sec. with about 1 sec. between exposures for adjustment of the card. He lifted the flap from behind the frame with his left hand, and slid the card with his right along the holder. The frame was so constructed as to conceal all manipulations from the *O*. At the close of each series the *O* recorded all the syllables he could remember from the just preceding series. No directions were given to him for dealing with the series during exposure other than that he was to fix in mind as many as he could.

Scoring. One credit was given for each syllable correctly reported, and, also, for the few syllables in which a mistake of one letter was such as might have happened if the *O* was holding on to the sound, not the look, of that syllable.

Results. These are shown in the following Table.

TABLE II

Relative Significance of Factors in Association

	Primacy Given: 39		Frequency Given: 42		Recency Given: 48		Vividness Given: 45		Unweighted Given: 162	
	Total Recalled	%	Total Recalled	%	Total Recalled	%	Total Recalled	%	Total Recalled	%
Claff	2.5		1.5		8		4		6	
Noyes	1.5		3.5		3		2		5	
Asnault	3.5		5.5		7		4		15	
Goff	1.5		4.5		3		5		14	
Guptill	2		2.5		6		1.5		0	
Coombs	6		6		4		3		18	
Total	17	*44*	23.5	*56*	31	*65*	19.5	*43*	58	*36*

Total no. weighted syllables given = 174
" " " " recalled = 91
% recalled = 52.

From this Table two things appear. (1) All four factors investigated are genuine factors in association, *each* one being shown by the percents to have a recall value superior to that of the unweighted syllables. (2) Primacy is as important as Vividness, but each is much inferior to Frequency and Recency, the latter topping the list.

TABLE III

Relative Significance of Factors in Association

	Primacy Given: 71.5		Frequency Given: 77		Recency Given: 88		Vividness Given: 82.5	
	Total Recalled	%	Total Recalled	%	Total Recalled	%	Total Recalled	%
Mitchell	4.5		3		3		1.5	
Kunkel	3.5		5		4		2.5	
Whitney	5		3.5		1		.5	
Allen	6.5		5		8		3.5	
Healy	2.5		5.5		3		2	
Dannis	4.5		2.5		5		2	
Bramson	2.5		2.5		5		1	
Ross	5.5		6.5		6		1	
Sullivan	2		2		7			
Philbrook	6.5		5		1		2.5	
Eldridge	2.5		2.5		5		2	
Totals	45.5	*64*	43	*56*	48	*55*	18.5	*22*

Total no. weighted syllables given = 319
Total no. weighted syllables recalled = 155 % recalled = 49
Total no. unweighted syllables given = 297
Total no. unweighted syllables recalled = 58 % recalled = 20

In 1921, M. O. Waterman repeated these experiments with the same material on 11 *O*s, using the same method as before. The results are given in Table III.

In this Table it appears that (1) all four factors investigated are genuine factors, each showing a recall-value superior to that of the unweighted syllables; and (2) Primacy is first in value, Frequency and Recency following at values about equal to each other, and Vividness far in the rear.

A comparison of Tables II and III shows (1) mutual confirmation on the main questions at issue; (2) a marked lowering of value for Primacy and Vividness, amounting practically to an elimination of the latter, as shown by comparison with the value of unweighted syllables. This result is perhaps not surprising, in view of the ambiguous nature of the means for producing vividness.

Here, then, on the value of Primacy, we have a complete contradiction of the results contained in the first Table of this report, and a complete agreement with the authorities whose statements were quoted at the beginning of the paper. In both cases presumably, and certainly in the experiments here reported, the difference in the value assigned to Primacy was due to a failure to control the procedure of the *O* during the experiment. In particular, he was allowed to repeat the earlier syllables of a series while the later were being displayed. Thus an opportunity was given for repeating the first syllable a greater number of times than any other. If one remembers a large number of these first syllables, that result might be due to Frequency. In our experiments on Primacy, with which this report opens, the factor of Frequency has been eliminated. The directions given to this end were specific; and each *O* gave his word that he would and did follow them exactly. The introspection offered nothing to invalidate the *O*s' oral assurances. Those who did not fulfill conditions in this respect were not included in compiling results.

Hunter's reference to first loves and first dances suggests that the apparent effect of Primacy might be the real effect of Vividness. However, that the first syllable gained no advantage from this or any other factor is shown by our results.

Summary

The foregoing experiments seem to show that there was good reason to regard Primacy as a factor in association until experimental control was obtained over the *O*'s tendency to "think backward" during the exposure of a series to be remembered. When this tendency was prevented, by the cooperation of the *O*, the alleged effect of Primacy disappeared.

EXPERIENCES DURING LEARNING TO SMOKE

By Christian A. Ruckmick, Wellesley College

The literature on the general topic of smoking tobacco does not appear to contain any studies of the initial experiences while learning to smoke. Careful scrutiny of the bibliography which accompanies the most recent book[1] on the effects of smoking tobacco reveals no title that indicates an analytical account of the earlier experiences which are involved in forming the habit. One of my professional colleagues when questioned about the matter pointed out the desirability of publishing my notes which were taken during the first few attempts at smoking. Few psychologists, he said, were trained in introspective methods when they began the practice of smoking.

Two definite advantages may be seen in making an analysis of consciousness under these circumstances. (1) The effect of tobacco on the psychophysical organism has been observed on men and the lower animals. Certain objective data are available, but the parallel subjective phenomena are not at hand. First evidences of definite mental effects would fortify or contradict assertions made on the basis of purely bodily reaction. This is especially true because, unless caught in the early stages of appearance, the adaptive mechanism of the body later would eliminate both the mental and bodily symptoms. Genetically we should have a picture of this type of consciousness in its progressive stages of development rather than a composite whole in which the elements are fused or lost. The following passage shows a typical approach from the physiological side, but also in several instances the need and the possibility of subjective corroboration.[2]

"Often when physically and mentally fatigued, a few minutes with a pipe has apparently so relieved me that I have been able to read and write without any sense of fatigue for a long time. This I believe to be due to the fact that tobacco smoked in moderate doses contracts the small arteries by stimulating the vasomotor nerves and the unstriped muscle in the vessel walls. This results in a slight rise of blood pressure and a more active arterial circulation. Tobacco has the same effect upon the unstriped muscle of the gastro-intestinal tract. This slight contraction of the smaller blood vessels and increased blood pressure continues for a considerable period of time and is, therefore, a tonic rather than a stimulant effect.

"On the other hand, carried beyond the physiologic effect the toxic effect of tobacco when overused is apparently the direct opposite, inasmuch as it seems to have a depressing effect upon the vasoconstrictor centers and nerves with resulting lower blood pressure, nausea, general relaxation, and the opposite of stimulation or of tonic effect.

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[1]M. V. O'Shea, *Tobacco and Mental Efficiency*, 1923, 237-252.
[2]*Ibid.*, 42-43.

"The statement I have made concerning the tonic effect of tobacco, the stimulation of the vasoconstrictor nerves and of the unstriped muscular fiber, is based upon clinical observation and also upon animal experimentation."

(2) Psychologists have for some time been concerned with the organic group of sensations. There is a considerable amount of interest accumulating concerning the rôle of the motor and organic processes as backgrounds of various experiences like perception, emotion, and thought. I have myself written out introspective accounts of many emotional experiences in which I have found organic sensations of various textures playing a leading part. There is no need to emphasize here the traditional importance of the organic sensations in the theoretical explanation of emotion. Not only in rhythmic perception, where kinaesthesis is fairly prominent, but in ordinary perceptions has analysis shown the prominence of these processes.

"In all the experiments we have found a predominance of kinaesthetic and organic factors over visual sensations and imaginal processes."[3]

In regard to the thought processes as long ago as 1909 Titchener stated that the problem, or *Aufgabe,* as well as the meaning of conscious experiences, may find a vehicle in the kinaesthetic or organic group of sensations:

"I am keenly alive to the importance of organic sensations and, as I shall show in a moment, to that of reduced or schematic kinaesthetic attitudes."[4]

Lately the psychological literature has given birth to statements intimately linking these sensations with the acquisition of meaning in learning experiments.

The apparatus used was a new French briar pipe with redmanol tip. It was therefore not "broken in". Those of my friends who are accomplished smokers have intimated that the apparatus was doubly exacting in its demands on the subject: (1) that a pipe, as compared with a cigar or a cigarette of average strength, presents the severest test to the organism, and (2) that a pipe whose bowl is not caked with burnt tobacco is the worst of all in this respect. The tobacco was an excellent grade of mild, unmixed Virginia leaf. Another condition operated directly against my best intentions, so I was later told: I wanted to weaken the effect by only partially filling the bowl with tobacco. Subsequently I was advised that a full pipe provided a cooler smoke, and that in "breaking in" pipes greater comfort can be obtained by slightly moistening the bowl of the pipe.

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[3]A. S. Rogers, An analytic study of visual perceptions, this JOURNAL, 28, 1917, 519-577.

[4]E. B. Titchener, *Experimental Psychology of the Thought Processes,* 1909, 179; also 180-182.

The observations began with the first trial on June 18, 1923 at 9 a. m. with a room temperature at 81°F. I had just finished lecturing to a large advanced class and found myself in good mental and physical condition, perhaps a bit exhilarated. There were no signs of fatigue, nervousness, discomfort, or irritation. By the time I began to smoke the mood was calm and agreeable.

After a few inspirations, there appeared rather warm and pricky pressures in the throat region, a fairly hot and painful group of sensations on the tip of the tongue, the characteristic odor of tobacco smoke, and a general bodily experience of unpleasant strain. A number of times the throat irritations induced coughing. Very definitely localized strain sensations at the sides of the head then came,—a tension specifically in the temporal region, but apparently just inside the scalp, a gentle pull without quivering or aching. The throat was bound a little more tensely and with slight pain. In about one minute there gradually appeared a form of pleasant dizziness which was felt as a dull suffused ache. It was localized, as nearly as I could make out, at the bottom of the brain-pan. This was one of the most interesting parts of the experience, because it spelled lassitude and pleasant, mild abandon which spread over the entire inner head region and ultimately over the superficial muscles of the body. A drowsy mood soon supervened.

In a few minutes, although the room was only moderately warm, fairly profuse sweating of the forearms set in. Organic sensations referred to the pit of the stomach, or just above the navel, were distinctly felt; they were not painful. They were characterized by a dull muscular pressure, but not so diffuse as is usually felt in the large muscular groups. Soon there were gaseous "rumbles" in the intestinal region with the customary slight organic pressures, and then a fairly distinct call to defecation, which was not heeded and which disappeared without further attention. It was mildly pleasantly toned.

The next sensations that emerged were palmaesthetic[5] vibrations in both hands together with coolness, localized first in the hands and then slowly over neighboring cutaneous areas. A sort of diarrhœal shivering went over the body several times in waves of quick succession. I also noticed that the veins stood out on the backs of the hands and on the lower forearms. A slight tendency to belch was present, and the taste and smell of tobacco were prolonged.

The first period of smoking lasted about 10 min. and was followed by a period of self-satisfied relaxation,—a sort of

[5]K. Dunlap, *Elements of Scientific Psychology*, 1922, 99-101.

vegetative existence, with freedom from strain and effort, an attitude of composure and a feeling of glorified self-importance, which gradually wore off.

At 9 p. m. of the same day the observation was repeated in the same manner and with the same method. There were fewer unusual experiences. Again a slight irritation in the throat and burning sensations on the tip of the tongue occurred. A bitter taste was quite prominent. Sooner than before came a general feeling of relaxation, more adequately described as lassitude and well-being accompanied by mild and pleasantly toned dizziness. No visual "swimming" was apparent, but there was a gentle "reeling" tendency without any distinct motor accompaniments. I could easily walk across the room without any conscious effort to maintain a steady gait, but there was some pleasantly colored anticipation of uncertainty in this regard. A curious anomaly thus appeared. The uncertainty of control should ordinarily be an unpleasant experience; but I am certain that, whenever this feeling came over me, it was welcomed in the sense that I abandoned myself to it. It meant irresponsibility, absence of concern for the immediate personal future, but withal a fairly steady and orderly stream of associations. If I can faithfully recall an experience of slight abandon following the use of intoxicating liquor, I am sure that the state of uncertainty of coordination is there also pleasant; but there is a pronounced difference. The intellectual functions do not reign with the same composure, are not nearly so orderly, nor in general so acceptable as they were in the investigation here described. Naturally each of the instances cited counts for one and not for more than one. The results of future investigations will offer a check against our data.

The palmaesthetic sensations, consisting as before of vibratory tinglings, were now much fainter. No organic complexes were noticeable as distinct experiences, although there was a general organic group of sensations, diffusely localized, which formed a background to the pleasantly toned sensations of mild tension in the head. These kinaesthetic sensations were again vaguely localized just above the nasal passages in the head, roughly below the brain, and occupying a fairly broad area which extended almost to the eyes.

All told, considerable attention was directed throughout to the operation of smoking. Part of this prominence was due, no doubt, to the self-imposed task of analyzing the experience; but much of it was naturally due to the elementary stage of the learning process. On the third occasion, at 9 a. m. of the succeeding day, most of the disturbing items of the experiences described above were lacking. The "biting" and the heat sensations from the tip of the tongue were still there, together

with the bitter sensations from the tobacco. Faint sensations of dizziness appeared, and also a general feeling of calm and well-being. There had been a moment of exhilaration, but it promptly gave way to composure and a tendency to relax the voluntary muscles. In fact muscular tonicity appeared, introspectively at least, to be reduced.

At subsequent trials a feeling of relaxation and pleasant composure remained with the "biting" sensations on the tip of the tongue as the only unpleasant feature. The repetitions of smoking were hereafter irregularly distributed. The "biting" sensations were gradually reduced to zero, and all that persisted was the feeling of bodily relaxation and mental composure, both pleasantly toned, and represented by a very obscure group of organic and kinaesthetic sensations. Of course, as always, the irrelevant tactual and olfactory sensations from the tobacco, together with a slightly bitter gustatory sensation which to me is usually quite pleasant, came with every smoking experience. Ideationally I felt much more "clear-headed", illusory as that may have been, during the operation of smoking. There seemed to be time to consider problems coolly and carefully, without hurry or confusion. The ideas that came seemed for the most part also to win approval, both then and later.

Summarizing, we may remark that it was both interesting and illuminating to observe the processes which passed in review when the human male organism was given a dosage of tobacco in smoked form for the first time in its life. While the physiological eliminating processes were under way, organic and kinaesthetic sensations were prominent and somewhat unpleasant. Relaxation developed at once. At first it took the form of dizziness and slight incapacity for adequate motor coordination; later it became a reduced motor tonicity. At no time was the mind ideationally confused; on the contrary it appeared extraordinarily clear. The mood was then calm and abandoned. The unpleasant features of the experience were practically removed after the third attempt.

A BIBLIOGRAPHY OF RHYTHM
(Third Supplementary List)

By CHRISTIAN A. RUCKMICK, Wellesley College

This list continues the bibliography begun in 1913[1] and supplemented on two subsequent occasions.[2] The entire compilation now comprises, together with the following 189 titles, a total of 714 references to the subject of Rhythm in all of its varied aspects. The introduction to the last supplementary list outlined the arguments for making the scope of the bibliography as broad as possible. No adequate reason for changing our policy in that regard has presented itself. The new compilation therefore contains the more important publications in the field of rhythm as applied to music, literature, aesthetics, and the biological, physiological, and psychological sciences. Small treatises, that were 'rhythmical' in name only, or insignificant musical 'methods,' *e.g.*, 'rhythmical exercises' for piano fingering, where no substantial advance on the subject is made, have been omitted from the list. On the other hand, items that had either been overlooked, or were not known to the compiler when the previous lists were made, are added to the present bibliography. Again, as before, we invite criticism and cooperation. While the catalogues and bibliographical works of reference for the various countries have been searched, and while several large academic and metropolitan libraries have been used to advantage, there must still be important omissions and probably some faulty citations. All such additions and corrections will be welcomed for the next supplementary index.

The compiler has had the friendly assistance of several interested librarians at Wellesley College, at Harvard University, and at the Boston Public Library. Some preliminary work was also done by H. J. Fisher, a graduate student at the State University of Iowa, and by J. Scheidenhelm, a senior at Wellesley College. For the material help of these persons and for the intellectual encouragement of his colleagues who have urged the continuation of the work, the compiler is deeply grateful.

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FRANZ BRENTANO: A BIOGRAPHICAL SKETCH

By Mario Puglisi

Franz Brentano, as his name tells us, was descended from an Italian family.[1] Tradition relates that a knight named Brentanus occupied in 1166 a castle on the river Brenta in the Italian Tyrol. This castle was destroyed toward the end of the year 1250; was rebuilt by Tebaldo da Brenta; and was again destroyed by the Caldomazzo family. There dwelt that Nicolò da Brenta who lived in constant feud with the famous Ezelino da Romano.

The name da Brenta appears for the first time in 1465; later it was changed to Brentano. In the seventeenth century, on account of wars and pestilence, the family of the Brentano di Tramezzo, from whom Franz was descended,—*propter multifaria praeclarissimarum virtutum suarum specimina*, as his family record has it,—left their native land to settle in the Rhenish provinces.

Franz Brentano was born in Marienberg on the Rhine, January 16, 1838. His father, Christian, was a writer of ability; his mother, Emilia Genger, was a pious and highly cultivated lady. In the circle of his nearest relatives are found the illustrious names of Savigny, Clemens Brentano, Sophia Laroche, and Bettina von Arnim. Franz was but thirteen years old when he lost his father. From his early youth he showed an unusual bent for study, and a marked vocation for the religious life, for which his mother lovingly educated him. He went first of all to Berlin, where he lived with his uncle Savigny, to attend the university courses in philosophy. Here he made the acquaintance of Trendelenburg, whom he always regarded with grateful affection,—grateful, in particular, because Trendelenburg taught him to appreciate Aristotle. In the autumn of 1856 he registered in the philosophical faculty of Munich, where he came to know and to esteem the great historian of the church Ignatius Döllinger, at that time one of the greatest orthodox theologians of German Catholic Romanticism, and one of the most subtle and courageous critics to suffer excommunication in the last century. From Munich he went to Tübigen where he took his degree, July 17, 1862. In the same year he published his first work, dedicated in gratitude to his teacher A. Trendelenburg: *Von der mannigfachen Bedeutung des Seienden nach Aristoteles*,—a work which shows how profound a knowledge of the great Greek philosopher Franz Brentano had even then acquired, and

[1]Cf. *Genealogisches Taschenbuch der adeligen Häuser*, Brünn, 1877.

how deep rooted was his predilection for the thinkers of antiquity. On August 6, 1864, he assumed the priestly habit; and in 1866 he became lecturer in the University of Würzburg, where he was the center of a large group of devoted students. But in the spring of 1870 he found himself at the most serious crisis both of his religious life and of his philosophical activity.

If, toward the end of 1864, we could have made our way within the grim, white, silent walls of the Dominican Convent at Graz, and could have watched in his cell the tall figure of the young priest, his pale face crowned with flowing black hair, his eyes full of light and fervour, who passed a great part of the long winter nights in silent adoration; if we could have seen him submitting himself rigorously, to the admiration of the monks, to the unusual severity of their rule; and if we could have seen him again a few years later, tortured by the grief that he had been forced to bring to the heart of a beloved mother, facing the calumny of his enemies and the malice of false friends, abandoning his chair in the university and the many students who had sought his teaching from every quarter of Germany and Austria; we should have thought, and rightly, that a storm of overwhelming violence had passed over his soul. The source of this tremendous upheaval is to be found, as the whole of his later life attests, in his love of truth, in his indomitable will to uphold the truth, and in his readiness to face the consequences, whatever they might be, to which his inexorable logic should lead him. It was in the summer of 1869 that all the world of Catholicism was thrown into agitation by the proposed dogma of the infallibility of the Pope. Germany became the theatre of a strong and well-defined opposition, led not only by Döllinger, to whom we have referred above, but also by able bishops like Ketteler and Hefele, and by learned ecclesiastics like the Benedictine Haneberg. The German bishops held a convention in Fulda, and on that occasion the young Brentano was engaged by Bishop Ketteler to write an historical and philosophical memoir in confutation of the dogma. This work, which was recognised as the best expression of the views of the party opposed to the dogma of infallibility, had a great effect in ecclesiastical circles; but very few laymen knew of it. Brentano pointed out in detail the errors incurred by Popes speaking *ex cathedra*, and brought various arguments against the dogma of infallibility,—the most audacious claim, according to Lacordaire, that has ever been made in the name of Jesus Christ.

The subsequent acceptance by the Church of the dogma which he had thus combatted was a heavy blow to Brentano's faith. It would, nevertheless, be a mistake to regard it as the cause which compelled him, some three years later (April 11,

1873), to lay aside the priestly habit and to effect his formal separation from the church. It was only the occasion, not the cause, of this decisive action.

In the preface to one of his posthumous works, recently published,—*Die Lehre Jesu und ihre bleibende Bedeutung*,—Brentano alludes to his religious crisis; it is the first and only time that he refers to it. "Born of a Catholic family," he writes, "I was led to accept the ecclesiastical life; but later I was obliged to separate myself from the church. My sole reason for taking this step was my wish to serve the higher interests of mankind; and my mature convictions made me realise that such service would be impossible if I followed the path upon which I had originally entered. For me, research has always been a vital necessity; and from the very first I tried, again and again, to resolve, in a satisfactory way, certain apparent contradictions of reason and what is called supernatural revelation. The failure of every effort to resolve these contradictions gave rise, little by little, to grave doubts regarding the truth of religious dogmas. But religious belief had been presented to me as a sacred duty, whose disregard meant eternal punishment; and a doubt regarding the obligation of belief thus appeared to me as a temptation to evil.... All this struggle was repeated over and over; and I should never have come to a decision if it had not been for a certain extraordinary event,—the assembling of the Vatican Council to discuss the infallibility of the Pope. Here a doctrine was at issue which I could still doubt without thereby falling into sin, and over against which I therefore was still free from any bondage that might disturb my conscience and prevent an impartial examination of the subject. I studied it, and the result of my study was the firmest possible persuasion of the untenability of that dogma."

But from the moment that the chuch accepted it, there was not the slightest doubt that, at least on one point, ecclesiastical doctrine would appear to Brentano to stand in conflict with the truth. The scruples that had hitherto withheld him from an examination of other dogmas had now no reason for existence; and with the dogma of infallibility there fell for Brentano the dogmas of the Trinity and of the Incarnation; even the doctrine of the obligation to believe must be subjected to reexamination.

When, in May 1872, Brentano was appointed *professor extraordinarius* in the University of Würzburg, he had not yet formally separated himself from the church. Before he took that step, he thought himself morally bound to resign his chair. Hence in March 1873, a month before his formal withdrawal, he left the university which had called him, as priest, to membership in its faculty.

His next step was to offer himself, as layman, to the University of Vienna, where, a year after his retirement, on January 22, 1874, he was appointed *professor ordinarius*. Soon, however, an unforeseen event changed the course of his lfe. In 1880 he became engaged to Miss Ida Lieben, who, as a Catholic, could not contract in Austria a religious marriage with one who had formerly been an ecclesiastic. Brentano was therefore obliged to assume Saxon citizenship, and consequently to resign the title of *professor ordinarius* in the Austrian university. On September 16 of the same year, 1880, he was married in Leipsic to Miss Lieben. Soon afterward, he returned to Vienna, where his wife's relatives were living, and resumed his teaching in the university,—but now as lecturer only. Two years later, September 30, 1882, he had the unhappiness of losing his mother. Seven years after his marriage, in 1888, he rejoiced in the birth of a son, Johannes; but on March 18, 1894, he suffered the loss of his wife. About nine months after this cruel stroke of fortune, Brentano left Vienna. Then began a long pilgrimage, made more than ever burdensome by a disease of the eyes, which threatened him with gradual loss of sight. On April 8, 1895, he went to Zurich; thence to Lausanne; and from Lausanne into Italy. Here he felt himself at home; he realised that his temperament was Italian; and in 1896, after spending some time in Palermo and Rome, he settled down in Florence. On December 30, 1897, after obtaining Italian citizenship, he contracted a second marriage with Miss Emilia Rueprecht of Vienna. The state of his eyes grew more and more serious, and in 1903 he underwent an operation in Vienna; but, far from regaining his sight, he was obliged to give up all hope of recovering normal vision.

His house in Florence and the villa at Schönbühel by Melk (Austria), to which he retired for some four months of every year during the heat of summer, were the resort of friends and disciples, toward whom he was always generous of hospitality, of counsel, and of assistance. After the outbreak of the European war he, a convinced pacificist, was unwilling to remain in a belligerent country; and he therefore left Florence in 1915 to establish himself in Zurich, where he died of appendicitis, March 17, 1917.

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(40) *Die Lehre Jesu und ihre bleibende Bedeutung.* pp. xviii, 149. Meiner, Leipzig. 1922.

[41] A volume on the Existence of God will shortly be published by the same firm. It will contain some of the lectures delivered by Brentano at the University of Vienna.

[42] A new edition of the *Psychologie* (O. Kraus) is advertised by the same firm as in the press.

THE EFFECT OF VARIED INSTRUCTIONS ON THE PERCEPTION OF DISTANCES IN TERMS OF ARM-MOVEMENT[1]

By Wilbert Andrew Garrison

I. Introduction

Our object in undertaking the experiments which this paper reports was twofold. We wished, first, to work out with Münsterberg's arm-movement apparatus[2] Müller's method for the determination of equivalents by way of constant stimulus-differences;[3] and we wished, secondly, to measure the influence of varied instructions upon the discrimination of distances perceived in terms of arm-movement. We failed in our attempt upon the method; and what we have to present is, accordingly, a set of experiments upon the perception of distances in terms of arm-movement, taken under two different instructions by the ordinary method of constant stimulus-differences.

These experiments were planned as a parallel to those of A. C. Reid on the perception of lifted weights (this Journal, 35, 1924, 53 ff.). It seemed a priori probable that the discrimination of lifted weights would be objectively more accurate under an object-instruction than under a pattern-instruction; we have all been accustomed from childhood to the estimation of weights as objects. The results of Reid's work bore out this expectation. It seemed, on the other hand, at any rate possible that the discrimination of rectilinear distances in terms of arm-movement might be objectively more accurate under a pattern-instruction than under an object-instruction; the perceptions are not of ordinary occurrence in everyday life, and the elbow-pattern is both massive and fairly well-defined. The results of our work only partially bear out this expectation; there is a progressive tendency, as practice is continued, toward the stabilisation of the object-basis of judgment.

Our failure to determine equivalents was due to the fact that, despite the contrary indications of the preliminary practice-series, we never obtained 100% *r*-cases at either extreme of the two stimulus-scales which we employed. We cannot account, except by the general difficulty of the task set to the *O*s, for the failure at the lower extreme of the scales. As regards the upper extreme, two of our *O*s independently reported that the longest distances tended to 'break apart' or to 'fall in two,' so that it was difficult for them to be sure whether they were passing judgment upon the perception of the whole distance or of only the latter portion of it. If these reports afford an adequate explanation, we evidently chose our standard stimulus too large.

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[1]From the Psychological Laboratory of Cornell University.

[2]For a description of the apparatus see E. B. Delabarre, *Ueber Bewegungsempfindungen*, 1891, 74 ff.; E. B. Titchener, *Experimental Psychology*, II., i, 1905, 104 f.; cf. H. Münsterberg, *Beitrage*, iv., 1892, 183 ff.

[3]G. E. Müller, *Gesichtspunkte und Tatsachen der psychophysischen Methodik*, 1904, 199 ff.; Titchener, *op. cit.*, II., ii, 1905, 258 f.

It may be worth while to note expressly that, as we have indicated in the preceding paragraph, the results of the preliminary practice-series were not borne out by those of the regular experimental series. There is, perhaps, a difference in the *O*'s attitude in the two cases. It may be that, in the practice-series, attention is keener; it may also be that judgment is less immediate. The difference seems to call for special investigation, since it undoubtedly means a defect of method.

II. Apparatus and Procedure

Münsterberg's apparatus was designed for a standing *O*. There are two principal objections to this position: bodily fatigue, and the difficulty of maintaining the same attitude relatively to the track during observation. Our *Os* were therefore seated in a chair placed upon a very heavy, solid table. A rigid frame fastened to this table supported a head-rest and a hand-rest. Both rests were adjustable horizontally and vertically. The track, which was always used in a horizontal position, was adjustable both laterally and vertically. A sighting mark on a wall 2.5 m. distant enabled *E* to keep the track in a constant lateral position throughout the series of observations. Permanent marks made it possible to maintain a constant position of the legs of the chair upon the table for every *O*. Felt placed under the base of the instrument prevented any jar. Early in the practice-series the track showed a tendency to vibrate; this tendency was obviated by firmly bracing the upright sheath.

The head-rest and the hand-rest were padded. The hand-rest was so placed that, when *O* brought his hand back to it, his forearm was perpendicular to the track and his index finger was opposite to, and just above, the center of the car. This position enabled *O* to place his finger in the thimble without search. In cold weather *E* warmed the thimble of the car to the normal bodily temperature. Heavy cloth was pasted to the top of the car to prevent contact of *O*'s free fingers with the metal. The wheels were frequently oiled so that the car moved easily and almost noiselessly. Physician's tape was wound around the axles to reduce the concussion when the car came into contact with the stops. Two stops were used, the one firmly clamped at the starting point, and the other, adjustable, at the point of termination of the movement. The pointer carried by the car is attached to the platform of the car and reaches the scale midway between the wheels. To secure accuracy is setting the movable stop, we found it necessary to attach to this stop a pointer so placed as to maintain a fixed relation to the pointer of the car at the termination of the outward movements. The distance between the two pointers was 10 cm. By preliminary tests the most comfortable positions as regards the height of the head-rest and hand-rest, their distances from the track, the height of the track so that *O*'s forearm should be perpendicular to the upper arm, and the position of the chair upon the table so that *O*'s forearm should be perpendicular to the track of the instrument at the starting point were determined for every *O*; these measurements were recorded and kept constant throughout.

We used a muffled bell-metronome to regulate *O*'s movements. The instrument was set to beat 52 per min., and the bell rang at every fourth beat. A long set of preliminary observations enabled the *Os* to coordinate respiration and movement and to make the movement at a fairly uniform rate. The *Os* found that they were able to breathe at the rate of one complete respiration to every four beats of the metronome set as described. This rate therefore remained uniform for all recorded observations.

O seated himself in the chair, placed his head in the head-rest, closed his eyes, and inserted the index-finger of his right hand up to the first joint into the thimble of the car. *E* gave the signal "ready," and *O* at the first ring of the bell, during an inhalation (2 beats of the metronome) moved

the car from the fixed stop, at a sensibly uniform rate, until it came into contact with the movable stop previously set by *E*. *O* then removed his finger from the thimble, and during an exhalation (2 beats) brought his hand back to the hand-rest, while *E* moved the car to the starting point and reset the stop. *E* always had the car at the starting point when *O*'s hand reached the hand-rest, so that *O* could insert his finger immediately into the thimble. *O* at the next ring of the bell made a like set of movements. The two sets of movements constituted a pair. The outward movements of every pair were the basis of a judgment. *O* allowed the interval of one ring (four beats) to pass between successive pairs; during this interval he gave his judgment and *E* made the record and set the stop. Then followed another pair of movements, and so on for 28 pairs, after which *O* rested for 1 min. before he began another set. When there were 11 stimuli, and consequently 44 pairs in a set, a short rest came after 22 pairs as well as at the end of the sets. With 7 stimuli we presented 5 sets (140 pairs) and with 11 stimuli 3 sets (132 pairs) during a period of observation. Any pair within a set or part-set for which *O* gave the judgment "doubtful" was repeated at the end of that set or part-set. *E* gave *O* a warming-up set of 10 pairs at the beginning of every hour of observation. The *O*s observed 3 periods a week, 1 period every second day. If, for any reason, two periods were required on any day, an interval of not less than 1 hour intervened between the periods.[4]

Repeated trials with three *O*s, one of them highly trained, led to the adoption of 26 cm. as the standard stimulus. The preliminary work was done with 5 stimuli, two greater and two less than the standard. For the experimental series proper the number of stimuli was increased first to 7 and later to 11. The difference between the stimuli was 5 mm. for the preliminary work and 3.33 mm. for all subsequent work. Every variable stimulus appeared twice in each time order in every set. The order of the stimuli and the time order were haphazard.

The *O*s worked under two sets of instructions, *A* and *B*. Instruction *A* required them to take the movements as linear distances in space, and *B* required them to discriminate the pressure-patterns which the movements set up in the elbow. One of our most highly trained *O*s made 20 series of observations, 10 of each time-order in hapazard arrangement, in which he gave phenomenological reports. These reports, which showed that the differential experience with the two movements of different extent lay in the region of the elbow, served as the basis of the pattern-instructions.

At the outset *E* gave *O* a typewritten copy of instructions without comment, and asked him daily before beginning the work whether he understood his task. No further questions were asked. The instructions ran as follows.

A. "In this experiment you will make a series of short movements as you have been instructed.

"Think of these movements objectively as linear distances in space; it does not matter how you perceive them psychologically, so long as your attitude is constant. These distances will be presented in pairs, and you will always judge the second distance of the pair in terms of the first.

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[4]This rearrangement was necessary for G, M, and S during the last three periods of observation in the last three series.

"Your judgments are to be: (1) independent, that is, they are to bear upon the relation of the second distance of the given pair to the first distance; and (2) immediate, that is, they are to spring from the direct impression made by the two distances.

"Your judgments will naturally take the form of 'greater', 'equal', and 'less.' If in any particular instance these terms do not satisfy you, or if for any reason you have failed to observe the above instructions, you may report 'doubtful'. The pairs upon which you have made this report 'doubtful' will be repeated later in the series."

B. "In this experiment you will make a series of short movements as you have been instructed.

"Attend to the pressure-patterns which these movements set up in the elbow. The movements will be made in pairs, and you are always to judge the second pressure-pattern of a pair in terms of the first."

The remainder of this instruction was the same as in *A*.

Five *O*s served throughout the entire experiment; a sixth performed all but the last 75 sets. The *O*s were Dr. L. B. Hoisington (H), assistant professor of psychology; Miss G. K. Adams (A); Mr. F. L. Bixby (B); Miss E. A. Möller (M); Mr. M. G. Scheck (S); and the writer (G)[5]; the last five were all graduate students in the department of psychology. Their training as *O*s varied considerably. H was highly trained; A, in the third year of her graduate work, was trained; M and G were moderately trained; B and S had comparatively little training. The *O*s were without practice in the movements required of them, and with the exception of G worked without knowledge of the problem. B did not complete the work.

III. Quantitative Results

The Tables exhibit, in both time-orders, the statistical results of every series for every *O*. Table I. shows the distribution of judgments for the first (AB) and the second (BA) time-orders, and for AB and BA combined, both for 7 and for 11 stimuli; Table II. shows the 'doubtfuls' and the 'general tendency of judgments' for every *O* with 7 and with 11 stimuli; and Table III. gives h'_u, h_u, L'_u, L_u, h'_l, h_l, L'_l, L_l, the interval of uncertainty (I.U.), and the point of subjective equality (P.S.E.) for AB and for BA for every 550 judgments, for the totals of AB and of BA, and for AB and BA combined, for every series of 11 stimuli; and *h*s, *L*s, and I.U. for AB and BA combined for every 700 judgments, and for the totals of 2100, with 7 stimuli. When 7 stimuli were used, three *O*s worked under instruction *A* and three under *B*.

IV. Discussion

Instruction and Attitude

In the discussion of the results the work of H, our best trained and most reliable *O*, is taken as a standard with which to compare other *O*s. H evidently followed instructions within the limits of possibility. He began with *B;* and an examination of his report shows that, after the difficulties of coördinating movement and breathing have been overcome, he is able to describe the characteristics of the pressure-pattern and to give

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[5]When G observed, Mr. A. C. Reid served as *E*.

the basis upon which his judgments were rendered. In the case of *A*, in the second group, he judged the movements as objects of linear extent. The succeeding reports under two different sets of instructions show a tendency for the criteria of the preceding series to carry over into that following. This tendency was particularly strong when *A* followed *B*.

B_1. "I did not follow instructions in the early part of the work through sheer inability to do so. It was wholly, so far as I can say, a matter of attention. The coordination of movement and breathing with the bell of the metronome diverted attention from the pressure-patterns in the elbow. This difficulty vanished after a little, and the instructions came into their right except for occasional momentary lapses.

"The judgment in terms of pattern bases largely on the dimension of extensity, although intensity (density) and quality both shared in the composition of the totally perceived pattern. Although duration as a dimension, so far as I can say, did not come in, yet the pattern formed in time, *i.e.*, showed a typical course of development, growing particularly in extent, although both intensity (density) and quality showed typical changes in the growth. It was not only the increase in the total extent but also characteristic irregularities in extent that carried the meaning of the judgment. Sometimes, although the total extent was relatively great, the looseness of texture or the lack of density meant 'less'. In these cases the characteristic irregularities were absent.

"It was the characteristics of the patterns at the terminus of the experiences that touched off the judgment. In the early part secondary criteria crept in frequently. If the first pattern meant very great or very small, it tended to touch off an absolute judgment.

"The pattern was localized in the inner side of the elbow, and extended downward a short distance, especially with the meaning 'greater'."

A_1. "At first I jumped from one criterion to another in an effort to find something stable on which to base a judgment. There seemed to be a number of secondary cues which worked more or less in combination; but now one predominated, and now another; no one seemed to be equally prominent throughout any considerable number of experiences. With uniform movement sound gave no cue as to length; and when, as infrequently happened, the rate was not uniform, the louder sound might associate to what otherwise came as the longer movement. Visual imagery seemed at first to serve as a basis of judgment, but soon failed because of other conflicting cues.

"I found it difficult to keep out experiences from the elbow. The time-interval seemed at times to be the basis of judgment, but only sporadically. There was a marked tendency at times to make an absolute judgment; *i.e.*, I judged the first as long, short, or medium and then tried to hold the 'set' largely in terms of throat-kinaesthesis and to compare the second with this set.

"Finally I assumed a more passive attitude and allowed the secondary cues to play *en masse*; I got a general impression 'longer', 'equal', or 'shorter' with the second stimulus. There was a strong tendency to use the terms, 'longer', 'equal', and 'shorter' rather than 'greater', 'equal', and 'less'.

"The differences were always rather large, *i. e.*, I could not get a meaning of finely graded differences. To the end I was uncertain of any exact basis for the judgments."

B_2. "I was already familiar with the pattern of pressure from a previous series. But I remembered the pattern as more definite in its different formations than I found it to be. I also found that the immediately preced-

TABLE I

Stim.	Time-Orders AB			BA			Combined		
1	12	37	101	114	33	3	15	70	215
2	15	45	90	98	38	14	29	83	188
3	31	57	62	86	45	19	50	102	148
4	34	81	35	46	61	43	77	142	81
5	60	66	24	47	52	51	111	118	71
6	93	45	12	17	43	90	183	88	29
7	101	38	11	10	49	91	192	87	21
	346	369	335	418	321	311	657	690	753
1	0	1	99	93	3	4	4	4	192
2	0	6	94	88	9	3	3	15	182
3	2	12	86	78	17	5	7	29	164
4	6	16	78	67	24	9	15	40	145
5	16	42	42	42	37	21	37	79	84
6	31	40	29	31	43	26	57	83	60
7	47	37	16	7	37	56	103	74	23
8	70	26	40	8	31	61	131	57	12
9	81	16	3	3	12	85	166	28	6
10	88	9	3	0	10	90	178	19	3
11	96	1	3	1	5	94	190	6	4
	437	206	457	418	228	454	891	434	875
1	0	3	97	92	7	1	1	10	189
2	1	12	87	90	4	6	7	16	177
3	1	17	82	79	16	5	6	33	161
4	8	23	69	67	22	11	19	45	136
5	21	33	46	58	27	15	36	60	104
6	24	42	34	35	41	24	48	83	69
7	60	29	11	21	35	44	104	64	32
8	80	15	5	17	33	50	130	48	22
9	85	13	2	8	25	67	152	38	10
10	97	2	1	1	23	76	173	25	2
11	99	1	0	4	15	81	180	16	4
	476	190	434	472	248	380	856	438	906
1	0	0	50	48	0	2	2	0	98
2	2	2	46	47	2	1	3	4	93
3	1	5	44	40	5	5	6	10	84
4	2	15	33	33	10	7	9	25	66
5	8	20	22	30	13	7	15	33	52
6	15	22	13	19	13	18	33	35	32
7	36	10	4	8	13	29	65	23	12
8	39	8	3	15	6	29	68	14	18
9	48	2	0	7	9	34	82	11	7
10	49	0	1	5	5	40	89	5	6
11	49	0	1	2	3	45	94	3	3
	249	84	217	254	79	217	466	163	471
1	0	1	49	45	5	0	0	6	94
2	1	5	44	38	12	0	1	17	82
3	2	5	43	34	13	3	5	18	77
4	2	47	21	30	14	6	8	41	51
5	10	24	16	22	25	3	13	49	38
6	13	30	7	12	31	7	20	61	19
7	27	20	3	3	38	9	36	58	6
8	38	10	2	4	19	27	65	29	6
9	42	6	2	5	23	22	64	29	7
10	48	1	1	3	11	36	84	12	4
11	48	0	2	2	6	42	90	6	4
	231	129	190	198	197	155	386	326	388
1	8	23	119	112	29	9	17	52	231
2	27	34	89	90	39	21	48	73	179
3	33	34	83	74	53	23	56	87	157
4	30	64	56	37	68	45	75	132	93
5	69	47	34	36	39	75	144	86	70
6	96	35	19	30	26	94	190	61	49
7	119	23	8	21	24	105	224	47	29
	382	260	408	400	278	372	754	538	808
1	0	1	99	99	1	0	0	2	198
2	1	1	98	94	2	4	5	3	192
3	0	5	95	88	4	8	8	9	183
4	4	7	89	83	8	9	13	15	172
5	14	18	68	64	17	19	33	35	132
6	25	34	41	16	44	40	65	78	57
7	77	8	15	19	21	60	137	29	34
8	88	5	7	6	9	85	173	14	13
9	92	6	2	4	0	96	188	6	6
10	98	1	1	1	2	97	195	3	2
11	100	0	0	2	3	95	195	3	2
	499	86	515	476	111	513	1012	197	991
1	1	0	99	99	0	1	2	0	198
2	1	0	99	95	3	2	3	3	194
3	1	3	96	88	2	10	11	5	184
4	3	3	94	83	5	12	15	8	177
5	20	9	71	66	9	25	45	18	134
6	31	33	36	26	43	31	62	76	62
7	73	10	17	20	14	66	139	24	37
8	91	1	8	11	3	86	177	4	19
9	97	1	2	4	3	93	190	4	6
10	99	1	0	4	1	95	194	2	4
11	98	1	1	1	1	98	196	2	2
	515	62	523	497	84	519	1034	146	1020
1	1	0	49	49	1	0	1	1	98
2	1	1	48	43	7	0	1	8	91
3	2	0	48	44	3	3	5	3	92
4	4	3	43	35	4	11	15	7	78
5	5	7	38	21	12	17	22	19	59
6	15	16	19	13	22	15	30	38	32
7	31	8	11	15	11	24	55	19	26
8	42	3	5	4	6	40	82	9	9
9	46	2	2	2	3	45	91	5	4
10	48	2	0	0	2	48	96	4	0
11	49	0	1	1	2	47	96	2	2
	244	42	264	227	73	250	494	115	491
1	0	3	47	49	0	1	1	3	96
2	1	0	49	47	2	1	2	2	96
3	2	0	48	45	4	1	3	4	93
4	7	1	42	32	10	8	15	11	74
5	8	3	39	38	4	8	16	7	77
6	11	21	18	17	18	15	26	39	35
7	37	4	9	11	13	26	63	17	20
8	47	0	3	7	4	39	86	4	10
9	46	1	3	2	1	47	93	2	5
10	47	0	3	1	4	45	92	4	4
11	49	0	1	3	0	47	96	0	4
	255	33	262	252	60	238	493	93	514
1	16	34	100	102	35	13	29	69	202
2	18	41	91	86	49	15	33	90	177
3	31	49	70	89	55	6	37	104	159
4	39	102	9	37	98	15	54	200	46
5	71	63	16	37	40	73	144	103	53
6	120	27	3	26	46	78	198	73	29
7	121	25	4	31	37	82	203	62	35
	416	341	293	408	360	282	698	701	701
1	6	16	78	84	12	4	10	28	162
2	7	16	77	80	19	1	8	35	157
3	6	12	82	67	28	5	11	40	149
4	13	27	60	55	40	5	18	67	115
5	14	45	41	50	46	4	18	91	91
6	15	82	3	18	79	3	18	161	21
7	42	53	5	12	42	46	88	95	17
8	74	24	2	19	28	53	127	52	21
9	86	11	3	14	23	63	149	34	17
10	96	4	0	11	19	70	166	23	11
11	94	5	1	8	13	79	173	18	9
	453	295	352	418	349	333	786	644	770
1	2	6	42	35	12	3	5	18	77
2	3	12	35	35	14	1	4	26	70
3	0	10	40	27	22	1	1	32	67
4	2	23	25	16	29	5	7	52	41
5	1	30	19	9	37	4	5	67	28
6	3	45	2	6	43	1	4	88	8
7	20	27	3	4	33	13	33	60	7
8	35	15	0	2	23	25	60	38	2
9	45	5	0	6	8	36	81	13	6
10	47	2	1	1	8	41	88	10	2
11	50	0	0	4	1	45	95	1	4
	208	175	167	145	230	175	383	405	312
1	10	44	96	78	62	10	20	106	174
2	13	58	79	55	79	16	29	137	134
3	22	78	50	34	96	20	42	174	84
4	4	134	12	13	128	9	13	262	25
5	51	82	17	24	76	50	101	158	41
6	60	72	18	17	73	60	120	145	35
7	91	53	6	12	75	63	154	128	18
	251	521	278	233	589	228	479	1110	511

O	Inst.	Stim.	Time-Orders AB			BA			Com[bined]
M	B_1	1	0	15	85	94	5	1	1
		2	3	23	74	81	18	1	4
		3	0	31	69	75	19	6	6
		4	16	45	39	58	38	4	20
		5	17	54	29	47	46	7	24
		6	22	73	5	17	77	6	28
		7	51	41	8	26	57	17	68
		8	78	21	1	24	43	33	111
		9	90	10	0	14	38	48	138
		10	93	7	0	6	32	62	155
		11	99	1	0	4	23	73	172
			469	321	310	446	396	258	727
	A_2	1	1	18	81	86	12	2	3
		2	6	22	72	73	25	2	8
		3	4	45	51	66	31	3	7
		4	16	44	40	57	37	6	22
		5	18	53	29	46	46	8	26
		6	23	63	14	25	64	11	34
		7	60	30	10	26	51	23	83
		8	67	28	5	25	38	37	104
		9	88	12	0	16	51	33	121
		10	87	10	3	15	38	47	134
		11	94	6	0	12	28	60	154
			464	331	305	447	421	232	696
	B_2	1	0	4	46	45	5	0	0
		2	1	9	40	44	5	1	2
		3	9	17	24	37	11	2	11
		4	8	16	26	36	11	3	11
		5	5	32	13	29	19	2	7
		6	21	26	3	15	30	5	26
		7	28	17	5	18	21	11	39
		8	43	5	2	12	27	11	54
		9	48	2	0	13	20	17	65
		10	45	4	1	8	27	15	60
		11	48	1	1	3	21	26	74
			256	133	161	260	197	93	349
	A_3	1	1	5	44	47	3	0	1
		2	1	5	44	45	5	0	1
		3	3	26	21	37	11	2	5
		4	13	10	27	35	10	5	18
		5	18	19	13	31	16	3	21
		6	25	19	6	17	28	5	30
		7	31	16	3	20	16	4	45
		8	41	8	1	13	22	15	56
		9	44	5	1	7	18	25	69
		10	46	3	1	5	13	32	78
		11	46	4	0	3	13	34	80
			269	120	161	260	155	135	404
S	A_1	1	24	43	83	86	50	14	38
		2	30	55	65	66	63	21	51
		3	32	67	51	59	69	22	54
		4	48	76	26	48	67	35	83
		5	62	64	24	39	66	45	107
		6	81	54	15	20	66	64	145
		7	104	39	7	15	62	73	177
			381	398	271	333	443	274	655
	B_1	1	4	23	73	66	28	6	10
		2	4	35	61	57	33	10	14
		3	8	29	63	51	42	7	15
		4	12	46	42	42	34	24	36
		5	14	50	36	36	43	21	35
		6	31	44	25	19	52	29	60
		7	42	45	13	17	45	38	80
		8	47	45	8	12	47	41	88
		9	62	30	8	12	40	48	110
		10	67	26	7	6	33	61	128
		11	85	15	0	3	29	68	153
			376	388	336	321	426	353	729
	A_2	1	0	16	84	63	23	14	14
		2	3	19	78	54	33	13	16
		3	3	26	71	53	33	14	17
		4	12	39	49	45	42	13	25
		5	16	39	45	37	34	29	45
		6	27	40	33	13	45	42	69
		7	35	43	22	15	34	51	86
		8	46	35	19	13	30	57	103
		9	54	34	12	3	35	62	116
		10	69	26	5	10	27	63	132
		11	73	21	6	5	29	66	139
			338	338	424	311	365	424	762
	B_2	1	2	6	42	28	18	4	6
		2	0	11	39	29	14	7	7
		3	5	11	34	22	17	11	16
		4	12	14	24	19	17	14	26
		5	13	17	20	19	19	12	25
		6	11	18	21	12	19	19	30
		7	11	26	13	12	11	27	38
		8	27	16	7	5	21	24	51
		9	33	12	5	5	9	36	69
		10	32	11	7	2	19	29	61
		11	37	8	5	5	15	30	67
			183	150	217	158	179	213	396
	A_3	1	2	10	38	31	15	4	6
		2	0	11	39	26	18	6	6
		3	2	13	35	25	19	6	8
		4	2	11	37	21	22	7	9
		5	13	12	25	13	21	16	29
		6	8	22	20	15	18	17	25
		7	15	26	9	7	22	21	36
		8	18	23	9	4	23	23	41
		9	29	19	2	7	20	23	52
		10	25	20	5	2	18	30	55
		11	40	8	2	5	16	29	69
			154	175	221	156	212	182	336
G	A_1	1	9	35	106	79	55	26	25
		2	9	53	88	54	84	12	21
		3	14	60	76	40	65	45	59
		4	29	68	53	25	70	45	84
		5	43	63	44	12	55	83	126
		6	52	67	31	10	40	100	152
		7	75	47	28	7	31	112	187
			231	393	426	227	400	423	654
	B_1	1	1	5	94	92	7	1	2
		2	1	3	96	85	7	8	9
		3	1	5	94	76	14	10	11
		4	2	13	85	59	16	25	27
		5	9	17	74	35	20	45	54
		6	27	13	60	24	18	58	85
		7	37	16	47	10	12	78	115
		8	44	16	40	3	13	84	128
		9	54	20	26	4	3	93	147
		10	72	10	18	1	4	95	167
		11	86	8	6	1	1	98	184
			334	126	640	390	115	595	929
	A_2	1	0	4	96	90	8	2	2
		2	1	2	97	81	12	7	8
		3	3	11	86	80	14	6	9
		4	6	17	77	57	20	23	29
		5	11	15	74	45	28	27	38
		6	28	20	52	33	20	47	75
		7	42	23	35	11	14	75	117
		8	64	13	23	4	7	89	153
		9	80	6	14	7	12	81	161
		10	85	7	8	2	6	92	177
		11	96	4	0	2	1	97	193
			416	122	562	412	142	546	962
	B_2	1	1	0	49	49	1	0	1
		2	2	5	43	45	5	0	2
		3	2	2	46	39	8	3	5
		4	2	9	39	34	9	7	9
		5	6	13	31	29	8	13	19
		6	14	8	28	18	13	19	33
		7	25	13	12	11	8	31	56
		8	39	6	5	4	3	43	82
		9	44	2	4	0	4	46	90
		10	46	2	2	1	2	46	92
		11	49	0	1	0	2	48	97
			230	60	260	230	64	256	486
	A_3	1	0	0	50	46	4	0	0
		2	0	5	45	41	8	1	1
		3	1	9	40	36	7	7	8
		4	6	8	36	24	19	7	13
		5	5	14	31	21	16	13	18
		6	17	9	24	18	16	16	33
		7	26	10	14	10	6	34	60
		8	45	3	2	5	4	41	86
		9	45	3	2	2	0	48	93
		10	50	0	0	0	2	48	98
		11	50	0	0	0	0	50	100
			245	61	244	203	82	265	510

TABLE

O	Inst.	Doubtfuls	7 Stimuli Tendency $>$	$=$	$<$	O
A	B_1	183	764	690	646	A
H	B_1	191	782	538	780	H
B	B_1	90	824	701	575	B
M	A_1	85	484	1110	506	M
S	A_1	7	714	841	545	S
G	A_1	9	458	793	849	G

TABLE

Instruction	Time-Order	Groups of 700	Upper h'	h	L'	L
B	AB & BA	1	.228	.684	2.055	.685
	" "	2	.236	.708	1.740	.580
	" "	3	.295	.885	1.284	.468
	" "	Totals	.249	.747	1.732	.577
B	AB & BA	1	.187	.561	2.109	.703
	" "	2	.280	.840	1.059	.353
	" "	3	.321	.963	.834	.278
	" "	Totals	.260	.780	1.242	.414
B	AB & BA	1	.147	.441	2.520	.840
	" "	2	.299	.897	1.188	.396
	" "	3	.328	.984	1.230	.410
	" "	Totals	.252	.756	1.488	.496
A	AB & BA	1	.144	.432	3.822	1.274
	" "	2	.211	.633	3.315	1.105
	" "	3	.234	.702	2.226	.742
	" "	Totals	.196	.587	2.991	.997
A	AB & BA	1	.155	.465	2.424	.808
	" "	2	.202	.606	1.725	.575
	" "	3	.144 –	.431	2.958	.986
	" "	Totals	.165	.494	2.330	.777
A	AB & BA	1	.167	.501	2.847	.949
	" "	2	.243	.729	1.932	.644
	" "	3	.246 +	.739	1.188	.396
	" "	Totals	.218	.655	1.892	.631

Instruction	Time-Order	Groups of 550	Upper h'	h	L'	L
A_1	AB	1	.292	.877	1.118	.373
		2	.367	1.102	1.172	.391
		Totals	.328	.985	1.149	.383
	BA	1	.262	.785	.996	.332
		2	.258	.773	.991	.330
		Totals	.275	.824	.958	.319
	AB & BA		.299	.896	1.047	.349
B_2	AB	1	.387	1.161	.885	.295
		2	.350	1.049	.555	.185
		Totals	.377	1.130	.664	.221
	BA	1	.221	.664	1.703	.568
		2	.211	.632	2.053	.684
		Totals	.221	.663	1.886	.629
	AB & BA		.275	.825	1.258	.419
A_2	AB	1	.381	1.143	.491	.164
	BA	1	.228	.683	1.248	.416
	AB & BA		.287	.862	.861	.287
B_3	AB	1	.342	1.025	.876	.292
	BA	1	.233	.690	2.597	.866
	AB & BA		.278	.853	1.702	.567
A_1	AB	1	.409	1.226	.411	.137
		2	.477	1.430	.601	.200
		Totals	.459	1.377	.508	.169
	BA	1	.321	.935	.576	.192
		2	.344	1.032	.209	.070
		Totals	.328	.985	.387	.129
	AB & BA		.387	1.161	.394	.131
B_2	AB	1	.385	1.156	.328	.109
		2	.410	1.249	.331	.111
		Totals	.421	1.264	.344	.115
	BA	1	.329	.988	.353	.118
		2	.310	.929	.250	.083
		Totals	.336	1.007	.295	.098
	AB & BA		.377	1.130	.323	.108
A_2	AB	1	.267	.800	.640	.213
	BA	1	.285	.856	.445	.148
	AB & BA		.331	.994	.562	.187
B_3	AB	1	.347	1.040	.431	.144
	BA	1	.304	.913	.763	.254
	AB & BA		.335	1.005	.588	.196
$_1$	AB	1	.233	.700	.546	.182
		2	.298	.894	1.281	.427
		Totals	.264	.793	.907	.302
	BA	1	.206	.617	2.656	.885
		2	.245	.736	1.834	.611
		Totals	.228	.684	2.245	.748
	AB & BA		.244	.731	1.567	.522
B_2	AB	1	.290	.870	1.110	.370
	BA	1	.245	.735	1.921	.640
	AB & BA		.276	.827	1.502	.501
B_1	AB	1	.276	.829	.822	.274
		2	.378	1.135	.703	.235
		Totals	.333	.998	.729	.243
	BA	1	.202	.605	3.682	1.227
		2	.219	.656	2.891	.964
		Totals	.226	.678	3.318	1.106
	AB & BA		.265	.796	1.949	.650
$_2$	AB	1	.255	.766	.977	.326
		2	.295	.886	.742	.247
		Totals	.276	.829	.873	.291
	BA	1	.173	.520	4.145	1.382
		2	.173	.520	4.246	1.415
		Totals	.178	.534	4.166	1.389
	AB & BA		.215	.645	2.293	.764
$_2$	AB	1	.289	.866	.350	.117
	BA	1	.158	.473	5.279	1.760
	AB & BA		.166	.498	2.499	.833
$_3$	AB	1	.266	.797	.150	.050
	BA	1	.211	.632	3.250	1.084
	AB & BA		.219	.658	1.622	.541
$_1$	AB	1	.207	.620	1.214	.405
		2	.203	.610	2.806	.939
		Totals	.201	.603	2.003	.668
	BA	1	.140	.447	3.649	1.216
		2	.127	.380	1.858	.619
		Totals	.139	.417	2.809	.936
	AB & BA		.168	.505	2.337	.779
$_2$	AB	1	.206	.619	2.010	.670
		2	.176	.529	2.953	.984
		Totals	.199	.596	2.474	.825
	BA	1	.129	.387	2.228	.743
		2	.132	.396	1.424	.475
		Totals	.131	.394	1.827	.609
	AB & BA		.161	.482	2.191	.730
$_2$	AB	1	.165	.494	2.230	.743
	BA	1	.119	.358	1.964	.655
	AB & BA		.144	.433	2.133	.711
$_3$	AB	1	.183	.549	2.905	.968
	BA	1	.120	.361	2.943	.981
	AB & BA		.151	.453	2.920	.973
$_1$	AB	1	.203	.609	2.841	.947
		2	.307	.922	1.932	.644
		Totals	.258	.775	2.300	.767
	BA	1	.269	.807	–.368	–.123
		2	.319	.958	.686	.229
		Totals	.296	.889	–.441	–.145
	AB & BA		.249	.747	.916	.305
$_2$	AB	1	.327	.981	1.721	.574
		2	.320	.961	.992	.331
		Totals	.291	.872	.942	.314
	BA	1	.287	.862	–.368	–.123
		2	.275	.825	.485	.162
		Totals	.280	.841	.074	.025
	AB & BA		.288	.865	.715	.238
$_2$	AB	1	.324	.973	.843	.281
	BA	1	.317	.951	.385	.128
	AB & BA		.333	.998	.649	.261
$_3$	AB	1	.406	1.218	.616	.205
	BA	1	.330	.989	.174	.058
	AB & BA		.369	1.108	.369	.123

ing attitude tended sometimes to get in the way. Except for this and accidental distractions and an occasional lapse of attention, I maintained the proper attitude.

"As before, the characteristic differences in pattern were density, extensity and quality. A pattern that was more dense, that was more extended in the longitudinal direction, particularly upward, that was more sharply defined, and that showed duller pressure and a slight hint of strain, meant 'greater': a pattern with the opposite characteristics meant 'less'. I gave the judgment 'equal' when the patterns were identical in all respects.

"This attitude was much easier to maintain consistently than the stimulus-attitude."

A_2. "When I came back to the stimulus instruction I did not attempt to sort out any particular aspect of the experience as a basis of judgment. I sought to take up the work where I left off with this instruction. This I was able to do only in part. It was even more difficult than before to rule out (or rather to keep the attention off) the pattern of experience, and very often pressure-experience, while not particularly focal, served as the basis of judgment.

"As before, sometimes one cue and sometimes another seemed to touch off the judgment: as time-interval, expectancy (very rarely), stretch of arm, absolute impression. I sought to take the experiences in a matter-of fact way without bother about the basis of judgment, just to allow the impression to come as it would. This was the usual thing. The attitude to take the experience in a meaningful way was very constant and about the only certain thing about the experience."

B_3. "With this second return to the pattern instruction I had less difficulty than before. Yet infrequently other bases for judgment came up. If these interfered with pattern through a considerable shift of attention, I gave the judgment 'doubtful'. At times they were present, but at a lower level of attention. In such cases they often demanded a judgment contrary to that demanded by pattern, but I always gave the judgment based on pattern. With the best care and training the rate of movement was not always sensibly uniform. This irregularity did not seem, however, to affect the pattern as it stood at the conclusion of the movement. The temporal factor, *i. e.*, rate or mode of formation, did not matter. In general, movement was perhaps sensibly slower under the pattern attitude.

"The same factors, density, extensity, and quality, were characteristic for the different patterns. A pattern of low density, of nebulous, indefinite extent, and of contacty pressure-quality meant 'less; the opposite, 'greater.' All possible shades of gradation of these elements came together, but always tending to vary together. I was able to make out very fine shadings of difference.

"In cases where both experiences meant 'very great,' as sometimes happened, extent and density seemed to be the only difference, and these differences were so slight as to be difficult to detect."

The relative values of the results under each instruction may be expressed by formulas. The sign > is used to denote 'better than;' so that $A > B$ means that the results under instruction A are better than the results under B. The formula designated 'practice' indicates what the results would have been if practice alone had been the determining factor. The 'quantitative' formula indicates the results in terms of h and L. Finally the 'qualitative' formula (as $A' > B'$) shows the ease and accuracy with which O followed the instruction, so far as his reports

afford us an adequate basis of inference. A subscript indicates a contamination; a +, alternation; an =, substitution. *S.C.* indicates secondary criteria, which are always extra-instructional. The subscripts *Exp.* and *Per.* indicate whether secondary criteria were experiences which directly influenced the judgments or perceptions which served as a basis of judgment. Individual variations will be noted where they occur.

The work of H, exhibited in Table III. and interpreted by his reports as given above, may be expressed by the following formulas:

Practice	Quantitative	Qualitative
$B>A$	$B>A$	$B'>A'_B+S.C._{Per.}$

The quantitative formula is written as above, notwithstanding the fact that the h in A is slightly larger than the h in B.

A. The reports of this O show that she succeeded better with A than with B. She began with B, and found the experiment difficult; she frequently complained of pain in the elbow and shoulder, and of exhaustion. She judged in terms of patterns, but had no clear idea what the patterns meant; the meaning of distance did not follow naturally from the pattern, until she undertook the last 1100 observations. In these, however, her hs are generally smaller and her Ls larger than before. In making the movements, the arm of this O rotated slightly at the elbow; this abnormality of movement was due to the structure of the elbow-joint.

B_1. "There were pressure and strain experiences in my elbow and shoulder. It was not as if something stretched between my elbow and shoulder, but that the two experiences came together, side by side. From my elbow I got mostly strain, aches, and bright pressure; from my shoulder dull pressure and drag. The experiences in the elbow tended to stretch across the elbow and the experiences in the shoulder, which were not on the surface but deeper seated, stretched down my arm and had width.

"In my elbow, for judgments of 'greater', the strain stretched and became more nearly in an exact cross direction. The bright pressures increased, became more clear and insistent, and some pain mixed with the experience. The pressure in the shoulder seemed to go further in, to stretch out longer, to become more like drag. For judgments of 'less' there was less strain in the elbow; the pressures were duller, softer, and more fluffy.

"Generalizing, there was more experience with the judgments of 'greater' than with 'less'. The main characteristic of the strain experience was an oiliness, although there was fluffy pressure also. The experiences in the shoulder were duller, deader, more obscure, than those in the elbow, but they took up more room through my arm and were bulkier."

A_1. "Often when I am judging in my own peculiar way, that is, how far my finger *seems* (not *feels* entirely) to move, I make a judgment of 'equal', yet I am perfectly certain from some other cues, I do not know just what they are, that the two distances have not been the same."

B_2. "I had a hard time with these instructions; it was very hard to break off from the stimulus-attitude. I had to set myself postively to do this, and when I did it, I found, at first, that often I had no criterion for

the judgments on the basis of the pattern; that is, in the first part of the series I could tell that the patterns were different, but it was hard to say in which way they were different. Finally I learned that a certain pressure meant less and another greater. After this the judgments came immediately and would have been comparatively easy, if every now and then the cues from the preceding experience had not come in, as my attitude did not remain exactly stable. I think, though, it improved as the series proceeded."

A_2. "I thought of the distance as a line lying in front of me and of my finger moving over the line. It was on how far it seemed that my finger had moved that I based my judgments. These judgments were more natural than those made under the pattern-instruction. I did not have to learn what meant a greater distance and what meant a less distance. Yet I would not say that these judgments were actually surer than those made under the pattern-instruction, for every now and then a line-judgment of which I seemed to be reasonably certain would be contradicted by a pattern which I was unable to keep out of the experience."

B_3. "My judgments were easier in this series than in any of those preceding it. By this time I had realized what the pattern was: a certain pull, drag, pressure in my arm (mostly above the elbow) which came to me as a total experience differently at one time from another. When I learned what this difference meant, what corresponded to 'greater' and what to 'less', I could make fairly satisfactory judgments. I realized now from other experiments that the judgments would, in a way, be grosser than with the stimulus-instructions, that is, the ideas of much greater and much less would not come in to bother me; but as soon as I realized the difference between the two patterns I named the difference and that was all. From the fact that I did not need to discriminate between much greater and much less, I believe, though I am not certain of the fact, that I gave more equal judgments than formerly; but when I gave 'greater 'or 'less', I was certain that it was greater or less. Before beginning each experimental series, I said to myself 'pattern', 'pattern', not with the intention of describing the pattern, but merely to keep it in mind that my judgments were to be in terms of pattern and nothing else."

We have to write two qualitative formulas for A since, according to her reports, alternations did not occur under the repetitions of A and since there was added alternation in the last series under B.

Practice	Quantitative	Qualitative
$B>A$	$A>B$	$A'+S.C._{Per.}>B'+A'$
		$A'>B'+A'+(B'=S.C._{Per.})$

B. The reports of this O, although very meagre, indicate that he followed instruction A but not B. In this part of the work he brought the car against the variable stop with considerable force. Unfortunately bad health enabled him to complete only 75 of the last 150 sets required. Consequently his results are not comparable with those of the other Os.

B. "The pattern was formed in the back of the elbow. I was certain of 'greater', which was judged from a kind of slip in the elbow. The judgment 'less' was made on the position and intensity scale. There seemed to

be a gradation of intensities in the elbow. 'Equal' judgments were made in the same way as the 'less.' In the case of 'doubtful' judgments something else seemed to cut across the patterns."

A. "In this series I had no difficulty in following directions. I judged the pairs as linear extents. The judgments came immediately and I can not say what processes were involved."

We may write B's formulas; tentatively, as follows:

Practice	Quantitative	Qualitative
$B>A$	$B>A$	$A'>B'_{\text{s.c.}}$ Per. & Exp.

In writing the quantitative formula we considered the fact that L_1 and I.U. are smaller in A than in B for the third 1100 observations.

M. The reports of M show that she had recourse to secondary criteria under A, but that her judgments under B were rendered in terms of patterns developed in the elbow. At first she had difficulty in determining what the difference in pattern meant, but this difficulty was soon overcome. Her attitude was more constant under B than under A. The terms 'direct' and 'indirect' in her qualitative formula indicate that, of the experiences which served as the basis of judgment, some entered directly into the judgment and others by way of an intermediate perception; that is, the auditory experience served as an immediate cue to the perception of distance, whereas the experiences of pressure in the throat gave rise to a perception of time which in its turn served as a basis for the perception of distance.

A_1. "I judged by the intensity of the click made by the car coming against the stop. If the click was louder relatively, the distance was judged 'less'; if weak 'greater'; for I moved at a uniform rate. Judgments were rendered in part in relation to the temporal interval. If the intensity of the click and the time-interval corresponded, the judgment of 'equal' was given. The results came spontaneously."

B_1. "If I failed to follow instructions I reported 'doubtful'. The judgments were made in terms of elbow-pattern. If the second was like the first, I said 'equal.' If the pressures of the second were duller, heavier, I said 'greater;' if lighter and brighter, I said 'less;' judging, therefore, not in terms of length but of elbow pattern. The pattern was greater, less, or equal. At times I was not sure of the second pattern or I forgot the first, in which cases I also reported 'doubtful.' While it was at first difficult to judge the patterns, it became easier with practice and was soon practically spontaneous."

A_2. "The judgments under this instruction came quickly. If the second distance could be described in the same vocimotor terms as the first, it was judged 'equal.' The description was twofold, temporal interval required to move along the distance and intensity of the final click. If the temporal interval was longer (*i. e.*, prolonged expectancy-strain) and the final click less intense, the judgment 'greater' was given. A short temporal interval (the limit coming before it was expected according to the vocimotor description) and a more intense click were followed by the judgment 'less'. Failure to follow instructions as well as uncertain cases were reported 'doubtful'."

B_2. "The judgments were made in terms of elbow-pattern. If the second pattern was like the first, the judgment was 'equal'. If the second pattern was a duller, heavier pressure than the first, the judgment was 'greater.' If the second pattern was lighter, quicker, brighter than the first, the judgment was 'less.' Doubtful cases were reported as 'doubtful' as were also those cases in which the instructions were not followed."

A_3. "The distances were compared by means of the temporal interval required to move along them and by the intensity of the bounding click. The rate of movement was of course kept as constant as possible. The first distance in terms of temporal interval and click was held by throat kinaesthesis. If the second distance duplicated this, it was judged 'equal.' If, however, the time-interval was prolonged, the kinaesthetic strain became greater and the second distance was judged 'greater.' The final click of the 'greater' distance was less intense than that of the first distance. If, in the second distance, the throat-kinaesthesis describing the first was interrupted, that is, the time-interval less and the click intensity greater, the distance was judged 'less.' The 'doubtful' judgments were those in which I could not be sure of the duplication of the first experience or in which I failed to follow instructions. The judgments came immediately."

We may write for M the following formulas:

Practice	Quantitative	Qualitative
$A>B$	$B>A$	$B'>(A'=S.C._{Per.\ [\text{direct \& indirect}]})$

S. An examination of the reports of this O shows that instruction A was not understood. His judgments, for the most part, either depended upon B-experiences or resulted from the fact that the second perception exceeded or fell short of an expectancy of equality. He succeeded better under B, but even here experiences are mentioned which could only divert the attention from the pattern as the basis of judgment. The hs, as would be expected, are small, and the Ls large.

A_1. "I followed instructions satisfactorily. I judged from the muscular strain in the whole arm and in the biceps as well. After every excursion I held the arm at the stopping point to get the set of the muscles, skin, and perhaps clothing. The time-element entered, not in any objective way, but as a feeling of expectancy. Sometimes the second interval exceeded the expectancy and sometimes fell short of it, such expectancy being an interval equal to the first presentation. I had a feeling of a moving distance of linear extent. I think the judgment was the result of factors already given, of 'feel' and 'duration.' Another factor entered, that of eye-movement, innervation of the eye-muscles as if the eyes were following the car. Strain of eyes very pronounced at times, but I doubt if actual movement took place."

B_1. "The pattern was very hard to find at first, but I succeeded in detecting a feeling of strain in the elbow-joint mainly on the inside surface of the arm with a slight sensation on the outer surface, probably from the skin. The judgment of 'greater' was made in terms of intensity of strain. The judgment of 'greater' seemed to arise from a more extensive and distinct pattern. Judgments of 'less' were given when the pattern did not stand out as clearly. Concentration of the attention upon the elbow was possible only with strong visual fixation, trying to keep the image of elbow clearly in mind. Conscious eye-strain accompanied this, probably with actual movement of eye-ball."

A_2. "Slight movements of the sleeve which accompanied the gross movements of the arm were noted. Judgments were made in terms of the extension of the arm from the body, duration of movement, and sometimes remoteness of click as the car hit the stop. A feeling of suspense arose if the car did not stop at a point approximately equal (subjectively) to the first distance; the greater the suspense the more certain I was of 'greater.' The premature stopping of the car gave the judgment 'less'."

B_2. "Attention is strongly fixed upon the sensations set up in the elbow. There is a decided tendency to visualize the opening of the elbow-angle as the arm moves out, that is, whether the angle approaches a right angle or is smaller or larger. This visualization was not accompanied by any muscular sensations from eye-movements, sensations which played a large rôle in the earlier series. The elbow was seen as actually forming an angle, and judgments reduced themselves to a comparison, kinaesthetic not visual, of the second extent of angular movement in terms of the extent of the first movement. The attitude was constant throughout, except for occasional wandering when distractions came in. The presence of the pattern was easily detected as long as thought was directed toward the elbow, but eye-movements, when I was conscious of them, were distracting."

A_3. "I tend to think of the hand as out there; apart from the gross muscular movements which probably come in, there is no evidence of pattern set up, except that for greater extension of the arm, there is a feeling that the hand is stretched further out in a frontal plane. There was a strong tendency to visualize the car moving along the track, and see the length of the track covered. When special care was taken to keep the rate of movement uniform, the duration of movement served as a cue in comparison of lengths. Sometimes the car came to a halt before I expected it to, giving rise to a positive judgment of 'less;' and the same thing was true of 'greater.' I do not believe that eye-movement occurred, but a steady fixation straight ahead was most favorable to a ready judgment."

S's formulas may be written:

Practice — Quantitative

$A > B$ — $A > B$

Qualitative

$$B'_{S.C._{Exp.}} + (B' = S.C._{Exp.}) > (A' = [B' = S.C._{Exp.\ \&\ Per.}]) + S.C._{Exp.\ \&\ Per.}$$

G. The reports of this O show that he was unable to keep the two instructions entirely separate. B was especially liable to contamination by A; A was seldom contaminated by B. This is the only O for whom both Ls in any group fell on the same side of the standard.

A_1. "While I was observing I assumed that I followed instructions; but upon reflection I realize that I did not. A part of the time I was unable to attend both to the coördination of breathing with the metronome and to the experiences of the movements. Often my attention was so fully concentrated upon the movements that the beats of the metronome were not noticed. Frequently I was plunged into a sea of uncertainty and underwent a severe struggle with the experiences in an effort to find a satisfactory basis upon which to render a judgment. My attitude shifted from one criterion to another. Such criteria as visual imagery, stretch of arm, and pressure against the terminal stop were tried, and all with dissatisfaction. Sometimes absolute judgments were rendered. Finally I settled upon the feeling of length located out on the track of the apparatus as the one

criterion, and I based the judgments upon this feeling of relative length. Even when one or both of the experiences of a pair seemed weak or vanished quickly, I felt impelled to render a judgment of some kind."

B_1. "In changing from instruction A to B my judgments were at times influenced by A in spite of great care used to follow B. The pattern was at the tip of the elbow in the form of a circular pressure area. The elbow often tingled during the period of observation and continued to hurt for hours afterward, much the same as it would have done had it been struck.

"For judgments of 'greater' the circular pattern elongated, extended toward the hand, and was more or less deeply seated; for judgments of 'equal' the pattern remained circular and nearer the surface; and for judgments of 'less' the pattern was on the surface, less intense, and covered a smaller area.

"A knowledge of the procedure, and especially of the fact that errors were more often made with the longest distances, may have influenced me, in difficult cases, to give the judgment 'less' instead of 'equal'."

A_2. "In returning to instruction A I found it necessary to be constantly on the alert to prevent B from influencing judgment. It was not easy to maintain a constant attitude and keep the movement uniform for both members of the pair. The basis of judgment was the feeling of distance over which the hand moved and 'greater', 'equal', or 'less' expressed the relation of distance between the members of the pair. The discrimination of distance was clearer when the muscles of the hand and forearm were drawn tense during the movements. Confusion arose when both members of a pair were unusually long, and sometimes when the two experiences were different but it was not clear in what respect they were different."

B_2. "The return to B was accompanied by a much greater fatigue in the arm than when B was first taken up, but the experiences were clearer and the judgments were made with greater satisfaction. Occasionally A would enter into the judgments. The pattern experiences were often hard to get, and sometimes a slower movement was necessary to induce any pattern at all.

"The patterns were localized in the elbow, as before, and were possibly a little more intense, deeper seated, and more extended for 'greater;' for 'equal' they were circular and somewhat more superficial; for 'less' they were less pronounced. The judgment 'doubtful' was given when the first experience of the pair was lost and when a difference occurred but the meaning of the difference was not clear."

A_3. "The change from B to A was made with less difficulty than any preceding change. There was a feeling that the attitude was more constant than it had been during any previous set of observations, and a greater concentration of attention upon the movements was possible. There were no secondary cues noticeable by which the distances were judged. A slightly greater speed in the movement of the car was noticeable, and the uncertain long distances occurred less frequently than when the movements were made at a slower rate. The judgments were rendered on the basis of distance as felt."

We write G's formulas as follows:

Practice	Quantitative	Qualitative
$A>B$	$B>A$	$A'>B'_A$+knowledge

Since A, B, and H worked under B and M, S, and G worked under A with both the first and last series, we could not compare the absolute values of h and L. Consequently we determined

whether A, B, and H did relatively better than did M, S, and G when we compared their performances in these initial and final series with what the respective groups did in the entire experiment. We find that the relative value of h is approximately the same throughout, and that A, B, and H have relatively smaller Ls in the first series but not in the last.

Practice

If practice alone had determined the results, the values of h would of course have been progressively larger in passing from one series to the next, and the values of L and I.U. progressively smaller. The quantitative results of A who started with B are better for A than for B, and the quantitative results of M and G who started with A are better for B than for A; consequently factors other than practice influenced their judgments. Moreover, four of the six Os had larger values for h and smaller values for L and I.U. under B than under A.

Every O who completed the work made 8700 observations in addition to the practice and to the warming-up series. So large a number of observations makes it possible to study progressive practice in any one of three ways: (1) by a comparison of the values of h, L, and I.U. within the series (intra-serial practice); (2) by a comparison of the values under the same instruction (practice in homologous series); (3) by a comparison of these values under different instructions (practice in heterogeneous series).

(1) *Intra-serial practice.* The number of observations taken in the various series was such that we fractionated the first three only. H, A, and B began with B, so that two of their fractionated series fall under B and one under A; M, S, and G began with A, so that two of their fractionated series fall under A and one under B. H, M, and G show practice within the first two series; A and B within the first series, and S within the first part of the first series. There is no general improvement shown by any O after the second series.

(2) *Practice in homologous series.* In order to show the effects of practice in these series, we must show that O accepts the instruction of a given series in the same way on his return to it. In the case of H there is a marked practice-effect as we pass from B_1 to B_2 but not as we pass to B_3: there is no indication of practice in the A-series. Since B' stands pure in the qualitative formula and the reports show the impurity of A' to be consistent, we might expect to find an effect of practice showing throughout. This is not the case. In the case of A we find the same result. Her reports state that she did not accept B in the same way on her second return to it. Her A changed in the direction of greater purity; yet here we find nothing which indicates practice. The unfinished results of B reveal no general tendency. M shows practice in the successive repetitions of A, but not in B, notwithstanding its purity. Although A is impure, yet its impurity according to the reports, so far as they go, is consistent. The quantitative results of S reveal no general tendency toward improvement, and his qualitative formula does not indicate that there should be improvement.

G shows a general practice-effect for both A and B. His qualitative formula gives A' pure. Although B' is impure, the contamination again, so far as the reports go, was constant and alternation was infrequent.

Since we found no effect of practice within any series beyond the second, we should hardly expect any consistent effect of practice as we pass from one series to another even under the same instruction. Our analysis reveals that A and H show practice with the first repetition of B; M with the two repetitions of A; G with the successive repetitions of both A and B; and B and S no effect of practice.

(3) *Practice in heterogeneous series.* The results in terms of the values of h, L, and I.U. for the combined time orders enable us to write the following formulas for the individual Os.

H: $B_1 < A_1 < B_2 > A_2 < B_3$, except $A_1 > B_2$ for h_u and hl, and $A_2 > B_3$ for L_u and hl.

A: $B_1 < A_1 > B_2 < A_2 > B_3$, except $A_1 < B_2$ for Ll and $B_2 > A_2$ for hl.

B: $B_1 > A_1 < B_2$, except $B_1 < A_1$ for h_u, and $A_1 > B_2$ for Ll and I.U.

M: $A_1 < B_1 > A_2 < B_2 < A_3$, except $A_2 > B_2$ for hl and L_u.

S: $A_1 > B_1 < A_2 > B_2 < A_3$, except $A_1 < B_1$ for h_u, $B_1 > A_2$ for h_u and hl, $A_2 < B_2$ for L_u, and $B_2 > A_3$ for L_u and I.U.

G: $A_1 < B_1 < A_2 < B_2 < A_3$, except $B_1 > A_2$ for Ll and I.U., $A_2 > B_2$ for L_u and I.U., $B_2 > A_3$ for hl and Ll.

The presence of an effect of practice in heterogenous series would mean that O had assumed a similar attitude under different instructions. None of our qualitative formulas shows this to be true. The quantitative results show several instances of improvement as we pass from one series to another of unlike kind; but the large number of contradictions and exceptions indicate no general practice effect. In the case of G alone there is evidence of progressive practice, and even here the evidence is weakened by the number of exceptions. From the above data we conclude that there is no general practice-effect.

Tendency of judgments

Table II. exhibits the tendency to judge 'greater' or 'less' for every O under each instruction. H in his initial series under B gives practically an equal number of 'greater' and 'less' judgments; in his next three series his tendency is toward an excess of 'less' judgments, but this tendency never rises above 3.9% of the whole number of judgments given in any series; in his last series there is a slight tendency toward 'greater'. There is a consistent tendency under A toward 'less', but under B there is no general tendency. A under B shows a consistent tendency toward 'greater', which becomes markedly less in her last series. Under A her results are contradictory. B shows a tendency toward an excess of 'greater' regardless of instructions, though this excess becomes progressively less during the progress of the work. M in her initial series, A, shows a slight excess of 'less' judgments, but in all her remaining series there is without regard to instructions a progressive tendency toward an excess of 'greater,' rising in one case as high as 23.8%. S in his first two series gives an excess of 'greater' judgments, but in his succeeding series there is an excess of 'less' and this excess continues with very little variation under change of instructions. G shows a

general tendency toward 'less,' and this tendency is greatest where knowledge entered and diminishes in succeeding series; but there is no correlation between tendency and instruction.

From the above analysis and in the light of the reports made by the Os there appears to be no general tendency toward 'greater' or 'less,' either in regard to instructions or in the manner in which the Os received the instructions, except for H under A.

'Doubtful' Judgments

The distribution of 'doubtfuls' may be expected to throw light upon the manner in which the Os accepted the instructions. H gave, progressively, relatively fewer 'doubtfuls' under B, but the decline was gradual. He gave the largest number (15.6%) under A_1 which offered considerable difficulty, but the number of 'doubtfuls' under A_2 fell to the lowest per cent. given by this O in any series. A gave progressively more 'doubtfuls' in her successive series until we come to the last, and her report states clearly the shift of her basis of judgment in this series. B gave progressively fewer 'doubtfuls' with the successive series. M gave the same relative number (4%) of 'doubtfuls' for every A-series. She gave about three times as many 'doubtfuls' under B_1 where she found difficulty in making the judgments, but the number decreased one-half on the repetition of B. S, after the first series, gave progressively fewer 'doubtfuls', relatively, till we come to the last series where there is a slight increase. G gave few 'doubtfuls' in his first series. In the next three series the relative number remains constant, and drops somewhat in the last series.

Every O, except B and S, gave a sufficiently large number of 'doubtfuls' to indicate that he assumed a definite attitude and that he knew he was to give a sign whenever by a shift of attention or by the intrusion of some distraction this attitude changed.

Only a few Os made comments, and these very seldom, when they gave 'doubtfuls'. Occasionally an O reported a shift of attention, or remarked that the experiences were different but that he could not tell in what way they were different.

The 'doubtfuls' were tabulated in regard to the judgments they followed in both time-orders, and in regard to the stimuli with which the judgments occurred; but no general tendency was found except a tendency to accumulate with stimuli in the neighborhood of the standard.

V. Conclusions

From the quantitative results it appears that in the perception of distances in terms of arm-movement a change of instruction does not necessarily bring about a corresponding change of attitude in the Os.

The values of L are relatively larger under A (object instruction) than under B (pattern instruction) in the early series. In the later series, after practice, the values of L under A are relatively as small as under B.

The values of h under the two instructions are not relatively different.

The 'tendency of judgment' supports the conclusion that a change of instruction does not necessarily bring about a change of attitude in the Os; a large number of 'doubtfuls' indicates constancy of attitude.

No effect of practice occurs for any O after the first 4300 observations.

FURTHER STUDIES OF HENNING'S SYSTEM OF OLFACTORY QUALITIES[1]

By A. ELME FINDLEY

Dimmick[2] and Macdonald[3] have sought experimentally to evaluate Henning's theory of olfactory quality.[4] Macdonald attempted to test simultaneously both the qualitative theory and the chemical theory by choosing as representative stimuli substances whose structural formulas were definitely known and which could thus be brought into relation with the chemical theory. He was able only approximately to verify Henning's conclusions in this manner. After Macdonald's paper had been completed and before its publication, Dimmick issued a note which indicated on its face value a considerable verification of Henning's qualitative theory. It was impossible, however, to resolve the apparent divergence between Dimmick's and Macdonald's results, because Macdonald, in selecting stimuli of determined chemical composition and structure, had chosen very few of the stimuli which Dimmick used.

In order to press the test of the smell prism further, the present writer decided to repeat Macdonald's work using the stimuli which Dimmick had found to give the greatest degree of constancy among his *Os*. It seemed as if the best test of the prismatic theory of smell at the present time would be to see whether any set of stimuli could be made with reasonable constancy to fall under it. If the stage were set as favorably as possible for the theory, then a failure to demonstrate it would be fatal.

Dimmick's Experiment

In examining Dimmick's work for the purpose of selecting representative stimuli, it appears that Dimmick has in some cases given the benefit of the doubt to the verification of Henning's theory classifications. His Table II gives the impression of a fairly high degree of agreement with Henning, but inspection shows that his Table II does not follow consistently from his Table I, from which it is supposed to be derived.

In the first place it should be noted that the great scattering which appears in Table I makes it difficult to convert it into Table II where the low frequencies must be totally disregarded and the high frequencies accepted, as characterizing the stimulus. It will inevitably be a matter of opinion just where the line between high and low should be drawn. Seventeen of the 56 classifications of Dimmick's Table II are certainly unjustifiable. Anise oil, for instance, is classified by Dimmick as SR, although most of the classifications of the *Os* were for E and there were many for F and none at all for R. Musk is classified as ERPB, whereas it should be classified as ESRB since there are three times as many S judgments as P.

[1]From the Harvard Psychological Laboratory. Communicated by E. G. Boring. This paper uses Macdonald's symbols for Henning's six principal olfactory classes: F = fragrant, E = ethereal, P = putrid, S = spicy, R = resinous, and B = burned.

[2]F. L. Dimmick, Note on Henning's Smell Series, this JOURNAL, 33, 1922, 423-425.

[3]M. K. Macdonald, Experimental Study of Henning's System of Olfactory Qualities, this JOURNAL, 33, 1922, 535-553.

[4]H. Henning, *Der Geruch*, 1916. On the qualitative theory see esp. 80-97; on the chemical theory see esp. 291-301.

Other similar instances are hops, cumarin, clove, marjoram, sassafras, nutmeg, myrrh, lactone, apiol, mustard oil and arbor vitae. Less striking errors of derivation are the cases in which Dimmick has selected one of two equal frequencies and has neglected the other: vanilla, lavender, cassia, and acetic ether.

Because of these inaccuracies in Dimmick's Table II the present writer has redetermined the classification of Dimmick's stimuli from his Table I. In such a determination it is necessary to select some arbitrary rule; and the following procedure seems to give a classification that summarises the results of Dimmick's Table I adequately. In classifying a stimulus from Dimmick's Table I one writes down with a capital letter the class in which the stimulus was placed the greatest number of times and one also writes with capitals those other classes in which the percentile frequency of classification is less than 10% below the percentile frequency of the maximal classification. Then one adds in small letters the names of all other classes in which the stimulus was placed with a frequency greater than one-third of the frequency of the maximal class. Other classifications are omitted. This procedure gives a rough picture of the results of Dimmick's Table I in terms of two degrees of emphasis. It would be possible to make up other rules, and the reader who questions the present procedure must satisfy himself of it by trial. The present rule at any rate is objective and treats all stimuli alike without respect to the relation of the results to Henning's theory. Table I of this Note results.

TABLE I

Classification of Dimmick's 76 stimuli, derived from Dimmick's Table I. F=fragrant, E=ethereal, P=putrid, S=spicy, R=resinous, B=burned. See text

F: jasmine oil, apple blossom.
E: lemon oil, amyl-valerate, amyl-acetate.
P: fish soap, glue, hydrogen sulphide, asafoetida.
S: clove oil, thyme flowers, cassia oil, pepper, cinnamon, marjoram, nutmeg oil, celery seed, dill seed.
R: Canada balsam, decar oil, pine needle oil, sandarak gum, spikenard oil, turpentine, cajuput oil, eucalyptus oil.
B: tar, guiacol.

fE: orange oil.
EP: arnica tincture, ammonium-valerate.
sR: camphor gum, rosemary oil, bergamot oil.
Rb: origanum oil, xylol, toluol.
bS: coffee.
FS: tonka bean. fS: camomile flowers, sage.
*Fb: musk root extract.
*eS: menthol.
ER: acetic ether, acetone. eR: juniper oil.
*PR: benzol.
Pb: pyridine.
*PS: hop flowers.

Fes: cumarin. FeS: bay oil. fEs: vanilla, anise oil. fES: sassafras oil. feS: lavender flowers.
feR: ethyl ether.
fSr: copaiba balsam.
eSr: peppermint oil. eSR: caraway oil, cardamom. esR: lavender oil.
ERb: collodion.
SRb: tansy oil, wormwood oil. srB: apiol.
**Psr: mustard oil.

Fesr: geranium oil. FEsR: ginger. fEsr: spearmint oil. fEsR: citronella oil. fESR: fennel oil. feSr: juniper berry.
***FPSr: lactone.
***ESRB: musk. ESRb: arbor vitae extract.
****FESrB: myrrh tincture.

*Diagonal duplex; impossible on Henning's theory but admitted by Henning.
**Triplex not in surface; impossible on Henning's theory.
***Quadruplex not in surface; impossible on Henning's theory.
****Pentaplex not in surface; impossible on Henning's theory.

Inspection of our Table I shows that Henning's prismatic theory holds in general for Dimmick's results. There are 76 stimuli in all. Five of these stimuli give duplex qualities for diagonal corners of a face, and 13 of them give triplex qualities for the three corners of a single square face. Macdonald has already pointed out that triplex ordors in a face or duplex odors for opposite corners of a face do not properly accord with Henning's theory; but Henning himself admits such odors, and there is nothing further now to say about the difficulty.[5]

It is interesting to note that apiol, tansy oil and wormwood oil, by this treatment, are placed in the triangular SPB end of the prism, where Henning has given us no examples. We are still without instances of stimuli that belong in the other end, FEP.

Of these 76 stimuli five, on this treatment, refuse to fall in the surface of the prism. Myrrh is FESrB; lactone is FPSr; mustard oil is Psr; musk is ESRB; and arbor vitae is ESRb. All of these combinations are impossible because they would lie somewhere in the inside of the prism which, on Henning's theory, is hollow. The entire showing appears, however, to favor Henning's theory quite as much as Dimmick's selection of stimuli in his Table II.

Procedure

On the basis of Dimmick's results (*cf.* Table I) the following stimuli were selected as representative of the 6 principal classes: F, jasmine oil; E, lemon oil; S, cinnamon; R, turpentine; B, tar oil; P, hydrogen sulphide (a saturated water solution of the gas made fresh every day). Dimmick's Table I shows that these 6 stimuli were placed within these respective classes with very great consistency; the percentages of judgments for these classes are respectively 94%, 94%, 88%, 83%, 88%, and 94%. The average maximal percentage for the 76 stimuli is only 56%.

As comparison stimuli, to be placed with respect to these standards, we chose 13 substances which seemed from Dimmick's table to have a high degree of consistency and which should, from the indications of Dimmick's Table II, have scattered as well over the surface of the prism as would be possible. These stimuli, with Dimmick's classification, are as follows: orange oil, FE; ammonium valerate, EP; tonka bean, FS; ethyl ether, ER; pyridine (only about 4 cc., since more is choking), PB; cardamom, SR; xylol, RB; coffee, SB; amyl-acetate, FEP; tansy oil, SRB; oil of peppermint, FESR; benzol, EPRB; hop flowers, FPSB. Had Dimmick's classification been final, we should have had an example of every one of the possible 9 duplex odors except FP and the equivocal duplex odors for opposite corners of a face; and we should also have had one triplex odor from each triangular end and one quadruplex odor from each of the three square faces. Unfortunately, we did not discover the error in Dimmick's second table until we had proceeded with the experiment. Nevertheless the stimuli represent a good scatter. Our Table I shows that they can be classified, respectively in the order listed above, fE, EP, FS, feR, Pb, eSR, Rb, bS, E, SRb, eSr, PR, PS.

[5]Macdonald, *op. cit.*, 551.

We proceeded as did Macdonald by the method of paired comparisons, comparing every one of the 13 comparison stimuli with every paired combination of the 6 standards. There are 15 possible pairs of standards. The stimuli were placed in wide-mouthed bottles covered with paper and were presented for dirhinic smelling. The *O*s handled neither the bottles nor the stoppers, and they worked with closed eyes. *E* presented first a standard, then a comparison stimulus, then a second standard: *O* was required to give a judgment under the following instructions.

"You will be presented successively with three olfactory stimuli of different qualities. You are to state whether the olfactory qualitative similarity between the second and third is greater than, equal to, or less than the olfactory qualitative similarity between the first and the second. You are to make judgments only on the basis of the olfactory quality of the stimuli. Disregard entirely all other criteria, such as intensity, affective tone, or cutaneous quality (pressure, pain, warmth, cold, or cutaneous qualities described as 'sticking,' 'penetrating,' 'stinging,' *etc.*). The three stimuli will be presented only once. Do not, however, try to force a judgment, or to guess if you are in doubt or if the task for any other reason seems impossible."

When the *O*s failed to make a judgment, *E* passed to the next presentation, but presented later the stimuli for which the failure had occurred. The failures were noted usually as due to distraction, and sometimes as due to fatigue, but judgments for all presentations were obtained from all *O*s ultimately.

There are 195 judgments when 13 comparison stimuli are judged in relation to 15 pairs of 6 standard stimuli. The order of presentation was the same for all *O*s in these 195 judgments. When they had completed the series, the work was repeated with the order of presentation reversed.

There were ten *O*s. Miss R. S. Murphy, Mr. L. R. Frazier, Mr. C. Goldthwaite, Mr. W. S. Hulin, Mr. T. Karwoski, and Mr. P. S. Young were graduate students of considerable observational training in the Psychological Laboratory. Miss I. L. Barnum, Miss E. Cronheimer, Miss N. N. Granton, and Miss L. Hayward were undergraduates in the Psychological Laboratory. The total number of judgments upon which this study is based is therefore 3900.

Results

The results of the experiment are given in Table II. There were 10 *O*s: every stimulus was compared twice with every pair of the standards. Thus the judgments in each cell must total 20. A cell shows how often the comparison stimulus was judged more like one standard than the other.

The striking thing about this table is the equivocality of the judgments. If the *O*s had been perfectly consistent, the numbers entered in each cell would be 0 and 20 (except in the case of equal similarity). If the judgments were determined simply by "chance" the numbers would presumably be 10 and 10. The nearest approach to univocality is for peppermint compared with the P and S standards: 19½ and ½. There is one score of 19, two of 18, and several of 16. In general, however, the differences are closer to "chance" than to perfect consistency. The average maximal score for all the cells of the table is 12.9; *i.e.*, on the average the degree of consistency is about 65%, where 50% is chance. These results are in accord with Macdonald's findings, in spite of the fact that we chose from Dimmick's results stimuli which in Dimmick's experiment gave the greatest consistency. In other words, we prejudiced the experiment toward precision and failed to find it. It seems, therefore, impossible with the methods available at present to get as much precision in the qualitative classification of odors as is possible, for example, with colors.

TABLE II

Qualitative similarities of olfactory stimuli to paired F, E, P, S, R, and B standard stimuli. M
ments for each of 10 observers; thus 20 judgments in each c

Comparison Stimuli		Pairs of Standard Stimuli																			
		F	S	F	E	F	R	S	R	E	S	E	R	E	P	E	B	R	B	P	R
1.	Amyl-acetate	8	12	8	12	16½	3½	11½	8½	9½	10½	11½	8½	12	8	14	6	10	10	5	15
2.	Orange oil	11	9	6½	13½	14	6	9	11	12½	7½	13	7	16½	3½	15½	4½	13	7	3	17
3.	Tonka bean	9½	10½	11½	8½	10½	9½	10	10	7	13	13½	6½	14½	5½	14	6	9	11	5½	14½
4.	Cardamom	12½	7½	12½	7½	11	9	10	10	10	10	14	6	14½	5½	14	6	13	7	2	18
5.	Ethyl ether	13½	6½	10½	9½	9	11	9	11	14	6	12½	7½	11	9	11½	8½	11	9	8½	11½
6.	Tansy oil	10½	9½	10	10	9½	10½	7½	12½	12½	7½	8½	11½	14	6	9½	10½	15	5	3	17
7.	Peppermint	10	10	10½	9½	6½	13½	9	11	12½	7½	10	10	16½	3½	13½	6½	13	7	3	17
8.	Hop flowers	11½	8½	12	8	8½	11½	9	11	6½	13½	7½	12½	13½	6½	10½	9½	10½	9½	6½	13½
9.	Ammon. val.	11	9	13	7	8	12	7	13	13	7	5	15	8½	11½	6	14	8	12	11½	8½
10.	Xylol	15½	4½	12	8	6	14	3½	16½	11	9	5	15	5	15	6½	13½	13	7	6½	13½
11.	Benzol	10½	9½	12½	7½	5	15	6½	13½	10	10	10½	9½	9	11	10½	9½	8½	11½	6	14
12.	Pyridine	11	9	13	7	8	12	5½	14½	7½	12½	8½	11½	6	14	5½	14½	8½	11½	16½	3½
13.	Coffee	7	13	14	6	9½	10½	12½	7½	10½	9½	7	13	10	10	5½	14½	10	10	8½	11½

TABLE III

Position of comparison stimuli within the surface of the prism. The columns of coordinates give the best position for each stimulus as determined by inspection of data derived from Table II. The figures are percentages of the length of an edge of the prism. The columns at the right show the classification of stimuli based on these results and on the experiments of Henning and of Dimmick

	Stimulus	Coordinates				Classification		
		Horizontal			Vert.			
		F-E | | S-R	E-P | | R-B	P-F | | B-S	FEP ||| SRB	Findley	Dimmick	Henning
1.	Amyl-acetate	35			83	Fe	E	—
2.	Orange oil	70			85	fE	fE	E
3.	Tonka bean	30			47	FeSr	FS	F
4.	Cardamom	50			60	FESR	eSR	SR
5.	Ethyl ether	57			53	FESR	feR	ER
6.	Tansy oil	57			47	FESR	sRb	FESR
7.	Peppermint	63			50	fEsR	eSr	FESR
8.	Hop flowers	35			23	fSr	PS	FS
9.	Ammon. val.		55		65	EPrb	EP	—
10.	Xylol		36		35	eRb	Rb	RB
11.	Benzol		63		33	prB	pR	RB
12.	Pyridine			12	58	PB	Pb	B
13.	Coffee			58	18	BS	BS	BS

F	Fe	FE	fE	E
Fs	Fes	FEsr	fEr	Er
FS	FeSr	FESR	fEsR	ER
fS	fSr	feSR	esR	eR
S	Sr	SR	sR	R

Fig. 1

Although the presence of such great variability prevents too definite conclusions with respect to the qualitative system of odors, we sought to go further and to see how well our odors could be made to fit Henning's prismatic theory. Again we prejudiced the procedure in favor of the theory. We assumed that the odors must lie in the surface of a triangular prism in which the three faces are squares. We then placed each odor in this surface, developed on paper, in the place where the observational results would give the least total deviation. We made this placement by inspection merely. The mathematics involved in an adjustment by the method of least squares is too laborious for the data at hand. Table III gives in terms of coordinates the placement thus determined by inspection. The first three columns of figures show the face in which the particular odor was placed and its horizontal distance from the left hand edge if the corners of the face are as indicated at the tops of these columns, *i.e.*, as if the prism were viewed standing on its triangular SRB base. The fourth column of figures gives the distance that each of the odors was placed above the SRB base. There seemed to be no need for placing odors in the triangular ends.

The character of each odor, as indicated by our results, is given in the first column under the heading "classification." For characterizing the odors in this column we resorted to the schema of Fig. 1 for the FESR face, and to similar schemata for the other two faces. In the next column is Dimmick's classification as restated in Table I of this Note, and in the last column is Henning's classification.

An inspection of these last three columns shows that, in the case of these 13 stimuli, there is considerable agreement between the three investigations. Henning's classification was not precise and depends upon interpretation of his statements. Between Dimmick and ourselves there is generally only a difference of emphasis: Dimmick finds Pb where we find PB; or, more extremely, Dimmick finds E where we find Fe, or eSR where we find FESR. The most extreme differences are for tansy oil, which for Dimmick should be sRb and for us is FESR, and hop flowers which for Dimmick should be PS and for us is fSr. These two stimuli are placed in different faces by the two experiments.

TABLE IV

Deviation of theoretical localization (best adjustment within surface of triangular prism) from observed localization of comparison stimuli. The numbers of the Table are distances expressed in percentages of the line connecting the standard (principal) stimuli; see text

Pairs of Standard Stimuli

	Comparison	FESR						EPRB					PFBS			
	Stimuli	FS	FE	FR	SR	ES	ER	EP	EB	RB	PR	PB	PF	PS	BS	FB
FESR	Amyl-acetate	29	22	10	3	10	5	12	4	14	13	24	13	7	4	5
	Orange oil	6	1	13	1	13	7	3	5	28	26	26	19	14	14	3
	Tonka bean	6	2	9	10	3	19	5	6	25	4	2	67	0	10	2
	Cardamom	7	12	1	0	5	15	1	2	2	56	13	16	10	6	3
	Ethyl ether	6	16	3	2	15	10	14	14	14	12	22	9	18	35	34
	Tansy oil	4	3	3	9	11	5	1	26	5	14	12	12	13	12	3
	Peppermint	1	8	10	1	6	1	12	3	5	14	25	9	29	2	12
	Hop flowers	24	7	0	16	3	2	3	10	19	5	12	2	11	26	11
EPRB	Ammon. val.	4	39	5	1	7	31	3	24	8	3	19	7	22	0	4
	Xylol	29	27	5	8	10	15	30	16	6	3	1	6	4	5	26
	Benzol	4	28	4	3	15	8	14	12	1	18	3	12	18	5	10
PFBS	Pyridine	3	1	27	39	2	8	3	5	11	8	20	2	0	9	2
	Coffee	3	6	12	14	31	11	14	3	22	18	17	13	1	5	1

Table IV shows the agreement of our inspectional placements with the prismatic theory. The coordinates of Table III take the length of an edge of the prism as equal to 100. Using these units we measured the distance of these theoretical placements from every one of the six corners. In measuring to corners in a face that was not the face in which the odor in question had been placed, we took the shortest linear distance through the surface (*i.e.*, the straight line through the prism developed on paper). Thus, horizontally, no corner could be more than 150 units away from the ideal placement. We now determined in the case of every pair of corners the percentage that the distance of the more remote corner from an odor was of the sum of this distance and the distance of the less remote corner. We also computed from Table II the percentage that the number of judgments in favor of one standard was of the total number of judgments (20). If consistency were perfect these two percentages, a "theoretical" and an observed percentage, should be the same.

The procedure may be clearer to the reader if illustrated in a concrete case. Amyl-acetate lies in the FESR face, 35% of the length of a side from the FS toward the ER edge, and 83% of the length of a side from the SR line toward the FE line (Table III). If we compare it with the FS pair of standards we find that the distance from the theoretical placement to the S corner is 69% of the sum of this distance and the distance to the F corner, *i.e.*, the theoretical placement is nearer F than S in the ratio 31:69. We find, however, from Table II that amyl-acetate was judged 12 times more like the S standard than the F, and 8 times more like the F standard than the S; therefore observationally it is more like S than F in the ratio 60: 40. The observational finding then places the similarity of amyl-acetate to F at 40%, whereas the "theoretical" finding (the finding that should give the least deviation for all cases) places it at 69. The difference between 40 and 69 is 29 and this figure is the value of the deviation of observation from theory entered in Table IV.[6]

TABLE V

Averages from Table IV

Comparison Stimuli	Avs. for FESR face	Avs. for EPRB face	Avs. for PFBS face	Avs. for entire figure
Amyl-acetate	13.1	12	13.6	11.6
Orange oil	6.8	15.8	13.6	11.9
Tonka bean	8.1	10.1	14.5	11.3
Cardamom	6.6	14.8	9.1	9.9
Ethyl ether	8.6	14.3	20.6	14.9
Tansy oil	5.8	10.5	9.3	8.9
Peppermint	4.5	10	11.3	9.2
Hop flowers	8.6	8.5	14.3	10.2
Ammon. val.	14.5	14.6	9.3	11.8
Xylol	15.6	11.8	11.8	12.7
Benzol	10.3	9.2	8.6	10.3
Pyridine	13.3	9.1	6	9.3
Coffee	12.8	14.1	6.6	11.4
Av. of FESR stimuli	*7.8*	13.5	13.1	Av. of avs. = 11.03
Av. of EPRB stimuli	12	*11.9*	11.6	
Av. of PFBS stimuli	13.3	9.9	*6*	

[6]This method of computation is not the same as projecting the placement upon the sides of the square, but seems to accord better with the meaning of the figure, for which it is not possible to say, *e.g.*, that S:R::F:E in the sense of qualitative relationships.

Table V gives averages derived from Table IV. The last column shows the general averages of the deviations of "theory" from observation for every stimulus compared with the 15 pairs of standards. These values range from 8.9% to 14.9%, and the average of averages is about 11%. One can show mathematically that two points placed independently at random upon a limited straight line will on the average be separated from one another by a distance equal to ⅓ of the length of the line. Hence a "chance" deviation would give an average of 33, whereas our finding is 11. Such a conclusion supplements Macdonald's finding, namely, that the verification of Henning's prismatic theory can be demonstrated to a degree that is represented by a figure between 30% and 40%.

It occurred to us that we perhaps ought not to expect the same degree of precision in comparing odors with very dissimilar standards that we might find in comparing them with similar standards. Greater precision might be found if the standards included in the face in which the odor had been placed were treated separately. The results of such a treatment are shown in the italicized numbers at the bottom of Table V. Stimuli in the FESR face show an average deviation of "theory" from observation of only 7.8 when only F, E, S, and R standards are considered. The corresponding value is 11.9 for the EPRB face and 6 for the PFBS face. Nevertheless, when it is remembered that chance deviation should be 33, it will be seen that the consistency is still not very high.

We have therefore to conclude that, even when stimuli which show a relatively high degree of consistency in being placed in accordance with Henning's system are used, and when they are placed in the surface of the prism in the way to do the least violation to Henning's system, we are still unable to establish the validity of the system with any great degree of precision.

Affective tone

As an incidental matter we sought to get some indication of the affective value of our stimuli. We asked every one of the 10 *O*s to make absolute affective judgments upon the 13 comparison stimuli and the 6 standards,—19 stimuli in all. They were to assign +2 to the odor if it was "very pleasant," +1 if it was "more pleasant than unpleasant," 0 if it was "indifferent," −1 if it was "more unpleasant than pleasant," and −2 if it was "very unpleasant." The averages for the 10 *O*s are given in Table VI.

TABLE VI

Affective values of stimuli on an absolute scale of judgment from −2 = "very unpleasant" to +2 = "very pleasant." Avs. of 10 *O*s

FESR face	Tonka bean	1.4	EPRB and	Coffee	.9
	Orange oil	1.4	FPSB faces	Xylol	−1.0
Av. = .7	Cardamom	1.1	Av. = −.7	Benzol	−1.0
	Peppermint	1.0		Ammon. val.	−1.7
	Amyl-acetate	1.0			
	Tansy oil	.5			
	Hop flowers	.5			
	Ethyl ether	−1.3			
FR and ER	Lemon oil	1.6	PB line	Tar oil	−1.0
lines	Cinnamon	1.5	Av. = −1.6	Pyridine	−1.8
Av. = 1.1	Jasmine oil	1.3		Hydrog.	
	Turpentine	.2		sulph.	−2.0

Henning states that the FESR face tends to be pleasant and the PB line unpleasant. A critical plane may be passed through the figure somewhere between the FESR face and the PB line. Table VI indicates that

this relation holds in general. All the odors in the FESR face are pleasant except ethyl ether and all the odors in the other two faces are unpleasant except coffee. Tansy oil, hop flowers, and turpentine are too near indifference to give the consistency that one desires; nevertheless, the correlation of the affective values of the odors with their position as measured from the FESR face toward the PB line is about 75%. We thus are able to verify the general tendency, *viz.*, that putrid or burned qualities tend to be unpleasant.

Conclusions

Judgments of the qualitative similarity of odors are very variable, and it is thus impossible to verify Henning's qualitative theory with any great degree of precision.

If one assumes that the system of qualitative olfactory relationships is properly represented by a regular hollow triangular prism, and if one chooses odors that have given a high degree of consistency in their qualitative relationships to the principal classes represented by this prism, then one finds that the internal consistency of the placement of the odors within the prism shows the assumption of the prismatic form to be approximately but not precisely valid. On the whole, Henning's prism cannot be said to represent a precise classification of olfactory relations; it is rather a tendency toward a certain set of relations.

Within the limits set by the variability there is considerable agreement between the classification of the odors used in this experiment by Henning, by Dimmick, and by the present writer.

Our experiment accords with Henning's conclusion that fragrant, ethereal, spicy, and resinous odors are usually pleasant, and that putrid and burned odors are usually unpleasant.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER

LXX. LAPSE OF MEANING WITH VISUAL FIXATION

By V. J. DON and H. P. WELD

We report in this paper the results of an experimental study of the lapse or falling away of meaning with continuous visual fixation. An investigation similar to our own has already been made by Severance and Washburn.[1] Their method was, in brief, to ask the *O* to stare fixedly at a printed word for a period of 3 min., and to report changes of meaning which occurred in course. We desired, however, to determine the average time required for the meaning to lapse, and thus to bring the investigation into accord with that of Bassett and Warne, who studied the lapse of verbal meaning with auditory repetition.[2] We hoped also to determine more fully the nature of the attitudes, of the meanings themselves, and of the course of the experiences which occur in an experimental situation of this kind.

Method. We employed a reaction method. The apparatus consisted of the Dearborn-Langfeld tachistoscope[3] which, by means of a piano-player magnet, was electrically operated in connection with a telegraph key, a lip key, and Titchener's electromagnetic stop-watch control.[4] The connections were so arranged that, when the key was pressed, the spring of the tachistoscope was released, the word exposed, and the stop watch actuated; when the current through the lip key was made, by *O*'s opening his lips in reaction, the watch was stopped.[5]

As stimuli we employed common and familiar nouns, such as *face, pen, sign, fish*, etc. All were monosyllables. They were typewritten on strips of paper, every strip bearing 10 words; 5 of these strips, or 50 words, constituted a series; and there were 5 series, making 250 words in all. In addition to these there was a preliminary series of 20 words which was employed as a practice series.

The *O*s were Miss R. Brown (B), Miss D. Johannsen (J), and Professor H. P. Weld (W). The two former were members of the senior class, and were majoring in psychology. Miss V. Don served as *E*. The instructions, which were typewritten, and were given to the *O* at the beginning of every experimental period, were as follows: "I shall present to you a familiar word. Take this word naively (*i. e.*, passively, uncritically), as it comes. Fixate it steadily, and give it maximal attention until its meaning has disappeared. At the close of the experiment you will be asked (1) to state the meaning that the word had, and (2) to report what happened during the experimental interval." It was our purpose to continue the experiment until the *O*'s attitude, as indicated by the average reaction time of a series, became fairly constant. For W 3 series were sufficient; for the other two *O*s 5 series were required.

Results. We give, in Table I., the reaction times and m.v. as determined by series for every *O*. The table shows that, as the experiment progressed,

[1] E. Severance and M. F. Washburn, this JOURNAL, 18, 1907, 182-186.
[2] M. F. Bassett and C. J. Warne, *ibid.*, 30, 1919, 415-418.
[3] W. F. Dearborn and H. S. Langfeld, *Psychol. Rev.*, 23, 1916, 383.
[4] E. B. Titchener, this JOURNAL, 31, 1920, 212.
[5] This disposition of the apparatus was made by Dr. H. G. Bishop.

TABLE I

Average Reaction Times and m. v. in Seconds

Series	*W*	*B*	*J*
1	5.0±2.6	9.6±3.4	14.6±2.6
2	3.5± .8	7.2±1.3	14.5±2.8
3	3.1± .7	5.9±1.7	9.1±1.5
4		5.1±1.4	8.6±1.8
5		4.3± .8	8.8± .7

the average reaction times tended to become shorter until they became approximately stable. Although the average times themselves show considerable variation, the ratios of the longest to the shortest average times are similar: W, 1.6; B, 2.2; J, 1.7. The shortest average reaction time of W is 3.1; of B, 4.3; and of J, 8.6 sec.

The quantitative results suggest and our reports show that the attitudes of the three *O*s were not quite the same. Both B and J tended to put successively several meanings, and W only a single meaning, upon the stimulus word, before meaning finally lapsed. B's attitude differed, however, from J's in that the attempt to take the stimulus word 'passively', as the instruction required, resulted in a 'passive' attention which was contrary to the instruction; whereas J, in the effort to give continuously maximal attention to the stimulus word, repeated the word over and over in verbal-motor fashion. B therefore frequently wandered, as in revery, by free association from one meaning to another, until the stimulus word itself had all but dropped out of consciousness; only when she realized that she was not following the instruction as regards attention did the stimulus word come back again. J's verbal-motor repetition, while she steadily fixated the word, resulted in a succession of meanings, some of which were derived by auditory association. In general, it was not until the verbal-motor process was consciously inhibited that meaning lapsed. J seemed also habitually to have rejected the first meaning which came, and which she reported, as not specific enough. We give as illustrations of the effects of this difference in attitude a pair of typical reports.

B, Series 1*c*. *Train*, 13.2 sec. "Could see a line of cars going through the valley, and I thought of home.... Trains always make me think of home ....I realized that I was doing something I shouldn't, that I was wandering. Then, as I fixated the word, the t dropped off, and it meant *rain*. Then the word was just *there* without any meaning, so I reacted right away."

J, S. 1*a*. *Pane*, 12.0 sec. "I knew that I knew the meaning of it, that if I tried to define it I could. Then I saw a window with four panes. I was saying the word all the time, and the sound called up the other spelling, p-a-i-n. I saw it written out in typed letters, and I was conscious that I knew the meaning of that. I stopped saying it, and I had to look at it quite a while before it fell apart."

These two tendencies were maintained throughout the entire experiment; but as the series progressed the succession of meanings became more schematic and quicker, B's attention better, and J's verbal-motor repetitions fewer.

Types of Meaning. There was also considerable individual variation as regards the type of meaning which the *O*s put upon the stimulus word. In 73% of all her reactions, B had object meanings, and most of these were the first that came. J's first meaning, in 97% of cases, was 'I know that I could define it;' but 77% of these were immediately followed by imaged objects. W's first meanings were objects in 45%, familiarity in 34%, and attitudinal in 10% of the cases. On the whole, there was a predominance of object meanings. In all these cases the object was present in imagery. 14 of B's meanings, however, and 7 of J's, were object meanings in the sense of an object present in the experimental room; *e. g.*, B, *hair*: 'I could see your black hair, and it and the word seemed to be the same thing;'

J, *chair*: 'then I became conscious of the chair I was sitting on, and shifted my attention to the feel;' *noise*: 'then in some way the word was connected with the click of the apparatus.' Other meanings which occurred occasionally for all *O*s were familiarity ('I know that I know'), word associations, and verbal definitions. There was apparently no change in the frequency of occurrence of any type of meaning in the course of the investigation.

Lapse of Meaning. In general it may be said that the lapse of meaning takes place instantaneously. *O* is at one instant conscious of meaning; at the next, he finds himself staring at a series of letters, with no other experience than either a mere 'blankness' or the awareness that the letters composing the stimulus word have disintegrated and now appear only as single letters or groups of letters. Frequently the awareness of 'nonsense-syllable,' of strangeness, or of comicality also occurs; but our results seem to indicate that all of these follow upon a change in the appearance of the stimulus word, and therefore succeed in time the one or the other of the two forms of experience to which our *O*s reacted as loss of meaning.

The experience which we have called 'blankness' may best be characterized by the reports themselves. B: 'Then it just looked blank, somehow. It didn't look different or queer or anything;' 'Then I reacted because there wasn't anything more happening. The word was just there; it didn't mean anything'. J: 'It seemed as if there was a period when I could recall the word if I tried, though the letters had lost their familiarity.' W: 'Then I found myself staring at the word, which didn't seem to mean anything at all;' 'I wish I could characterize the experience; it is as if I were not attending, as if the cognitive side of me were directed somewhere else, or as if the word were simply there, and I were in a revery. Nevertheless I was staring at the word, and I wasn't thinking about anything else.'

A more frequent form of the experience when meaning was gone we have characterized as a disintegration of the letters that composed the stimulus word. In this case the stimulus word divided or changed in several ways. It might become a series of letters which, at times, carried the meaning of 'word' without a more specific meaning; or, at others, meant 'letters'; or, at still others, meant only 'marks.' The following are typical reports.

B, *face*: 'Then the word began to look like disconnected letters. They didn't seem actually to separate, but just to stop being a word;' *steak*: 'Then it was just letters'. J, *noun*: 'As the meaning disappeared, the four letters stood next each other, but without any connection with each other;' *keg*: 'The k and g were not even letters; they were simply marks;' *bog*: 'The b and g didn't seem to be even letters; I had the same sort of feeling I get when I see Greek small letters; I know the capitals, but I don't know the little letters.' W, *jail*: 'There was no awareness that the word was "jail;" it was rather letters which were not integrated into a word;' *dog*: 'There was no realization that the word I was staring at was the same word that "meant" a minute or two ago; nevertheless the thing was still a word in the sense of a group of letters which somehow seemed to belong together.' In other cases the stimulus word divided into non-related letters or groups of letters, and frequently a letter or a group of letters became more clear or insistent than the remaining parts of the word. At times this clearness or insistence was accompanied by a change in the quality or relative position of the letter or group, in the sense that it was seen as blacker and nearer to the *O*; or that the spaces between the letter or group and the remaining parts of the word were larger than they at first appeared; or, finally, that certain letters or syllables came forward toward *O*.[6] Instances from the reports are:

[6]Similar results have been reported by G. E. Müller, *Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes*, 3, 1913, 282. See also A. J. Schulz, *Zeits. f. Psychol.*, 52, 1919, 285.

B, *bulb*: 'The l seemed to stand out and grow darker;' *scale*: 'The "cal" seemed to come forward a little bit;' *steel*: 'The double e sank back, and the t and l seemed to come out.' J, *wood*: 'Then the word separated in the middle; the "od" and the w and the o didn't seem connected at all;' *slice*: 'The letters seemed to fall apart, and the i to stand out in the third dimension. It seemed to stand out as though it were on a block or something.' W, *friend*: 'Then the "rie" became focal; f and d were less clear;' *pear*: 'Then it broke into "pe" and "ar;" there was no auditory component; there is actually more space between these groups, *i. e.*, I can see more space between the e and a;' *hay*: 'The "ay" became clear; the h didn't seem somehow to belong to it; this was purely visual;' *brook*: 'Then the double o became clear; I think maybe r was attached to it; and just as I reacted it became *rook*, with the meaning of a bird.'

Discussion of Results. It is probable that our reaction times do not serve as a measure of the shortest possible interval for the lapse of meaning; the smallest unit of our chronoscope was 0.2 sec., and there was an appreciable interval between the starting of the stop-watch and the appearance of the stimulus word. Our times show, however, that the lapse of meaning with visual fixation may occur within the period of that of auditory repetition. W's average time for the last series was 3.1 sec., which is approximately the final average found by Bassett and Warne, who also employed a stop-watch. It is possible that with prolonged experimentation B's times might have been further reduced; for although her reports, as has been indicated, showed an improvement in the uniformity of attention, we cannot be sure that her attitude had become stable. The average times of J in the two final series, taken together with her introspective reports, seem to show that her attitude had become fixed, and that the recurrence of meaning would continue to condition longer reaction times.

Bassett and Warne remark that in the course of a single experiment "there is no shift from a commonsense to a psychological attitude." With this statement we are in complete agreement; in a reaction situation that is meaningful and one in which the *O* is, by implication at least, instructed to react as soon as the meaning has disappeared, there is little opportunity for a shift to a psychological attitude. Our *O*s were, therefore, unable to report the content-processes which accompanied the 'feeling of blankness' and the spatial distribution of the disintegrated letters of the stimulus word.

The fact that so large a proportion of our meanings were visual objects is in disagreement both with the results of Basset and Warne, in which the meanings were principally 'familiar feels'[7], and also with those of Severance and Washburn, in which the meanings were, apparently, verbal and came by way of auditory association.[8] It is true that 34% of W's meanings were 'familiarity'; and it is probable that, had J been satisfied with the first meaning that came, her object meanings would have been fewer in number. But even so the frequency of object meanings is greater than was to be expected. Severance and Washburn report only a single object meaning, and they say that "the look of a word probably cannot suggest its meaning without the simultaneous presence of auditory-motor images."[9] In our study auditory associations appeared, on the whole, only when *O* repeated the stimulus word in verbal-motor fashion, and they are therefore found most frequently in the reports of J, who, as we have seen, habitually repeated the stimulus word, presumably as a result of the instruction to keep the stimulus maximally clear. We suggest, therefore, that the *O*s in the Severance-Washburn experiment were in a similar way disposed for auditory associations; and that the auditorily presented stimuli and verbal repetitions of the Bassett-Warne investigation were less potent as a con-

[7] *Op. cit.*, 417.
[8] *Op. cit.*, 185.
[9] *Op. cit.*, 185.

dition of visual object meanings than the visually presented stimuli of our Study. We cannot, of course, agree with the statement that "the look of a word probably cannot suggest its meaning without the simultaneous presence of auditory-motor images."

The 'feeling of blankness' as an experience when meaning has lapsed has also been reported by Bassett and Warne. The disintegration of the stimulus word was the characteristic result for Severance and Washburn. The latter also noted the "abnormal prominence of the visual elements in the ordinary word," and they think that "this concentration of attention on a single letter was a powerful factor in bringing about the loss of the normal sound association [which for them conditions the meaning] of a word, and also in destroying its familiar look."[10] The loss of 'familiarity,' which is preceded by the loss of sound-association and followed by a" bare peripherally excited sense impression of the marks which normally are letters," is for them a stage in a continuous narrowing of the field of consciousness, and suggests a kind of auto-hypnosis. We do not venture an opinion concerning the hypnotic nature of the loss-of-meaning consciousness, for we do not know whether the lapse of meaning is a characteristic of the hypnoidal state. But if this should prove to be true, our 'feeling of blankness' might well be a limiting case. This 'feeling' deserves further study; for the characterizations which we have of it seem to indicate that not only may a familiar meaning drop away and the stimulus become only a word among words, or a group of letters, or a series of 'marks', but all meaning may also lapse.

Summary. (1) The meaning of a word may, with continuous visual fixation, lapse as quickly as with verbal repetition. (2) With visual fixation there is a greater proportion of visual object meanings than has been found with verbal repetition. (3) The experience when normal meaning has lapsed is either a feeling of blankness in which the stimulus word is devoid of all meaning, or a perception of certain elements of the stimulus word which have become insistent.

LXXI. Delayed Meaning

By M. V. Wilson and H. P. Weld

What, in this paper, we call 'delayed meaning' has been characterized by Titchener as follows: "An experience and its meaning may be disjoined in time. We often ask, in conversation, to have a remark repeated; we have heard without understanding; but before the speaker has time to repeat, we ourselves begin to reply; the meaning has come, but after an appreciable interval."[1] We have made an experimental study of such meanings with regard particularly to the attitude of the *O* when placed in a situation in which delayed meanings frequently occur, and also to the general nature of the processes during the interval between the presentation of a stimulus word and the accrual of its meaning.

Method. The one of us who served as *E* (M. V. W.) speaks a dialect of southeastern Texas some words of which are apprehended with difficulty by persons unfamiliar with it.[2] On the phonetic basis of certain of these

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[10] *Op. cit.*, 184. They did not regard 'familiarity' as a meaning. It should be remembered that this paper, published 17 years ago, was a pioneer study of meaning.

[1] E. B. Titchener, *Beginner's Psychology*, 1915, 27.

[2] Characteristics of this dialect as spoken by *E* are the almost complete elision of r and l when they occur in the middle of a word; the weak pronunciation of final d and t; the sound of final r as u in *but*; and the sound of the dipthong ai as similar to o in *doll*.

words, which *E* knew, we prepared a list of about 40 familiar words. These were, one at a time, verbally presented to *O* who, by a method of reaction, responded when the meaning came. The apparatus consisted of a lip key, a stop-watch placed in Titchener's controller, and a telegraph key, connected in such fashion that the watch began ticking when *E* touched the telegraph key, and stopped when *O* released the lip key. The *O*s were Florence Crane (C), Lucy Crawford (Cr), W. T. Ellsworth (El), Dorothy Durling (D), Dr. Cora Friedline (F), H. S. Scofield (S), T. V. D. Stern (St), I. C. Wen (Wn), and H. P. Weld (W). F and W were *O*s of long experience; D and Sc were graduate students in Psychology; and the remainder were undergraduate students in Cornell University. The typewritten instructions given to every *O* were as follows: "I shall pronounce a word the meaning of which is doubtless familiar to you. As soon as you have the meaning of the word say 'Ah'. In case the meaning does not come at once, wait until it comes or until *E* stops the experiment. At the close of the experiment we want you to give an account of the meaningful experience during the interval between the presentation of the stimulus word and the end of the experiment." In a single experiment *E* gave a 'Ready' signal and *O* adjusted himself to the lip key. Then *E* gave the signal 'Now' and after 1.5 sec. pronounced the word and, at the same time, pressed the telegraph key. When *O* had said 'Ah', *E* recorded the time and *O*'s report.

Results. We performed in all 784 experiments, and delayed meanings occurred in 38% and meaning failed in 9% of them. Meanings were reported as 'immediate' in the remaining 53%. Our method was, therefore, successful in furnishing a large number of cases of delayed meanings. A reaction method was employed primarily to provide an experimental situation, and not because we regarded the reaction times themselves as significant. We expected that some *O*s would grasp the meaning more quickly than others and we found that, in fact, the individual averages of delayed meanings varied between 4 and 14 sec.; but all *O*s except C, whose greatest delay was 6.6 sec., showed individual variations greater than these limits. If no meaning occurred within 60 sec., the experiment was arbitrarily stopped; the length of time for which a meaning might under our conditions be delayed depended in part upon the persistence of the *O*, and in part upon his auditory memory of the stimulus sound; as a result of the fading memory of the stimulus, meanings delayed longer than 15 sec. were, as a rule, either incorrect or entirely lacking.

The task which *O* under the instruction set himself was either to identify the word, or to find a meaningful word whose sound was sufficiently like that uttered by *E*. In the first case *O* tried to recognize the word as familiar; in the second he tried to make the sound heard meaningful. The former of these two attitudes was characteristic of D, F, W and Wn; the latter of C, Cr, S and St. El in the course of the experimentation changed from the second to the first. When meaning was reported as coming 'immediately' there was no awareness of a temporal interval between the perception of the sound and the appearance of its meaning; "the sound," as S put it, "seemed to carry its own meaning," and the difference in attitude did not emerge. When, however, meaning was delayed *O* was definitely aware that the sound heard was meaningless, and immediately began a search for the meaning. If, under the first attitude, he undertook to identify the word, he first repeated the sound as heard; and if this was not a meaningful word, he either continued to repeat the sound in the hope that by auditory association a meaningful word would 'come', or else he uttered other sounds like the stimulus and, in auditory imagery, compared every one with the remembered sound, until a meaningful word came that he felt to be correct. If, however, under the other attitude, he sought to make the sound heard meaningful, he produced auditorily or visually familiar words whose sound was similar to the stimulus word, and by successive comparison selected the one that seemed to have the greatest similarity.

With either method *O*, as the experiment progressed, was aided by his acquired knowledge of *E*'s special mode of pronunciation; and frequently after long delay an *O* who habitually employed the first forsook it for the second method. We give as illustrations some typical reports.

D, *iron*: "First, auditory memory image of *E*'s voice; then a visual verbal image of 'ion' together with image of *E*'s voice; then a feeling of certainty and resolution to speak;" E, *park*: "I said the sound 'pahk' over and over; then it came like a flash that it must be 'park' pronounced with Southern accent;" *dryad*: "Repeated 'drad' many times until finally it turned into 'dried' and that seemed to be correct;" W, *bayou*: "At first I got the sound 'by-o,' but it had no meaning,—a conscious attitude of strangeness. Then, as I repeated the sound, the meaning of 'bayou' in the sense of an old river bed as employed in the South came;" Wn, *dial*: "I heard 'dow; but it had no meaning. I repeated the sound many times; tried to insert an r; but still the sound had no meaning."

C, *dower*: "First saw 'dowl' in printed letters. Then tried to think of other words, and 'dowl' faded and returned until 'dial' came, also in letters flashing in from the side. Could not decide between them;" Cr, *caulk*: "I debated 'caulk' and 'cork' and finally decided it was the latter because *E* frequently omits the sound of r;" S, *barn*: " 'bon' came first, but did not mean anything; then 'bond,' but I was not satisfied. Next I repeated the sound over and over trying to insert an r. Finally, 'barn' came;" St, *sour*: "At first I thought *E* said 'sod'; then thought it might have been 'sallow'. The latter came after repeating the sound with variations;" E, *wires*: "I felt at first that it was 'wires' but was uncertain. Then thought of 'wise'. I knew it was one of the two, but could not decide between them."

It is characteristic of our experiments that, after the delay, meaning comes suddenly; it has no growth, no development. The typical experience is so striking that the protocols are full of such statements as: "the meaning came like a door slamming, something like a shock;" "it came in a sudden flood," "like a blow in the face," "as a surprised flash," "a revelation," "like a sky-rocket," "as a cold spot flashes out," "like turning on an electric light in a dark room and seeing what is there." There are, however, occasional reports of the 'dawning of meaning', as, for example: "the meaning dawned upon me, but not slowly as real dawn; it was rather like an empty spot opening out into words;" "it seemed to float in;" "it drifted in like a fast moving cloud." A study of the reports, together with a more explicit account of the experience by some of the *O*s who had it, seems to show that the 'dawning of meaning' is an illusory experience. (1) It may occur when the meaningless interval is short and, before the meaning comes, *O* is only vaguely aware that the sound (which psychologically is clear) is meaningless. Apprehension then seems not only to be slow but also to develop. At times the experience is characterized as a gain in distinctness; W, for example, says: "it is like the experience one sometimes has when turning from one conversation to another; the voice attended to becomes definitely more clear and at the same time more distinct; the increase in distinctness is taken solely to be an increase in meaning." (2) An increase in the intensity of the glow or feeling of familiarity is mistaken for an increase in the meaning of familiarity. D reports: "The feeling of familiarity was at first not strong but it became sizable. . . .it was a growing familiarity, a feeling that more meaning would come;" and C said: "It was a quick dawning like recognizing a face once familiar." (3) A succession of meanings which progressively become more specific may seem to be a continuous growth of the meaning. It sometimes happened that, when the stimulus sound was again and again repeated by *O*, there came first a vague and then suddenly a definite familiarity, or some other definite meaning, which seemed to develop from, to grow out of, the first. F, for instance, frequently declared: "Repetition makes me develop a meaning;" on one occasion, when the stimulus word was *mark*, she reported: "Heard it as 'mahk' and

pronounced the sound over and over until 'mark' emerged. I spelled it; there was no other kinaesthesis, no imagery; it was a gradual development from 'mahk' to 'mark;'" and on another: "The meaning came suddenly, but I saw it coming before it was clear." C, who reported dawning of meaning more frequently than any other *O*, did not react until she saw the word in visual imagery. The assemblage of the elements of the word was, however, not immediate, and the growth of the visual word was regarded as a growth of meaning. When questioned she said that either the word was known as familiar before visualization began, or else meaning did not come until the last element of the visual word had appeared,—in which case the meaning seemed to come suddenly.

The content-processes which occured during the interval of delay were exceedingly complex, and since our *O*s were not definitely instructed to describe them, the reports we have are general in nature and confined to the practised *O*s. The recognition of the stimulus word as meaningless is regularly characterized as a 'feeling of strangeness' or a feeling of uncertainty, and occasionally as a conscious attitude, 'a knowing that I did not know,' 'a feeling of not knowing what it meant.' The processes which bear directly upon the search for the meaning have already been indicated. They consist principally in verbal-motor kinaesthesis, and in auditory imagery which carried the repetition and memory of the stimulus word. F frequently employed manual kinaesthesis, and C visual imagery, for the former. Since failure to find a meaning was sometimes conditioned upon the fading out of the memory of the sound as produced by *E*, we are inclined to think that in these cases the meaning was carried by the memory after-image. Finally, when meaning was long delayed, there appeared a complex of processes which carried the meaning of effort and of hope or of despair at failure. These are only roughly indicated as muscular strains localized in the jaws and tongue, or in the muscles of respiration. When the meaning came it was accompanied by sudden relief and relaxation.

Summary. (1) Under the conditions of our Study delayed meaning occurred in nearly half of the experiments. If meaning is delayed longer than 15 sec., it tends either to be incorrect or to fail completely. (2) The appearance of meaning after delay is marked by its suddenness. Cases of the 'dawning of meaning' in the sense of experienced growth from an earlier stage seem to be illusory. (3) The experience during the interval of delay is highly complex, and is characterized by a definite awareness of 'no meaning,' by processes which are concerned with the search for the meaning, and when the interval is long by effort and states of uncertainty, doubt, hope or despair.

PSYCHOLOGICAL PERIODICALS

Arch. f. d. ges. Psychologie. Bd. xliii., Heft 2-4. Arbeiten aus dem psychologischen Institut der Universität Bonn. M. SCHORN. 'Experimentelle Untersuchungen ueber den Uebergang von unmittelbarem zu dauerndem Behalten.' [Continues and extends the work of Meumann and Moers on total and discrete attention in relation to immediate and permanent retention. (1) There are three types of total attention. The first (*a*) is strongly subjective. The unity formed, and the elements that enter into it, are subjective; the subjective processes of attention are revived to touch off reproduction; attention is experienced as function, not as content attended to. In the second (*b*) these subjective factors make way for the objective, sensory factors; the whole experience tends to passivity, though it may contain an active moment of synthesis or comprehension. The third type (*c*) is characterised by extreme passivity.—The differences between total and discrete attention depend in the last resort on differences in disposition for immediate or permanent retention. The discrete type (which is rare) is incapable of strictly immediate retention. (2) If the series of stimuli is presented two or three times, while the observers are still under instruction for immediate retention, we note a shift of attitude. (i) Total attention changes in the direction of discrete: (*a*) becomes (*b*), or total becomes half-way discrete (some elements lapse from consciousness), or half-way becomes fully discrete. (ii) Discrete attention becomes still more discrete; intercurrent processes that further apprehension and impression of stimuli (associations, references to self, judgments, abstractive apprehension) play an important part. (iii) In some cases, the isolating activity of discrete attention is supplemented, at the third presentation, by a mechanical tendency to connection (secondary totality). With further presentations, this tendency would probably prevail.] J. KASTENHOLZ. 'Untersuchungen zur Psychologie der Zeitauffassung.' [Reports experiments aimed, in the first place, to decide the Meumann-Schumann controversy regarding surprise. An irrelevant surprise may be due either to large differences or to the influence of the preceding observation: it is a disturbing factor which has definite effects on the results. Relevant surprise, in Schumann's sense, was reported by 8 out of 12 observers with empty auditory intervals, and by 10 out of 12 with filled visual intervals; its necessary and adequate condition is a renewal of the standard time during the time of comparison, with repression of the time of comparison itself. There are, in general, four typical attitudes in temporal discrimination: (1) the times are taken separately, with either subjective or objective estimation; (2) the standard and comparison times are taken as parts of a total time, with apprehension either of duration or of succession (auditory) and intensity (visual); surprise appears in the subjective form of (1).—The negative estimation-difference (overestimation of the time of comparison) is ascribed, by a process of elimination, to enhanced concentration of attention upon temporal course during the time of comparison.—Duration is probably a derived, succession the primary, temporal experience.] A. PIRIG. 'Experimentelle Untersuchung ueber Lageempfindung und -auffassung und ihre Beziehung zur Auffassung der Bewegung.' [Attempts a qualitative and quantitative study of the sensation of position freed from the influence of movement. (1) Two positions of the forearm in the kinematometer are preceded and separated by passive movements to and fro; two ocular fixations are similarly preceded and separated by prescribed voluntary movements. Limens and introspective analyses are obtained; but it is possible that the movements had some effect on the results. (2) The kinematometer experiments are repeated, with distraction of attention (by easy addition) during the two sets of passive movements. Movement is not apprehended as such; but the objective movement may still have influenced the results. (3) Movement is slowed to imperceptibility over a

brief distance before and after the normal position of the arm in the kinematometer; is slowly quickened; and then is again slowed to imperceptibility over a brief distance before the comparative position. The results are now decisive; the sensation of position is specific, and does not depend on the cooperation of a movement-sensation. We perceive position by way of the sensations of the resting arm, whose apprehension is connected with a spatial (not necessarily visual) localisation of the arm itself. The limen obtained by this procedure is the smallest of the three.—Special experiments show that the impression of arm-movement depends on the apprehension (*a*) of continuously changing sensations (pressure, strain, etc.) and (*b*) of the moved member as an object in space. The experience of movement, like that of position, is unique and independent.] F. Schmitz. 'Hemmungen beim unmittelbaren Behalten von Buchstaben und sinnlosen Silben.' [(1) Experiments with specially prepared series of consonants show that associative and reproductive inhibitions are effective in immediate as they are in mediate retention; their separate effects cannot, however, be distinguished Apprehension of the series as a unit lessens the inhibitory effect. (2) As the apprehension of a series of meaningless syllables proceeds, a progressive inhibition shows itself, due to the progressive decrease of available psychophysical energy. The inhibitory effect is more marked with total than with discrete attention. (3) The fact that progressive inhibition does not persist throughout the series, but that the number of errors is reduced for the last member or members, is negative proof of regressive inhibition. In the period directly following the process of apprehension, the effect of regressive inhibition may, in certain circumstances, be clearer than that of progressive; as immediate is replaced by mediate retention, however, progressive inhibition gains the greater importance. (4) A pause within the series favors the formation of complexes. (5) If the time between presentation and reproduction is increased from 2 to 5 sec., reproducibility is decreased. If the time is filled by some mode of mental activity, the resulting effect of regressive inhibition is greater than that of the interval as such.] G. E. Mueller. 'Gesellschaft für experimentelle Psychologie. [Notice of the eighth congress, at Leipzig.]

Bd. xliv., Heft 1 u. 2. E. Schulze. 'Die Anpassung eines Willensimpulses an eine Verzögerung seines Effektes und ihre Prüfung durch die Herstellung einer Vergleichszeitstrecke.' [In the reaction experiment we associate an impulse to a perception. We can also associate a perception to an impulse: the observer presses a key, and at a fixed brief time thereafter a hammer falls; a few trials suffice to impress the interval.—The association is utilised as follows. A normal (empty auditory) time is given; a pause succeeds; then the first stroke of a comparison time is given. The observer is to mark off the comparison time, not when he judges it equal to the normal, but when he judges it so far advanced that the addition to it of the interval impressed during the association-practice would equate it to the normal; the moment of his reaction is recorded. Normal times and association-intervals are varied.—The comparison time increases with increase of the association-interval; with the smaller intervals it is objectively too small, for the larger it is in general too large. This departure from the ordinary time-sense result (comparison time too small) is probably due to the division of the comparison time by the observer's impulse. The total average comparison times (all observers) agree very closely with the normal times: this result may be accidental, or may be a group-effect, due to adaptation to the range of variation employed.] R. H. Goldschmidt. 'Grössenschwankungen gestaltfester, urbildverwandter Nachbilder und der Emmertsche Satz (mit Bermerkungen zur Logik der experimental-deskriptiven Psychologie). '[Reports a principal series of observations of the tertiary positive after-image of a white rectangle, seen parafoveally, and subsidiary series planned to answer special questions. Emmert's law holds: or, at any rate, it may be said on the basis of these observations that "in general,

subjective visual phenomena of determinate form increase and decrease in size at least very nearly proportionally to the increase or decrease of their distance." There are incidental discussions of entoptic and ectoptic images; of the tendency of localise at the place of stimulus and of the tendency to localise some 2 m. away; of peripherogenic and centrogenic factors; of prevalence and insistence; of objective and subjective distances; and so forth. The account of the experiments is also prefaced and constantly accompanied by discussions of the methodology of an experimentally descriptive psychology. The author emphasizes in particular the need of a parallel systematic study of eidetic images; some of Jaensch's results may, he thinks, be due to the interplay of the opposing principles of perspective and of reciprocal perspective (Emmert)s law).] A. Fischer. 'Soziologie, Sozialwissenschaften, Sozialpsychologie.' [Pure sociology is a morphology of society; it studies the form of society as visible expression of its constitution, constitution as the law of its form. Genetic sociology examines the course and laws of change of society at large (socialness), of societies, and of mankind in so far as mankind is able and tends to become a society. Applied sociology deals with the alteration of social conditions by voluntary action.—It has been said that sociology is nothing more than the sum of the social sciences. But these sciences (history included) on the one hand take society for granted, and do not investigate the social aspects of their subject-matter, while on the other hand they point beyond themselves (as when linguistics seeks to account for dialect, or law for martial law) to a sociology proper. They are better named 'cultural' or 'mental' sciences than social.—The data of sociology are largely psychical, and all have a psychical aspect; yet sociology is not psychology, if only because the psychical is precondition of all forms of society alike, and therefore cannot explain their difference. Social psychology in the sense of *Völkerpsychologie* falls to the ground because it has no subject-matter; there is no *Volksseele*. Hence there is but one psychology; and when this psychology turns its attention to social facts and values we shall do better to call it simply psychology of society (*Sozialpsychologie*).] J. Kollarits. 'Sprachpsychologische Notizen. [(1) The adult's acquisition of a foreign language passively, purely by ear, shows many analogies to the child's acquisition of its mother tongue. (2) Polyglots are likely to betray their nationality by using their native form of an international word, or by giving such a word in its foreign form their native accent. (3) Monoglots tend to overvalue their own speech (cf. the *admirable clarté* claimed by the French for French); polyglots tend to exaggerate the delicacy, the fine shading, of a foreign language.] E. Czuber. 'Lineare Ausgleichung und Korrelation.' [Discusses briefly papers by L. J. Reed and C. Gini in *Metron*; offers a treatment of the problem, based solely on the method of least squares, which brings out the coefficient of correlation in a natural way. The calculation of r is not always necessary; and r itself is not easily intelligible, and if used unintelligently may work harm. The theory of correlation marks, nevertheless, an advance in the mathematical treatment of statistical data; the author agrees, so far, with W. M. Persons and C. J. West against H. Westergaard.] W. Wirth. 'K. Pearsons Angepasste Gerade (Best fitting straight line) und die mittlere Regression.' [In any hypothetical elementary analysis of correlation, the errors of observation of Pearson's construction must be replaced by the naturally given deviations of the (more or less accurate) individual measures from certain mean measures, in which the proportionality that finds expression in the total correlation table would be brought out in purest form.] Referate. G. E. Mueller. 'Einladung zum achten Kongress für experimentelle Psychologie in Leipzig vom 17-20 April 1923.'

Bd. xliv., Heft 3 u. 4. E. Posch 'Umriss einer realistischen Psychologie.' [Outline of a realistic (materialistic, behavioristic) psychology, which was published in book-form, 1915, in Hungarian. All substantives are to

be banished from psychology, which deals only with activities. The outside world acts upon us in such a way as to drive us from pose to pose, from attitude to attitude, from muscle-tension to muscle-tension; and there is no other subject-matter for psychology. Intellectual apprehension of all kinds is a sum of muscular adjustments; in perception, we act toward the object; in ideation (imagination, abstract thinking) we act as if toward the object. Attention, feeling (since James-Lange) and will are obviously motor functions. Of retention and the faculty of memory we know nothing; our nervous and muscular systems are so constituted as to produce the phenomena. To sense is to transform vibrations into qualities (another property of the nervous system) and to be set into movement by these qualities; the qualities are, none the less, in the world, and not in us. Consciousness, the self, mind disappear; they are, so to say, errors in the psychologist's calculation. The most important distinction within psychological subject-matter is that of the easy and the difficult; all attitudes, postures, muscular adjustments group themselves about these two poles. The most hopeful direction of experiment is the study of the psychogalvanic reflex.] O. Michel. 'Experimentelle Untersuchungen über das Gedächtnis; Reproduktion und Wiedererkennen von optischen Eindrücken.' [Reports experiments by Gotschlich's method of polyeidoscopy (visual exposition of objects of everyday life): the objects were exposed; were named after exposure (immediate reproduction); and then the objects not recalled were exposed along with other, new objects for recognition. Number of objects and time of exposition were varied; for reproduction and recognition a time of 6 sec. was allowed for every object.—Range of reproduction and recognition increases with the demands made by the experiment, but tends toward a maximum. Both reproduction and recognition improve with increasing time of exposure up to a certain point, beyond which reproduction improves, recognition falls off. The greater the number of objects, the slower is the course of reproduction and recognition; a large number also tends to obscure recognition. There are considerable individual differences both in range of reproduction and in range, certainty and correctness of recognition.] A. Zink. 'Die Unterscheidung des Physischen und Psychischen nach Robert Reininger.' [Phenomenological part (the others, epistemological and metaphysical, are not here printed) of a critical study of Reininger's *Das psychophysische Problem*, 1916. According to Reininger, immediate reality knows neither physical nor psychical phenomena; only the psychophysical is real; every item of reality has its experience-side (psychical) and its ideation-side (physical). The difference between psychical and physical is that of quantitative difference of the two constitutive components of reality, experiencing and ideating.—Reininger thus stands in relation to the philosophy of immanence, and to the views of Mach and Avenarius.] H. Dingler und R. Pauli. 'Untersuchungen zu dem Weber-Fechnerschen Gesetz und dem Relativitätssatz.' [(1) Logarithmic correlations occur in all sorts of bodily (plant and animal) and ment a responses to stimulus and phenomena of growth, and occur with a frequency that puts other numerical relations of dependency in the shade. (2) The facts are summed up in a law of relativity: the measurable relations of dependency that appear in the field of bodily and mental processes of response to stimulus and of growth show, over a wide range, a unitary character; a biological magnitude of the kind in question varies with the variable in such a way that it rises at first quickly, then much more slowly, to an empirical maximum, in the sense of the logarithmic curve. (3) This relativity has a peculiar importance for the conservation of life; the biologically harmful state is attained comparatively slowly, the advantageous correspondingly quickly.—So far Pauli; Dingler continues the argument. (4) Formal consideration of the Weber-Fechner Law shows that in its logarithmic formulation it takes on the simplest functional form; this is attained when multiplicative diminution of stimulus-change itself proceeds proportionally to stimulus. (5) Causal consideration of the Law must explain it

by the statistical cooperation of a large number of physical-chemical individual processes; appeal has been made to the law of mass effect, to osmotic-electrolytic processes, and to a combination of the two. There have also been attempts to treat the process of excitation as a unit, and to find an explanation in terms of the distribution or inhibition of excitation; no complete theory on these lines has so far been worked out. Indeed, at the present time we have no comprehensively satisfactory theory on any basis.—Both authors are clear that the interpretation of the Law must be physiological.] O. LIPMANN. 'Bemerkungen zur Gestalttheorie.' [(1) Configurations are either subjective, *i. e.*, essentially dependent upon organism or subject, or objective, *i. e.*, such as present themselves ready-made for adequate recognition. The *Gestalt-theoretiker* seem to go too far in their assertion of physiological and physical (objective) configurations. (2) Psychology need not be exclusively configurational; there is room also for a scientifically orientated analytical psychology. (3) Analysis itself is a process of configuration; and what we call 'association' may be such a process, in which neglect of the configurational side allows the elements to appear as such.—Degree of intelligence shows itself in the complexity, and more especially in the variety and variability, of configuration; but those who test intelligence should remember that form of configuration points to different types of intelligence.] Literaturbericht: Referate.

Bd. xlv., Heft 1 u. 2. G. KROGH-JENSEN. 'Der Unterschied im männlichen und weiblichen Entwicklungstempo und seine Bedeutung für die moderne Koedukationsfrage.' [The establishment of coeducation, at any rate in secondary schools, has been far more a matter of politics and political economy than of pedagogy. The problem of coeducation has been discussed under its moral, psychological and physiological aspects, with varying thoroughness and with varied result. In fact, physiological considerations are decisive. For boys and girls develop at different rates; the girl is mature in about 18, the boy in 24 years. It follows that the two curves of development, which in the mathematical sense are practically identical, nowhere coincide; thus, for some years girls are taller and heavier than boys; and girls reach puberty two or three years earlier than boys. Further, development in the pubertal years is more rapid and brings more profound changes in girls; girls are now weaker than boys of the same age, and are more subject to illness. And so on. The outcome is that we shall have an ideal or perfect coeducation only when boys and girls go to the same school, are educated (*erzogen*) by the same men and women in common but not in the same form, and are instructed so far as possible separately. Common instruction, as at present practised, is an immense *non sequitur* of modern pedagogy.] J. HANDRICK. 'Zusammengesetzte Reaktionen mit Superposition komplexer psychischer Vorgänge.' [Reports reaction-experiments involving an immediate succession of psychical processes; the problem is to discover whether these processes are altered by conjunction, and thus whether Wundt's subtractive procedure is justified. The use of two chronoscopes made it possible to examine: cognition reaction, cognition reaction followed by subsumption reaction, cognition-subsumption reaction followed by association reaction, and cognition-subsumption-association reaction followed by association reaction. The subsumption was also, in other series, executed as a process of choice. (1) In general, a following process operates to simplify, to abbreviate and to compress a foregoing. (2) Cognition begins with meaningless visual perception and ends with apprehension of meaning; it may be palpable or impalpable. A following subsumption reduces sensory contents and thought-relations, and induces a categorical apprehension. (3) In subsumption the categories are run through, and set in relation to the cognitive meaning. A following association simplifies the process; subsumption fuses with cognition; it may either oppose or further association. (4) Association is rich in contents, not sharply delimited, often sensorily represented; as a rule, the association-word is chosen arbitrarily from a complex. Preceding subsump-

tion often determines association; a following association makes it more definite and more distinct. (5) Quantitatively, preceding processes are shortened by following; cognition is least affected. (6) The abbreviation may be genuine (elimination of part-processes) or apparent (overlapping of total processes). (7) The subtractive procedure is not justified.] H. EHRENWALD. 'Versuche zur Zeitauffassung des Unbewussten.' [In sleep (suggestion to wake after so-many hours) and in hypnosis (terminal suggestion) subjective time seems in general to accord with objective, though some persons may be bradychrone or tachychrone; in the waking life estimation of time by way of change of conscious contents is uncertain and inaccurate. These results point to a nativistic theory of time-experience (Hering, Semon, Mach), as distinct from the 'time-sense' of experimental psychology. The nature of the organic processes that underlie this experience is still undetermined.] S. TROUET. 'Der Willensakt bei Wahlhandlungen; eine experimentelle Untersuchung.' [Reports experiments with a spring-ergograph. The instruction to exert maximal effort, however phrased, does not ensure an act of will; the act proper, as in the reaction experiment, precedes the whole experiment. An instruction to choose between response and non-response, on the other hand, when three modes of response are prescribed (strong, medium, weak), secures—though not with all observers—the desired result. The internal voluntary act has five phases. (1) The *intention* to concern oneself in what is to come: this consists simply in acquiescence in the status of observer. (2) The *idea* of the action to be performed. (3) The *motive*. This may be a motive in the ordinary sense, some affectively toned intellectual complex; or it may be a motive in the wider sense, a matter of feeling. Under the second heading fall (*a*) feelings that attach to individual ideas or judgments; these include motives in the narrower sense; (*b*) situational feelings, or feelings which represent a reaction to individual situations; and (*c*) moods. Situational feelings are for the most part unpleasant. The interplay of situational feelings and moods may act as a substitute-function for the ordinary struggle of motives. (4) The *choice* based on motive. Again, the interplay of feelings may act as substitute for choice in the ordinary sense; if, however, the consciousness-of-self (in the ethical sense) becomes involved, we have a true choice. (5) The *decision* issuing from the choice. Besides (*a*) the ordinary form of decision we may have (*b*) an interplay of feelings, at times cut across by the (ethical) consciousness-of-self as a summation-centre of feelings, and (*c*) a new form of affective decision; mode of response expresses an attitude (*Stellungnahme*) toward the conditions of experimentation.—There are thus two types of voluntary act, the 'intellectual' and the 'emotional;' the latter type has fewer phases, is dominated by strongly motivating feeling-masses, and is, so to say, all of a piece.]

Bd. xlv., Heft 3 u. 4. J. WITTMANN. 'Ueber das Gedächtnis und den Aufbau der Funktionen: eine experimentelle Untersuchung über das An- und Abklingen der Reproduktionen taktiler, akustischer und optischer Eindrücke.' [We have to distinguish, in psychophysical context, between processes of organisation and the resulting functions (ordered courses of psychical phenomena). The relation of the two is regarded realistically by the Wertheimer school, idealistically by the author; the former posit effective forms, the latter takes recourse to physiological processes.—The experiments show that eidetic phenomena are observable in the fields of touch and hearing, as well as in that of sight. With certain individuals, a touch or group of touches recurs spontaneously, more or less frequently, as eidetic image, indistinguishable (except, perhaps, as seeming to well up from within the body) from the original impression or impressions; and, more than this, a present impression may revive older impressions: thus, a single touch, after stimulation by 1, 2, 3 and 4 touches, may evoke the reproductive series 3, 2, 3, 4, 4, 2, 2, 3, 1, 1 (additive reproduction). Similar phenomena occur with tones. In both departments, the set of attention is

important.—In functional explanation we must assume a high degree of liveliness (*Lebendigkeit*) and plasticity of nervous occurrence; we must also assume, in addition to the direct excitatory processes, a widespread excitatory process (corresponding to the organising function of attention) of the same nature, if not in part identical with them.—Memory is thus a conserving and organising function of the nervous system, based on liveliness and plasticity; the relation of mind to body is neither parallel nor interactive, but functional, mediated by attention.] P. ASTRUCK. 'Ueber psychische Beeinflussung des vegetativen Nervensystems in der Hypnose: i. Hypnotische Beeinflussung der Herztätigkeit und der Atmung.' [Reports experiments made with a variety of instruments. (1) In deep (not in light) hypnosis the activity of heart and respiration is subject to the influence of verbal suggestion. The activity of the diaphragm is changed from that of the normal state. (2) Whether the suggestive influence bears on heartbeat or respiration, the pulse falls abruptly, *i. e.*, becomes small and weak. (3) Under heart-suggestion peculiar forms of heart-beat appear. (4) The suggestion of acceleration or retardation of heart brings with it a respiratory reaction in the same sense; the converse of this statement as a general rule does not hold.] C. M. GIESSLER. 'Zur Charakterisierung der phänomenalen Räume, insbesondere des Hörraumes.' [There are many phenomenal spaces, inner and outer, which vary in range, duration and structure. In their (subjective) structure we may distinguish a general spatial qualification (in the last resort, space-feelings of expansion and contraction, realisation of the place of the body in its surroundings) and a special qualification (details of the space-feelings, characters of stimuli).—Auditory space is discontinuous; it varies in range with intensity of sound and degree of attention; in structure it is one of the least uniform spaces. Fairly loud, uniform and harmonious sounds enlarge the field of regard and favor statisation; louder sounds of the same sort, and irregular and disharmonious sounds or complexes of noise, narrow the visual field and induce astatisation. Loud and irregular noises have a special power to distract us from the outer world and so to prevent our adjustment to it; hence the conjunction of ear and static apparatus serves a useful biological purpose.] E. STOERRING. 'Pneumographische Untersuchungen von Gefühlszuständen.' [Proposes new measures for the evaluation of the respiratory curve: especially (*a*) the quotient upper breadth/lower breadth (the breadths are arbitrarily determined) and (2) the absolute upper breadth, an angular measure. By these means the author is able objectively to distinguish sensory, dispositional and emotive pleasantness and unpleasantness, and also to give an unequivocal objective characterisation of anger, which (against Stumpf) marks if off from all modes of pleasantness. The regular stimuli were tastes; for emotive pleasure and for anger the author employed catch-words derived from the observers' foregone experiences. The anger was for the most part an 'original,' *i. e.*, a positively reinstated anger.] C. MEINTS. 'Die Anschauungen Meumanns über das zustimmende Urteil als Faktor der Willenshandlung.' [Begins with an exposition of Meumann's views from the *Intelligenz und Wille*, 1913. (1) Meumann's judgment of acceptance really includes two processes: the judgmental determination of the purposiveness of an act, and the acceptance of its execution (*Zustimmung zur Ausführung*). Judgment influences voluntary action only mediately; the direction, control and regulation of the psychical processes that in such action connect with a judgment of acceptance are to be ascribed to the acceptance-of-execution. (2) Meumann's confusion led him to miss the fact that not every act of will need be preceded by a judgment of acceptance. (3) The same confusion accounts for his vacillation as regards instinctive actions.] Literaturbericht: Referate. ['Reflexologie und Psychologie,' by F. Schneersohn.] Mitteilung. [Würzburg Congress of Applied Psychology.] W. Wirth. 'Aenderung des Archiv-Verlages.'

Bd. xlvi., Heft 1 u. 2. Festschrift zu Ehren des 70. Geburtstages von Prof. Dr. Goetz Martius: I. Teil. E. R. JAENSCH und W. SCHOENHEINZ. 'Einige allgemeinere Fragen der Wahrnehmungslehre, erläutert am Problem der Sehgrösse: nach Untersuchungen über Mikropsie beim Rollettschen Konvergenzplattenversuch.' [Reports experiments undertaken to determine the relation between the micropsy induced by Rollett's plates and the Aubert-Foerster phenomenon (with constant visual angle, small near objects are recognised over a larger area of the retina than large distant objects.) The results of the experiments with Rollett's plates may be summed up in the statement that micropsy increases with increasing complexity of the object. The results of the second set of experiments (Rollett's plates set to give strong convergence with ready fusion of the half-images in Hering's haploscope) show that extension of the field of vision is greater, the more complicated the background. There is thus a parallelism of the two phenomena in question; special experiments fail to reveal whether the change of apparent magnitude is the condition of the attentional attitude (which, according to Jaensch, underlies the Aubert-Foerster phenomenon), or conversely.—Eidetic considerations suggest that, in certain circumstances, simple objects of perception may approximate the behavior of ideas (high invariance); here is the explanation of the experiments on micropsy.] F. KAUFFMANN. 'Zur Theorie des Mythos.' [Myths are "those linguistic creations of the imagination which take their origin in the ideas of a religious faith, which are dedicated to the secular life of society, which are independent of the symbolic actions of cult and of the didactics or magic of sentential poetry, and which belong to a purely narrative poetry."] T. NISSEN. 'Die Physiologie und Psychologie der Furcht in der Ilias.' [We cannot, in the Iliad, separate physiological from psychological, or human from animal psychology. The paper takes up in order: terminology; the seat of the emotion; animal fear; the physiological concomitant phenomena (cold shiver, trembling, heart-beat and chattering of teeth, paralysis, uncertainty of regard, the dropping of objects, ducking, flight); descriptions of the total course of the emotion; fear for oneself and fear for others; the causes of fear.] M. PAPPENHEIM. 'Kritische Untersuchungen zum Rasengang (ganga undir jardarmen) der Islandersagas.' [Recourse was had to the ordeal of 'passage under the turf strip (or strips)' in three cases: the undertaking of blood-brotherhood, the invoking of a judgment of God, and the expiation of serious trespass against honor. It has been argued (K. Maurer, E. Mayer) that the common element in all instances is the taking of an oath. That position, however, cannot be maintained; it is highly probable, on the contrary, that the usage arose in one of the three cases and was from that transferred to the others; certainly, its intervention in the rite of blood-brotherhood is late and secondary.] W. WIRTH. 'Bedeutung und Gültigkeit des Fechner-Helmholtzschen Satzes über negative Nachbilder.' [Recurs, for the most part controversially, to the author's study in *Philos. Stud.*, 16, 17, 18. Helmholtz had laid it down that the intensity of the sensation aroused over a fatigued area of the retina is a function of the intensity of the reacting light; Wirth, bringing this statement into relation to Fechner's parallel law, named it the Fechner-Helmholtz Law. He now shows that the law is independent of Helmholtz' theory of fatigue; that it may be given a psychophysically indifferent formulation; that he had never considered its validity to be more than approximate; and that he had from the first taken account of deviation from it ('Hering's deviation'). In its approximate form, it holds both for light and for color.]

Bd. xlvi., Heft 3 u. 4. Martius-Festschrift, II. Teil. J. WITTMANN. 'Widmung.' W. AHLMANN. 'Zur Analysis des optischen Vorstellungslebens: ein Beitrag zur Blindenpsychologie.' [The writer was blinded in 1916 by a shot which severed both optic nerves; the present paper occupied him during the year 1922. (1) Orientation, unless some affective interest is in play, reduces to the necessary minimum; *i. e.*, it is in principle a matter of knowledge (*wissensmässig*) only. (2) Instructionless visual realisation of

present surroundings is rare; when it occurs, it is fragmentary, transient, spatially inadequate. Visual recall of past situations, affectively or attentively experienced, is common; since the experiences were often non-visual, the visual reproduction is here formative or constitutive. (3) Visual complexes are of two types. They may be tridimensional, plastic, colorful, bathed in a warm light; they then constitute an organic whole. Or they may be mathematicised, superficial outline drawings; the plane of their projection divides into sections; the colors are dull and impersonal; the surrounding light is pale and sombre. (4) The space within or against which these complexes appear is not the intrinsic retinal grey; it is a dark cavity, like a stage bounded by curved lines, concave to the observer. (5) The perspective or plastic complexes may be built up from a bare qualitative datum, or may appear practically ready-made; the superficial outlines are added one to another in time. Here the shifting of observational regard is important; the author notes the difficulty of tracing by movement of regard distances in the third dimension. (6) The consciousness of reality arises (*a*) with the 'placing' of perceptive experiences; until the fulfilment of orientation takes place, the experiences are simply 'there,' not yet real. It may also occur (*b*) during intensive ideation of (real or imaginary) data of a present surroundings in their spatial relation to the thinker's own spatial position.] K. Graf von Dürckheim. 'Erlebensformen: Ansatz zu einer analytischen Situationspsychologie; ein Beitrag zur Psychologie des Erlebens.' [In every moment of experiencing, a determinate objective and a determinate non-objective are conjoined; their conjunction gives a situation. Sequences of situations that connect into units are experiences. Every total experience has content and form; we are here concerned with the analysis of typical forms. There are two fundamental forms of experiencing, according as an affective relation of non-objective (experiencer) to objective is or is not given: we term them directed and undirected, and we deal here only with the directed. This, in turn, may be either conative (*zielstrebensbestimmt*) or non-conative: in the former case, the objective is something to change, and the reactions are both passive and active; in the latter, the objective is something to be 'taken' in some way, and the reactions are passive. Conative experiencing in regard to its objective may be either experience of action or experience of wish; non-conative experiencing may be either contemplative (synthetic) or analytic.—Characteristic of passive reactions are pleasantness-unpleasantness and yes-no reactions; of active, expressive and impressive actions. Judgment is a special reaction of analytic experiencing. All forms of experiencing may include, along with pleasantness-unpleasantness, expressive movements, movements that refer to the objective, and yes-no reactions; all three reactions, however, differ in nature with difference of form of experiencing.—So far we have been describing; we now turn to theory. The determinate course of an experience may be referred to an initial set of apprehension. The determinate course of a reaction points to "reactive connections, stored in memory, which in certain circumstances may attain to reproduction;" such reactive connections the author terms, with Martius and Wittmann, 'functions'. Finally, adequate reaction to a novel situation depends upon the apprehension of the situation in its specificity; with this apprehension, specific conditions of reproduction are given.] B. Petermann. 'Ueber die Bedeutung der Auffassungsbedingungen für die Tiefen- und Raumwahrnehmung; eine experimentelle Untersuchung.' [Reports experiments, made with discs or groups of discs, seen by transmitted or reflected light, variously disposed in objective space, under conditions which ruled out empirical motives to localisation. (1) *Monocular observation.* Every visual object is seen at a qualitatively definite distance. The grouping of visual objects in visual space (which shows marked differentiation in the third dimension) does not correspond with the grouping of their stimuli; it is, nevertheless, dependent on ascertainable conditions. Voluntary control of attentive regard is of determining influence. Uniform observation of different ob-

jects brings them into the same plane. Spontaneous collective apprehension is fundamental for the configuration of a tridimensional distribution; it brings the individual object and the total space into a relation of reciprocal conditioning. The objective depth-motives for a group of discs are brightness, size, brightness and size, geometrical form; throughout, the arrangement of stimuli influences apprehension and only thus, indirectly, the arrangement in depth. Hence a grouping conditioned on objective depth-motives can nearly always be deranged by voluntary control of observation; their operation is also subject to the influence of collective apprehension.—Brightness also affects apparent size; of two discs of the same visual angle, the brighter appears the larger. The dual influence of brightness renders the law of visual angle invalid. Visual magnitude is qualitatively given, even if there is no clear localisation of the object. (2) *Binocular observation.* The conditions of apprehension are again constitutive of depth-distribution. If the stimuli are objectively equidistant, voluntary shift of regard gives the impression of seeing-into-distance; voluntary fixation of one object brings it nearer, unless the object is brought into relation to its background. Further, the brighter object is the nearer; with increase of brightness the object moves toward the observer. Lastly, collective apprehension brings object and total space into reciprocal conditioning.—Brightness, as before, affects apparent size. The dual influence of brightness sets limits to the law of visual angle. (3) It follows that a psychology of visual space must consider binocular and monocular results together.] K. MARBE. 'Kongress-Vertagung.' [Postponement of Würzburg meeting.]

Bd. xlvii., Heft 1 u. 2. F. KIESOW. 'Zur Frage nach der Gültigkeit des Weberschen Gesetzes im Gebiete der Tastempfindungen; nach Versuchen von cand. sci. nat. A. Gatti.' [Summarizes the results of experiments by A. Gatti (*Arch. ital. di Psicol.*, 1923) on the stimulation of sparsely scattered hair-spots on the fore-arm. Concludes (against K. Hansen and M. von Frey) that Weber's Law holds, with a relative DL of about 1/7, for a range of stimuli from 3 to 6 or 7 g/mm, and that the judgments are made on the basis of intensive, not extensive, stimulus-change.] H. MEUER. 'Experimentelle Beiträge zur Lehre vom Wortgedächtnis nach der Methode der Polyeidoskopie.' [Reports experiments on immediate and permanent (30 min. to 4 wks.) memory of series of monosyllabic words of concrete meaning, auditorily presented once only to children of both sexes (11 and 12 years) and to uneducated adults. (1) Increase of the difficulty of the task (5 to 30 words) brings about absolute improvement in reproductive performance. (2) Continued effort of memory induces fatigue which impairs reproductive performance. (3) Pauses may have a favorable (long midday pause) of unfavorable (brief and unsurpervised pauses in instruction) influence on memory. (4) The temporal curve of retention at first falls sharply, may then rise again or remain at a constant level, and finally falls; it thus differs from the curve for visual impressions, which first falls, then rises to a point higher than that of initial performance, and thereafter remains at a constant level. (5) Auditory impressions are strengthened by repeated reproductions; the closer the first reproduction follows on exposition, the better is retention.] B. KLOPFER. 'Das Problem der seelischen Hemmungen.' [A study, in the interest of education, of the concept of inhibition as it appears in current psychology. We have in fact three concepts: that of vulgar psychology, which agrees in part with that of psychoanalysis (emphasises subjective activity and the effectiveness of an individual ego; is markedly teleological), that of experimental and especially of associational psychology (seeks an objectively demonstrable effect, due to the operation of stimulus; explains causally), and the intermediate concept of phenomenology and statistics. (1) In experimental psychology we find two lines of work, according as the effect of the interfering stimuli is considered to be direct (Heymans, Ranschburg, Aall, Grünbaum) or conditional (Müller and Pilzecker, Ach, etc.). These lines show a progressive

tendency to converge upon a subjectivistic interpretation. (3) Phenomenologically we may distinguish centripetal and centrifugal inhibitions, the latter of which has a negative and a positive sub-form: we thus get a temporary block or disturbance (experimental psychology), a suppression or passive resistance (psychoanalysis), and an active resistance or act of inhibition (Ach). In their primitive form, attention and inhibition form an undifferentiated unity; in their development as activities the two diverge, thought heir functional interaction is close. Inhibition is also closely related to emotion. (3) Psychoanalysis operates with the concept of suppression (*Verdrängung*). Its tendency, despite associative terminology, runs through personalism to a superpersonal metaphysics. (4) The writer accepts the theory of inhibition put forward by Stern in *Die menschliche Persönlichkeit*. Centripetal inhibition guarantees the integrity of the individual organism over against the outside world; centrifugal inhibition is of prime importance for the transition to the higher forms of organisation of human existence.] M. HONECKER. 'Komik und Einstellung; ein Beitrag zur Lehre von den Bedingungen des komischen Erlebnisses.' [By predisposition (*Einstellung*) the writer understands the sum of beliefs or assumptions (*Meinungen*), conditioned on a given phenomenon or on anticipatory expectation, regarding the further or future experienceable characters of the given or coming phenomenon. An impression of the comic may arise if some (disinterestedly observed) object disappoints a predisposition (whether originating in expectation or in apprehension) in a way that is significant for the object's meaning (but that does not disturb any important interest of the experiencer). This formula improves and fulfils that of Lipps.] M. I. GUTMANN. 'Ueber Augenbewegungen der Neugeborenen und ihre theoretische Bedeutung.' [Infants in the first month may like, dislike, or be indifferent to a light-stimulus. A reflex pupillary reaction is observable in all children at birth. A few infants are able, in the first days of their life, to fixate a bright object and to use their eye-muscles in coordination; most acquire this ability later. In general, the correct use of the eye-muscles is 'learned', like that of other bodily muscles.—These results agree with the anatomical facts (Flechsig).] E. BERNEN. ' "Die reine Logik" von Friedrich Albert Lange.' [Abstract, with running commentary, of a 'Pure Logic,' which Lange began in 1855 and left incomplete. The work may be regarded as a confirmation of a theory (of mathematical thinking) by practice.] R. NAGEL. 'Die Kontrolle der Konstanz einer heterochromen Helligkeitsvergleichung, insbesondere an der Hand des Korrespondenzsatzes für Aequivalenzwerte.' [Reports experiments on the determination of the brightness-equivalence (method of constant stimulus-differences; complete series) of red (pigment-color of full saturation and moderate intensity) and grey, so planned as to admit of test by Wirth's law of correspondence, which declares that, if the true equivalence-value A of the first experimental arrangement is introduced as normal stimulus R of a second experimental arrangement, then (other things equal) the equivalence-value B of the second experimental arrangement must coincide with the normal stimulus N of the first experimental arrangement.—The writer insists that, in all heterochromatic photometry, the observers must be trained to judge directly in terms of brightness, as a relatively independent character, apprehensible by abstraction, of all color sensations. Langfeld's *Einstellungen* are *Fehleinstellungen*, that bring with them more or less constant or individual errors of comparison.—Systematic preliminary experiments (four observers), undertaken to determine the approximate position of the equivalence-values and the zone of uncertainty, showed that the task set was not too difficult; the observers learned, with practice, to judge consistently as they were instructed. The following principal experiments (two observers) led to the conclusion that "the law of correspondence is fulfilled within the limits of accuracy, so that no systematic error in the determination of brightness-equivalents can be deduced from the deviations."] Literaturbericht: Referate.

NOTES

An Ancient Record of Righthandedness

In the course of an article by G. G. MacCurdy on The Field of Palaeolithic Art (*American Anthropologist*, 26, no. 1, 1924) there are some interesting observations on the righthandedness of prehistoric man. The caves of the Pyrenees, which are rich with the art-work of their Palaeolithic inhabitants, show many impressions of the human hand. One hand was pressed against the cavern wall, while the other applied a colored powder over the wall so as to leave a negative imprint when the hand was removed. MacCurdy observes that such imprints are usually of the left hand, and thus indicate righthandedness on the part of the artist. This conclusion is borne out by the results of another method, namely, that of stamping hands. After the palm was dampened, it was covered with dry powdered paint and applied to the wall of the cave. The impressions are generally of the right hand.

Although these observations may add nothing to the solution of the problem of dextral predominance, they call attention to a very ancient record of its existence, since this mural art belongs to the Aurignacian Epoch of the Palaeolithic period, approximately 25,000 years old. Furthermore, they afford a splendid instance of securing data on a personal trait such as motor habit from archaeological remains.

Erna Gunther

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The New Princeton Laboratory

On Jan. 9th was laid the corner stone of the new psychological laboratory of Princeton University. The laboratory has been named Eno Hall, in honor of the principal donor, Mr. H. L. Eno, a research member of the psychological department. The building, of Gothic style, is to be 128 ft. long and 36 ft. deep, with two bays on either side. It will contain 38 rooms, beside lavatories, closets and storage space: two lecture rooms, a library and seminary room, five undergraduate laboratory rooms, seven graduate research rooms, six offices or preceptorial rooms, three editorial rooms, a large studio, three dark rooms, a sound-proof room, three animal-research rooms, a machine shop, a mechanic's room, three apparatus rooms, and a general storage room. The research rooms, offices and preceptorial rooms are to be of substantially the same size and pattern, so that they may be used interchangeably.

The first director of the laboratory will be Professor H. S. Langfeld, hitherto associate professor of psychology at Harvard University. It is hoped that the building may be ready for occupancy at the beginning of the academic year 1924-25.

Eno Hall is, I believe, the first independent building especially designed for psychology to be erected on the American continent. So far as I know—though I am hazy about Japan—it is the second building of its kind in the world; the first independent laboratory was that built at Moscow, some fifteen years ago, for Professor G. Tschelpanow. It would be interesting to know what has happened to this building in the present Leningrad; letters addressed to Professor Tschelpanow have failed to bring a reply.

E. B. T.

A Note Regarding Retroactive Inhibition After a Twenty-Minute Interval

E. S. Robinson[1] reports that retroactive inhibition is equally effective when the interval between the learning of one group of three-place numbers and another group varies up to 20 min. Five subjects each performed the experiment seven times. A group of 27 students in the writer's laboratory course in psychology find less retroactive inhibition on the average after a 20-min. interval than when the interval is 9 sec.

The procedure of the experment is divided into four parts. In part A the subject learns a pack of 10 nonsense syllables in the following manner. The pack is laid before him with a blank card on top. At intervals of 3 sec., indicated by an automatic bell-signal, he turns over a card and studies the syllables thus disclosed. At the conclusion of the trial of 10 syllables, three signals or 9 sec. are allowed to pass, and the series is repeated. Beginning with the second trial, the subject attempts to anticipate the next syllable as each is disclosed. These trials are continued up to the first errorless series of anticipations. After the usual 9 sec. interval the subject turns to a new set of syllables and learns these to the first errorless series, keeping track of the number of trials required. After the 9 sec. interval following the errorless series he returns to the first pack and retests himself.

In part B of the experiment, which follows after 15 min., he learns a third pack. At the conclusion of this learning he allows an interval exactly equal to the time required to learn the second pack in A, which can easily be calculated from the number of trials required to learn pack 2. At the end of this interval he retests himself on pack 3. He thus has a measure of retention with intervening learning and (B) without intervening learning.

Part C is a repetition of part A with the exception that 20 min. are allowed between the first pack (4) and the second (5). In part D the procedure of part B is followed with the newly calculated interval corresponding to the learning-time for pack 5 *plus* the 20 min. inserted between pack 4 and pack 5.

The effect of retroactive inhibition would be the reduction of the amount retained of pack 1 as compared with the retention of pack 3, because of the intervening practice with pack 2 in the case of the former. According to Robinson's findings, the same degree of effect should obtain for pack 4 as compared with pack 6 which was used in D. The following results, however, were obtained by the 27 subjects:

Number of syllables correctly anticipated

	A Retest pack 1	B Retest pack 3	C Retest pack 4	D Retest pack 6
Mean	3.4	7.4	3.8	6.5
S. D.	1.71	1.43	2.14	1.69

The effect of retroactive inhibition is quite clear in the score for pack 1 compared with pack 3. Retroactive inhibition also appears when pack 4 is compared with pack 6. But in the latter case the effect is not as pronounced. Not only is the score higher for 4 than for 1, but the longer period used in C and D before reproduction was called for results in a decrease in retention of pack 6 when compared with retention of pack 3—entirely offsetting any practice effects gained in learning nonsense syllables—whereas pack 4 not only does not suffer corresponding decrease but shows an actual gain.

Study of the individual results shows that, in at least half of the cases, retroactive inhibition appears to diminish when the 20 min. interval is used. Although the differences between the means of A and C and of B

[1]Psychological Monographs, 28, 6, 1920, 36-40.

and D are 1.1 P.E. diff. A−C and 3.1 P.E. diff. B−D respectively, the results obtained make it evident that the interval of 20 min. *may* reduce the retroactive effect upon the retention of the original material, and indicate the desirability of further investigation upon this question.

Yale University By LLEWELLYN T. SPENCER

FACTS AND THEORY IN AUDITORY ANALYSIS

Analyses of tonal sensations are usually made upon the basis of experimental observation, phenomenological observation or ability to explain the facts of auditory combination. Ortmann[1] has given us an analysis on a new basis, ability to explain the feeling-tone of the sensations and, indirectly, musical appreciation. The novelty of this analysis challenges the critical reader to a questioning of its basis, its procedure and its adequacy.

To summarize this analysis, we may note that but one primary attribute of tones is postulated, extensity. This attribute is not identified with the volumic aspect of tones, which is placed elsewhere in the system, but with extensity of the tectorial membrane set into vibration. Moreover, the primary attribute of extensity is manifested in three forms: transtensity (pitch), intensity, and protensity (duration). The quality of a tone is a secondary attribute which depends upon the relative values of the three forms of the primary attribute. By quality is meant such characteristics as flatness, sharpness, volume, depth, breadth, vagueness, clearness, etc. Finally, there are tertiary attributes which are in no sense inherent in the tone but are the result of associations through contiguity and through similarity. The tertiary attributes include such characteristics as brightness, hardness, height (as applied to pitch) and the near-high, far-low associations.

Is this description of tonal characteristics adequate to the purpose for which it was conceived? Its author is able to state in the terms which he develops the conditions under which a tone is pleasant or unpleasant, although he gives no experimental verification of those conditions. To this extent it answers his purpose. But it does not appear to lead to any satisfactory explanation of musical appreciation for the reason that, as Ortmann admits,[2] a large part of musical appreciation is non-auditory in character.

We may next raise the question whether it is a legitimate scientific procedure to base an analytic description of a portion of experience upon its adequacy to explain further facts. Such a description must necessarily be theory, not fact. But the purpose of a theory is to fill those gaps in our knowledge where facts are lacking. Today, however, we have a vast array of facts concerning auditory sensations. Since the facts are known, is a description based upon explanatory value and ignoring fact more than hypothetical (even though it be a useful hypothesis)?

We find a still more startling anomaly in Ortmann's analysis of tone. Not only is it a theoretical description of known phenomena, but in addition it is a psychological description built out of a physiological theory—a reversal of the usual procedure. We have in psychology many physiological theories, examples of which are the theories of vision and of audition. Their purpose is to explain psychological phenomena in physiological terms, and their value is measured by the number of psychological facts that they are able to explain. But Ortmann follows the opposite procedure. Assuming that the extensity theory as applied to the tectorial membrane is an adequate description of the physiology of hearing, he proceeds to build up

[1]O. Ortmann, The Sensorial Basis of Music Appreciation, *Jour. of Comp. Psychol.*, 2, 1922, 227ff.

[2]*Ibid.*, 255.

an account of the very psychological facts which that theory should explain. His account is thus doubly insecure, for the extensity theory is far from adequate, especially as applied to the tectorial membrane. It cannot explain more than a minority of the known facts of tonal hearing, nor does it compare favorably with other theories as regards explanatory value.

If, on the other hand, we question the adequacy of Ortmann's description to the known facts of hearing, we see that the attributes which it postulates are not in accord with those found by observation. The primary attribute of extensity is not identified with the extensive attribute of tonal experience (volume), but with the highly theoretical concept of extent of tectorial membrane set into vibration. Moreover, the characteristic similarity of octaves (tonality) is not included in the description. While the attributive status of tonality is not as yet fixed, a sufficient number of observations have accumulated to demand that this characteristic be taken into account in any systematic description of tones.

It is evident, then, that in the field of hearing, as in any other field of psychology, a systematic description of mental phenomena is adequate and valuable only when it starts with observed fact. An account built upon a foundation of questionable physiological theory cannot satisfactorily describe psychological phenomena.

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STUDIES FROM THE CRACOW LABORATORY

We are glad to note the publication, by Professor W. Heinrich, of a volume (391 pp.) entitled *Travaux du laboratoire de psychologie expérimentale de l' Université de Cracovie.* After a general paper on *Le problème de la méthode,* by W. Heinrich, the contents of the volume fall into three divisions. I. Investigations of the psychology of space: W. Heinrich, *Le problème psychologique de l'espace*; I. Zajac, *La localisation en profondeur des images doubles*; M. Boniecka, *Les courbes de repère de la vision monoculaire dans la lumière homogène*; II. Investigations of the psychology of attention: W. Heinrich, *Les problèmes*; W. Heinrich *Sur la fonction de la membrane du tympan* (previously published in the Bulletin of the Cracow Academy); M. Falski, *Recherches sur l'acte de lecture* (abridgement, by Heinrich, of a doctorate thesis); C. Sobolewska, *Recherches sur les rapports entre la perception et la reproduction des images* (abridgement of a doctorate thesis); C. Bankowska, *Note sur l'acuité de la perception visuelle dans la vision peripherique*; III. Plethysmographical studies: H. Trzcinska, *Le travail mental et la courbe plétysmographique.*

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A THEORY OF EMOTIONS AND AFFECTION BASED UPON SYSTOLIC BLOOD PRESSURE STUDIES

By William M. Marston, New York, N. Y.

I. Problem

It has so chanced that, during the last year, I have been faced with the task of compiling and attempting to interpret, simultaneously, the results of several separate systolic blood-pressure studies upon the emotional and affective reactions of adult human subjects. The original Deception Test experiments were begun in the Harvard Laboratory in 1913, and have been followed by various laboratory and applied blood-pressure investigations made by E. H. Marston, students, and myself, to the end of the academic year 1922-23. Besides Deception Test results, the researches which I have been attempting to analyze comprise the following: Sex Characteristics of Systolic Blood Pressure[1] (laboratory study); Sex and Individual Blood-Pressure Characteristics during Association Reaction-Time Work (laboratory study); Negro Blood-Pressure Characteristics (field study); Individual Female Blood-Pressure Characteristics, with Special Reference to Menstrual Variations (controlled study); Special Appetitive and Sex Studies (laboratory and applied). While I have found systolic blood-pressure change to be an exceedingly sensitive indicator of emotion, its very sensitivity to affective influence produces a complexity of resulting curves scarcely less difficult to interpret, qualitatively, than are galvanometric measurements, except where a "natural" control can be obtained, as in the case of applied Deception Tests. It became evident, therefore, that unified interpretation of the several blood-pressure studies enumerated above required simultaneous study and comparison of all results, together with experimental notes of subjects' behavior and introspection pertaining to each separate set of data. This general analysis I

undertook at the beginning of the academic year 1922-23, making such supplemental studies as seemed indicated to check up conclusions as the analysis was carried forward. Since the total data cover a very wide emotional and affective field, but cover it only very thinly, it seems advisable to reverse the usual order of presentation, and to sketch as a thesis, rather than as a conclusion, that rough skeleton of theory which seemed inevitably to rise from the total analysis. By this form of presentation, the reader may have before him this somewhat complicated theoretical structure, as a unit, in considering the application of the data and literature, when presented in detail, to the general theoretical interpretation. I do not for a moment advance the contention that the results thus assembled and cross-sectioned *prove* the theory of feelings and emotions set forth below; I merely venture to submit that fundamental interpretation which the coordinated analysis of results just completed suggested to me, in the hope that other investigators may rally to the support or attack of the theory in question, with more comprehensive data from other fields.

II. Proposed Theory

In a deductive exposition of emotional and affective theory, it may be as well to proceed from the more complex elements suggested as emotional primaries to the simpler ones. Nor is it paradoxical to characterize elementary or primary emotions as "complex," since psychologists at the present time are in general agreement as to both the physiological and psychological complexity of even the simplest emotional responses. For this reason, a search for primary emotions need not be hampered by the possible criticism, now advanced against previously postulated primary sensations, that such primaries possess only an academic, and not a psychological, existence.

(1) *Fear and Rage*

James said we are afraid because we run away, and we are angry because we attack. He also said that the emotions of fear and anger are the sensations, mostly of visceral and vasomotor origin, which occur during such flight or attack. These two statements have usually been treated as one; I regard them as widely different in both significance and application.

Struggle as we may against the inevitable implications in the affective results of Sherrington, Cannon, and the physiologists, we are, it seems to me, struggling against our own better judgment when we try to stretch the meager and sporadic visceral sensations into distinctive emotional floods of fear and rage; nor are we aided, to any great extent, when we add sensations possibly resulting from those vasomotor changes which both Langley[2] and Cannon,[3] in their work of expounding the mechanisms of the autonomic nervous system, have definitely proved so similar. Reluctant as I always have been to reject the physiologists' refutation of James' identification of emotion with special groups of sensations, my own results, both intro-

spective and behavioristically considered, made it impossible for me to subscribe to the current James-Lange formula, if I hoped to find a unified interpretation of all the studies before me. If, on the other hand, we are obliged to concede a central element in each emotional arc which gives its conscious correlate distinctive primary quality, it seems mystical, to say the least, to assume an altogether new and unique *type* of nervous path, or pattern, which would have to rank with sensation-mechanisms as an indivisible and elementary unit in our mental structure. Psychological theory is greatly clarified, it will be agreed, if sensations can be thought of as comprising all the original materials furnished by the body to the brain: the contents of consciousness, no matter how complex, thus being limited to sensations and to awareness of relations between sensations. To preserve this effective simplicity of theory seems to me to be the motive—unconscious perhaps—behind the persistent tendency of a majority of contributors to the contemporary literature to cling desperately to the original expression of James' theory of emotions, rejecting what seems to me to be the factual necessity, imposed by physiological results, of accepting a central localization for the differential element in our primary emotions. If we are willing simply to admit that James was wrong in his statement identifying emotion with sensation, and then turn to a more critical examination of his first suggestion that "we are afraid because we run," we may be able to attain our desired end of psychological simplicity of theory, without flying in the face of modern physiological fact.

Fear

If, then, we turn to a critical consideration of James' statement that we are afraid because we run away, we come first upon the apparent qualification that we are afraid when we run away from some things, and not afraid when we run away from others. A boy playing tag is not afraid when he runs from his play-fellow, who happens to be "it." A football player is not usually afraid while running away from an opponent for a touchdown. The boy might, of course, be afraid if he were running away from his father who wanted to give him a whipping; and the football player might be afraid if he were running away from a maniac who was shooting at him as he ran. Yet these instances of fear accompanying flight from dangerous objects must again be qualified. The fear experienced by our fugitives will vary in almost exactly inverse proportion to the *success of their flight*. As the small boy perceives that his father's unusual exertion is causing a shortage of breath, with consequent rapid increase of the distance between pursued and pursuer, elation begins to replace fear. But the boy is still running away. Or we may very easily suppose an instance where an experienced hunter, let us say, running for his life from an enraged bear, feels only exhilaration in the contest from the first, with no slightest tinge of fear. Surely, then, we must modify the general statement that fear *always* accompanies running from danger; although there remains the persistent idea, at the back of our minds, that both boy and football player would feel less fear, or possibly none at all, if they met their respective dangers with fight instead of flight.

Let us approach the problem from another angle. It is generally assumed by psychologists, especially absorbed in attempting to explain all human mechanisms by theoretical reconstructions of prehistoric racial history, that fear *increases the powers* of the fleeing organism, enabling it to overcome obstacles insurmountable in the absence of fear. Cannon emphasizes this conception, devoting, in fact, considerable space to enlarging upon the necessity of retaining fear and anger in human activities as "a great source of reserve energy"[4] in times of stress. This theory, it seems to me, checks up very poorly with psychophysiological and psychoanalytical results. It is difficult to discover the benefit which psychotic patients derive from that exaggeration of normal fear which may be at the root of their difficulty; nor is it apparent how the delayed reaction-times resulting, it is usually supposed, from normal fear of detection in the Association Reaction-Time Tests can be interpreted as an indicator of increased energy in those portions of the organism which are engaged in attempted escape from danger. No more, in every day experience, do we find ourselves gaining efficiency of action in proportion to the increase of our fear. It is generally admitted that, as James pointed out, we feel less fear if we oppose a threatening danger boldly, and more fear if we run from it. But if, in the first instance, when we ran instead of struggling, fear was found correlated with *inhibition* of maximally efficient action, why should we expect to find the order reversed, once we begin to run, with fear now correlated with *reinforcement* of efficient action? Or, if it is actually the most efficient course of action to run, as in the case of the unarmed hunter attacked by an enraged bear, shall we hold that fear would render his running more effective than the feeling of exhilaration? Not, at least, according to Cannon, for he describes the most efficient mental state of a football player before a big game not as dread or fear, but as a feeling of "such access of strength that he felt able, on the signal, to crouch and with a jump go crashing through any ordinary door".[5] Great danger, certainly, is often accompanied by great "excitement," which utilizes bodily reserves, and increases normal muscular performance. But, without citing in detail the accumulated proofs that, psychologically, fear is *not* efficient, I may say that careful consideration of my own results, especially those obtained upon the most successful performers in conditioned Association Reaction Tests,[6] has led me to believe that the effective element in the "excitement" produced by great danger is pure *appetitive, or self-enlarging emotion*, while the negative, hindering, opposing, inhibiting factor, whenever present simultaneously with appetite, produces the state of consciousness known as *fear*.

The psychological definition of "coward" seems clearly to be a man who is kept by fear from doing something that would be to his benefit. If a prize-fighter battles the champion, certainly it is not fear that causes him to fight, nor is it anger. It is pure acquisitiveness; he *wants to enlarge himself*, first, by getting his share of the gate receipts, and, secondly, if it is a real fight, by attaining the championship. If the contender quits during the fight, it means that *fear* of one sort or another has inhibited, somewhere within his nervous system, that driving motive of self-enlargement which led him to undertake the fight. For the moment, we are concerned with the fact and nature of this inhibition, rather than with the mechanism of the appetitive motive itself, which has its place in the next step of our attempted simplification by analysis. The contender enters the ring with the champion, with a will-setting strongly directed toward knocking the other man out. The champion batters him with heavy blows, and at first the very intensity of this stimulation merely increases the power of the contender's responses. Each time he is hit, as long as his will-setting remains intact in its original appetitive direction, the fighter hits back harder than ever. But, if the man is a "coward," the perception of his opponent's strength, skill, and apparent superiority will begin to creep *inside the contender's consciousness.* As long as the struggle between the fighters remains objective, the only way either one can win the fight is to knock the other out, *physically.* But the moment the struggle gets inside the nervous system of one or the other man, that man begins to be knocked out *psychically*; and *that is fear.* Slight inhibitions in the intensity, vigor, and accuracy of the contender's responses begin to take place, and these inhibitions pile up rapidly, until the *fighting* reactions are cut off altogether. At this point the contender begins to *retreat,* or "run away," as James puts it. The state of partial inhibition of the appetitive drive, or will-act, which is fear, begins, in this instance, some time before actual flight is commenced. The final retreat, or flight, merely *reveals the nature and direction* of the motor impulses which were warring within the contender's nervous system against those nervous impulses which had hitherto been in control. In short, the set of impulses flowing from the contender's appetitive will-setting tended to produce the total response of vigorous fighting, whereas these appetitive impulses were first gradually inhibited, and finally overcome, by a new set of impulses having their origin in sensations forced upon the contender by his opponent, which tend to produce a total response of retreat. Once retreat is accepted as a new appetitive end by the contender, however, fear will diminish once more, in proportion as this new type of willed conduct is successful in gaining its object of self-enlarge-

ment by protection from injury. If we refer back to our former example of the boy running from his father to avoid a whipping, it is perfectly plain that his fear must diminish, as we saw, in fact, it did, in proportion to the success of his flight, since, in proportion as his new appetitive end of escape is attained, the struggle which is going on *within* the boy, against this end, will tend to *diminish*, as it did with the pugilist after he once *willed to retreat*. The unarmed hunter who runs from the bear is an experienced, and therefore a "brave" man; he exerts his trained powers in a purely *objective* struggle with the enraged animal; and, since there is no struggle within his nervous system against the appetitive motor impulses directed toward saving his life by flight, he experiences no fear, but only exhilaration in the free exercise of his physical strength. In the light of this analysis, I would modify the famous James statement as follows: We are not afraid because we run away from the bear; *we are afraid because we see we are not running away fast enough.*

Of course, in running away, it is always possible that the new appetitive end of escape by flight will not be wholly accepted by the organism as its will-setting. If, for example, the experienced hunter's best judgment tells him that he need not run from the bear, but can overcome or put the animal to flight by standing up to it with a club, then the act of running would, as James supposed, *cause* fear. But, in this case, the appetitive will-setting would be directed, during the flight, *not* toward saving the fugitive's life, but toward *overcoming the bear*. As long as I continue to wish to enlarge myself by overcoming an opponent, and, at the same time, allow the more deep-seated will-end of saving myself possible pain or injury to control the motor paths to my skeletal muscles, I have committed myself to a sort of *voluntary fear*. In such case, it is not the motor impulses from sensations forced upon me by the dangerous object that enter into contest with impulses directed toward conquest of that object, but rather impulses flowing from *another appetitive will-setting* within my own organism. The struggle, in the instance suggested by James, takes place in a single flash, and is almost wholly concluded in favor of the deeper-seated will-setting at the very outset. A vague trace of the conquest-setting remains, however; not sufficient in strength to produce noticeable motor inhibition, but rather registering in consciousness as *shame* at running away. Such contest within the subject's nervous system between two opposed appetitive will-settings constitutes a sort of subjective fear, having little or no reference to sensations originating in the dangerous object, at the moment, or to the motor impulses flowing from such sensations. In fact, the latter are often virtually eliminated by the struggle of will-settings, as when a squirrel approaches a nut lying in an open space, only to retreat sharply without the slightest change of external stimulus; repeating this behavior over and over, and becoming more and more oblivious to sensations of objective origin warning of real danger, until finally a cat may run almost up to the squirrel before the latter reacts in the least to its cat-sensations. With human beings, a struggle of this sort, accompanied by subjective fear, is said to give rise to "moral courage" whenever the less deep-seated will-setting wins through to motor expression over the more archaic, or deep-rooted one. I do not believe this is the phenomenon James referred to, in his example of running from the bear; but would rather say he had in mind *true* fear, or internal struggle between being-caught and escape impulses, continuing as we run.

Finally, it may be of value to consider the application of the definition of fear suggested above to the common experience of the dissipation of fear by 'acting as if we were not afraid.' Roosevelt reports, in his autobiography:[7] "There were all kinds of things of which I was afraid at first, ranging from grizzly bears to 'mean' horses and gun-fighters; but by acting as though I was not afraid, I gradually ceased to be afraid." I take this statement to be accurate introspection, for the very reason that it emphasizes the *gradual* cessation of the emotion of fear as a response to stimulation by various dangerous objects, in proportion as the struggle between Roosevelt and the objects was gradually *ejected from his nervous system*, and carried on as an objective contest only. It is only while the conflict between appetitive and anti-appetitive motor impulses is going on within the organism that the experience known as fear obtains. The probability that anti-appetitive motor impulses will gain outlet through the nervous system of the organism depends wholly upon the nature of the unit-perception[8] of the relation existing between the organism and the appetitive opponent. As long as the subject is *fighting* the bear, horse, or gun-man, the unit-perception of the relationship between subject and opponent is less likely to gain anti-appetitive motor expression, because the relationship perceived is less unfavorable to the end willed, or desired by the subject. When the subject has decided upon flight, even though in making the decision he may have remained uninfluenced by fear, the perception that he is forced to run away in itself connotes that he is weaker than his opponents, and this perception is, therefore, just so much more unfavorable to the end willed than was the perception of a fighting relation between subject and opponent. If the subject can force himself to go through the motions of fighting, or of behaving as if he were exactly as strong as his opponent, he thereby furnishes a unit-perception for his own senses which has far less chance of setting up opposition, in his nervous system, against those motor impulses which are being directed by his will-image or will-setting toward the end of self-enlargement. The subject, in such cases, is an actor, performing before himself as audience, for the purpose of furnishing a show which will eliminate any desire, on the part of the spectator, to leave the theater. If this performance is sufficiently convincing, and can be continued long enough, the dramatic illusion is bound to supersede, to a greater or less extent, an awareness of the world of reality. Former perceptions of a stronger-weaker relation between opponent and self become mere memories, and these memories grow fainter and fainter. It is not, however, until the last such memory has been completely eliminated from participation in the total motor expression of the organism that fear wholly ceases; because, until that time, anti-appetitive impulses flowing from the memory-images of the superiority of the opponent will continue to struggle with the motor impulses directed by the subject's will-setting toward self-enlargement, for control of the motor outlets of the organism. If the acting is not wholly convincing, or if it is not uniformly continuous, new perceptions of the opponent's superiority will be received by the subject's senses, and the motor conflict, or fear, will be renewed from this source.

Such analysis of Roosevelt's experience serves to explain the everyday fact, noted under our initial review of James' statement about fear, that fear more frequently accompanies the saving of the self by flight than it does the accomplishment of the same appetitive end by fight or attack. While the subject is attacking his opponent, he is furnishing a convincing drama depicting his equality with, or even his superiority to, his opponent. But while the subject is fleeing from an opponent, the drama is equally persuasive of his opponent's superiority. In war, "strategic retreats" are the most difficult of maneuvers simply because the drama is so convincing that the retreating army itself is convinced of its opponent's superiority, becomes afraid, and the stratagem is likely to be turned thereby into a

reality. On the other hand, situations are not rare where fear decreases tremendously when fight is suddenly turned into flight. If a small body of men is sent out to attack a strongly fortified position, frank introspection reveals that fear increases proportionately with their increased perceptions of the enemy's strength, and decreases sharply, or even ceases altogether, when the attack is given up, and they are free to run back toward their own lines. During attack, in such cases, perceptions of the opponent's superiority increase steadily, except as such perceptions may be obscured or inhibited by the subjects' own acting; whereas during flight, in which they know the enemy can not pursue them, perceptions of the stronger-weaker relation between self and opponent steadily fade out, or cease altogether, and fear similarly ceases. As long as we entertain perceptions of our opponent's superiority, while we are fighting him, we shall *tend to run away*, whereas *we want to fight;* and this conflict is the correlate of fear. And, if we elect to run away, and simultaneously allow ourselves to harbor perceptions of our opponent's catching us, we shall *tend to be caught* although *we will to run faster;* and here, again, we experience the conflict which is accompanied by fear. To refer, once more, to the bear-case cited by both James and Roosevelt, our fear does not cease when we have trained ourselves not to run away from the bear, but only when we have trained ourselves *not to perceive the bear's superiority* while we are fighting it. And, if we decide to flee from the animal, our fear ceases only when we have trained ourselves *not to perceive that we are being caught* while we are running away.

I should define fear, then, as the awareness of a motor conflict within the organism, at a nervous level correlated with consciousness, between motor impulses flowing from an appetitive will-setting, and motor impulses from anti-appetitive perceptions. An "appetitive" will-setting, or unit will-image as I propose to use the term, is one directed toward self-enlargement; and the source and nature of such settings must be considered in detail below. An "anti-appetitive" unit-perception image is a unit-perception of environmental factors tending toward self-depletion; and, if we accept in full the ideomotor theory, such a unit-perception, in so far as it is allowed to bring its meaning into consciousness must, in just so far, gain motor outlet through the organism's musculature for impulses *tending to move the organism toward self-depletion.* I will to escape the bear. This is my appetitive will-image. From this setting flow motor impulses moving my legs rapidly. I then perceive that the bear is gaining upon me. In so far as this unit-perception gains the meaning of *being caught by the bear*, in my consciousness, in just so far do its motor impulses slow up the movements of my legs. Unless the inhibiting or slowing impulses did gain just such outlet, I could, of course, have no awareness of being caught, or depleted by the bear. This is my anti-appetitive perception image. Will says: "Run!" Perception says: "Give up!" The state of consciousness correlated with a conflict between these settings, with their constituent and concomitant sensations, is fear.

Anger

The statement "We are angry because we fight" seems to me less significant than James' famous description of fear, considered above. Most fighting, between both men and animals, according to my analysis, is accompanied by the active phase of appetitive emotion, to be considered in the next stage of analytical simplification. The fierceness evidenced in battles between animals, prize-fighters, or business men, has in it, according to my results, no element of conflicting impulses within the nervous systems of the fighters. There is, on the other hand, a maximally efficient unification of motor impulses rigidly enforced by the appetitive will-setting. This is not rage, or anger; for anger, according to the analysis of my introspective results, always contains an element of feeling "defeated", or "baffled." In other words, there is a motor conflict of some sort going on within the organism, between will-setting and perceptions, just as is the case during fear. The distinction is, in my opinion, a crucial one; and is actually very easy of application, even in a purely objective study of animal or human behavior. Appetitive conduct, no matter how violent and aggressive it may be, is perceptibly shaped toward the end of self-enlargement. Angry conduct takes no heed of self-enlargement or self-protection, but is directed only toward destruction of some object outside the self, and, in true anger, includes the self also, to a greater or lesser extent, as an object of destruction.

In any purely appetitive struggle, the opponent wills and expresses in bodily action a unit-image of self-depletion. That is, each prize-fighter is voluntarily shaping his behavior toward depleting his opponent, as a means towards enlarging himself. But this final end of self-enlargement is the controlling one, and the more completely it does control his conduct, the fiercer and more effective his attack becomes. Other, and more deep-seated appetitive ends, such as protecting his body from injury, and conserving his strength for future battles, may be eliminated by the immediate appetitive end of winning this particular fight. It is at this point that his behavior is often said to be expressive of rage, but as long as the fighter's blows are both maximally powerful and maximally well-directed, his behavior expresses only appetite.

If his blows begin to weaken owing to central motor inhibition, it is evidence that a state of fear is setting in. If, on the other hand, he begins to shower blows at random on his opponent, regardless of rules or result, it is evidence that his original appetitive end of winning the fight is being diverted into the slightly different purpose of dominating his opponent at all costs. This type of behavior certainly denotes a motor conflict within the fighter's nervous system, which might well be classified as true anger were it not for the fact that there is not a genuine tendency in such conduct toward self-depletion, or destruction. The fighter, in discarding his skill and craft, as well as his defensive reactions, has certainly laid himself open to greater depletion or injury than before. But his own movements do not tend to bring such depletion about, save as a result of negligence. He does not "rush on the spears," and gather them to his breast. The distinction is rather a fine one, but is, I believe, accurate. There is a conflict of motor patterns; but both patterns are appetitive, and, therefore, both are aimed

at self-enlargement. The first pattern is directed toward winning this prize-fight; the second, and interfering pattern is directed toward dominating a particular individual, regardless of winning the fight. The opponent has succeeded in substituting himself, within the will-image of the fighter, for the fight decision, as central object in the end to be attained. There is no radical variance between the direction of the motor impulses flowing from the appetitive image of winning this fight, and the direction of the motor impulses flowing from the appetitive image of subduing this individual at all costs; but there is just enough variance to inject "wildness" into the fighter's behavior, and to add that tinge of "baffledness" or "defeat" to the fighter's consciousness which is the conscious correlate of the blocking, or partial blocking, of motor impulses from the original appetitive will-setting, within the central nervous system. This state of the organism is very close to anger, and constitutes, in fact, the type of response most frequently produced in laboratory animals and human beings, upon interruption of various appetitive will-settings, and reported as true anger. Since that motor element of self-depletion which constitutes, according to my analysis, the peculiar differential criterion of true anger is missing, however, I shall call this defeat, or diversion of one appetitive setting by another, "pseudo-anger."

On just the other side of the line, however, lie many cases of true anger *following* the complete defeat, or elimination of an appetitive will-setting. If an appetitive opponent succeeds in substituting the essence of his will-setting for the appetitive one originally held by the subject, then anger results. The essence of the opponent's will-setting which must be accepted as the subject's own is depletion of the subject. The unit-perception image will then include a substitution of opponent for self in relation to the property or object formerly possessed by the subject, and now possessed by the opponent. The result will be a depletion-response directed mostly at the self, but always in conflict with a tendency to direct it toward the victorious opponent, in so far as motor impulses from the unit-perception image of substitution of opponent for self are able to gain outlet through the subject's organism. This is true anger, but of an unstable type, usually tending to revert to the original appetitive response, automatically, after a very brief interval, since the will-image of self-depletion has been artificially imposed. A little boy, for instance, asks his mother for a big piece of cake. She represents his appetitive opponent when she refuses to let him have the big piece he wants. Finally, she consents to his having a little piece. The humiliation of appetitive defeat has been thrust upon the boy. This victory of his appetitive opponent may loom larger, for the moment, than his desire for cake; and if so, he may adopt for his own his mother's will-image of self-depletion. In this case, he rejects the little piece of cake, perhaps forcibly, and begins to cry. In short, "he cuts off his nose to spite his face." The rejection of the smaller piece is action proceeding from the child's artificially adopted will-image of self-depletion; while the forcibleness of such rejection, and the crying and possible pounding of the table, are the bodily expression of motor impulses directed by his unit-perception image of his mother substituted for himself as possessor of the piece of cake he wanted. The instability of this self-depletion will-setting, however, is usually evidenced by the little boy's hurried grasping for the smaller piece of cake, if his mother threatens to remove it from the table altogether. Sometimes, in adults, this type of self-depletion motor setting, following defeat of some deeply ingrained appetitive response, persists long enough for the subject to commit suicide; but if the means chosen is not a rapid one, or if, in the course of the act, sensations arousing still more deeply seated appetitive responses than the one defeated gain the subject's attention, his will-setting immediately reverts to an appetitive one, and the perception of suicidal environment becomes a stimulus to fear.

The generic source of that type of motor conflict within the organism which constitutes true anger seems to be sex-rivalry. As I shall attempt to set forth at greater length below, the sex will-setting is one of self-depletion. The animal or human sex-subject wills that the sex-object shall deplete him, in the fulfilment of some vital need or desire of that sex-object. But, since no organism can escape from the egocentric predicament, this typical sex will-setting takes the form of active self-depletion, with the self in both rôles of depleter and thing depleted. In its rôle of depleter, however, it is a mere substitute for the sex-object; and, during the actual sexual act, the sex-object does, in fact, actually become the active depleter, since it directly stimulates reflex mechanisms by which part of the self is ejected to serve the needs of the sex-object. The essence of the sex will-image, therefore, is the self as thing depleted; just as the essence of the appetitive will-setting is the self as thing enlarged. If it were true that the essence of sex-desire is the wish to *own*, or *possess* the sex-object, then intrusion of a rival would simply result in a true appetitive struggle, with each contestant acting primarily to enlarge himself. Sex-combats give every evidence, however, of reckless abandon of self, and, in fact, of an admixture of behavior voluntarily directed toward self-depletion. In no instance of animal appetitive contests do we find the opponents mutually respecting a set of rules operating, to a considerable extent, as a handicap in the gaining of the end desired. Yet not only are animal courtships governed by formalities more or less strictly observed, but even the combat of rivals, in many species, is carried out in accordance with a mutually respected form of procedure. Challenges to open fight are issued, and accepted, and other males present refrain from interference, in most instances. All these items of behavior are in the nature of deliberately willed limitations placed by the animal upon itself, and could not, therefore, proceed from a will-setting directed toward self-enlargement. The same animal, when in search of prey, leaps upon its victim from ambush, even if the animal attacked is very much smaller than the attacker. In an appetitive struggle no slightest advantage is sacrificed, and every movement—save those symptomatic of a state of fear—is directed toward the greatest possible self-enlargement.

If, as I suggest, the sex will-image is one of self-depletion, combats of sex rivalry, accompanied by that state of motor conflict by which I have defined anger, are very easily explicable. Each sex-subject wills to deplete himself, at the dictation of the mutual sex-object. The intrusion of a rival tends to force upon the subject the unit-perception of the rival as thing depleted by their mutual sex-object, in place of the self. In just so far as this perception of the substitution of rival for

self is forced upon the sex-subject, to exactly the same extent will those motor depletion impulses which were directed by the will-setting against the self, now be diverted toward the rival. The result will be a will-setting of depletion, with a conflict of motor direction of the resulting impulses as between self and rival; which total condition of the organism will have anger for its conscious correlate (just as the same type of motor conflict following the artificial substitution of a will-setting of self-depletion for the appetitive setting of self-enlargement was found to be accompanied by true, though transitory, anger). In the sex-rivalry anger-response, however, a much larger proportion of the total depletion-impulses would seem to be directed against the opponent than in the post-appetitive anger situation. Human suicides, following loss of a lover to a rival, are fairly frequent; but usually the rival is killed first, and the subject's ensuing suicide, if it occurs, is attributable, perhaps, in part, to the cutting off of appetitive setting toward life itself, on account of anticipated social complications inevitably following murder. In general, however, sex-rivalry anger represents a much more stable and enduring motor condition of the organism than is found in post-appetitive anger; and is, in fact, likely to persist as a response to perception of the rival as long as the original sex will-setting toward the particular sex-object persists. In sex-rivalry anger, moreover, is probably to be found the original source of that definite motor conflict pattern, into which the peculiar, though frequently recurring, post-appetitive emotional situation above considered falls.

The anger of a mother, animal or human, aroused by the intrusion of any object or organism between herself and her offspring, constitutes a still more objectively directed and enduring condition of motor conflict of the type described. The material reaction to the young constitutes, according to my analysis, a singularly pure type of true sex-response; and it seems to me wholly unnecessary to postulate any additional "instinct," or primary emotion, wherewith to describe this reaction. The mother simply acts under the control of a strongly organized self-depletion will-setting. She wills to serve her young; to deplete herself, both by furnishing milk from her own body, and by expending her strength in their service. If the essence of sex is to make the self the thing depleted by the sex-object, what clearer sex-setting is possible than that of the mother toward her offspring? In such a definitely organized setting, perception of the interposition of any strange object or organism between mother and young is likely to effect an almost complete subsitution of the intruding object for the mother's self, as the object toward which her depletion impulses are directed. The state of motor conflict, within the

mother's nervous system, between the impulses directed toward depletion or sacrifice of herself for the protection of her young, and the impulses directed toward depletion or destruction of the intervening object, constitute a long enduring, as well as an exceedingly intense state of true anger.

Finally, in considering interruptions of sex-response which give rise to anger, the most enduring of all, as well as the most objectively directed, would seem to be that occasioned, under certain conditions, by rejection of a female subject by the sex-object. According to indications reported in a previous paper,[1] the sex-responses of female human subjects are much more extensive than are those of males, and are found to be organized in larger units. From this result it is, perhaps, a fair conclusion that a female subject's sex will-impulses of self-depletion constitute a much larger proportion of her total motor activity than do the sex-impulses of the male. Therefore, when the substitution of the sex-object for herself as the object toward which those depletion impulses are directed is accomplished, her attack upon the substituted object must be correspondingly extensive and enduring. The resulting reaction, in human subjects at least, would seem to justify the well-known literary precept:[9]

> "Heaven hath no rage like love to hatred turned,
> Nor hell a fury like a woman scorned."

By rejection of all the services and substance offered by the sex-subject, the sex-object has transformed himself into a complete barrier interposed between the subject's self as depleter and the same self as thing depleted. Since the sex-object was, in the first place, a stimulus to the subject to act as depleter, he continues to be such a stimulus, necessarily, since he can not change his appearance or nature as sex-object. But, on the other hand, he is the only organism or object with the power to eliminate completely, by rejection, the sex-subject's self as thing depleted, and, in so doing, to substitute, almost as completely, himself for the subject's self as thing depleted, within the sex will-image of the subject. Since the depletion-impulses are identical in sex-response and anger, in this instance, the stimulus remaining the same, the "hate" or anger-attack upon the sex-object is bound to be exactly as extensive and enduring as was the original "love" behavior, or offering of the services and substance of the self to the sex-object. In this type of anger, the conflict of directions of motor impulses is almost completely resolved in favor of the objective direction, in contrast to the post-appetitive type of anger where scarcely any substitution of object for self takes place, and the depletion-impulses are consequently almost wholly directed against the self. Between these extremes lies a great number of types of

blockages of sex-responses, differing only in the proportion of the total depletion-impulses directed toward self and toward intruding object.

The above resolution of fear and anger into motor conflicts of distinctive types of action-patterns places these responses upon a purely mechanical basis; and places the neurological correlate of the distinctive conscious qualities of fear and anger solely upon the motor side of each individual sensation arc, summated, in all probability, for each unit-pattern, in cortical paths furnishing, as conscious correlates, symbols of conflict-relationships existing between the definite types of motor impulses involved in the conflict. The fear-type of motor conflict is between enlargement and depletion impulses; while the anger-type of motor conflict is between the direction of existing impulses toward the self and toward another object. Obviously, neither type of motor conflict can be as effective, in physical work performed, as any unified and non-conflicting group of constituent impulses. Pure appetitive behavior is bound to be more efficient than is behavior resulting from a conflict between appetitive and anti-appetitive impulses. And pure sex-responses will prove, inevitably, more effective than a response of the same type which the organism is seeking to direct against two different objects simultaneously. But, if a choice must be made between the two types of motor conflicts summarized here, anger should prove more efficient than fear, since there is less motor conflict involved. In fear, motor impulses of reciprocal opposition are struggling against each other, within the organism; whereas, in anger, only the direction in which the same impulses shall be pointed is in issue. Once we get over the notion that escape from danger is a fear-response, and place self-preservative retreat in the same appetitive category as the procuring of food, we find our everyday experience confirming the above theoretical conclusion as to the relative efficacy of fear and anger. Anger may remove a sex-obstacle before the subject himself is destroyed; whereas fear tends only to defeat the most vital needs of the subject. If my analysis is correct, however, both fear and anger represent imperfections in natural evolution, and not biologically useful types of response. Appetitive response was developed to build up the individual organism, and sex-response was evolved for the purpose of giving the individual organism something to do with the power it had acquired. The perpetuation of the race, as I see it, goes on continuously, almost independent of the lives of individual organisms, since the germ cells are practically isolated from their somatic environment, are probably capable of perpetual life, and might easily be stimulated to combination and reproduction by mechanical means, as happens in the cases of some lower organisms. The individual sex-mechanisms serve, in essence, only to furnish each organism with a constructive outlet for that substance and energy which is taken in through the appetitive mechanisms. Fear and anger constitute the primary types of opposition to appetite and sex, respectively, within the individual organism; and all other emotional patterns are either variants or complexes of appetite and sex, fear and anger.

(2) *Appetite and Sex*

In the course of the above analysis of the complex emotional responses of fear and anger, "will-settings" or "will-images" of appetite and sex were referred to, in contradistinction to motor settings, patterns, or images flowing from the sensations constituting a unit-perception contradictory to the will-image. In the ordinary nomenclature of psychology, such a distinction would probably not be permissible, since motor patterns set by

any sensation or perception unit would equally deserve the name "will-settings." I have ventured to limit my use of the terms "will-image" and "will-setting", however, to those motor patterns which are set up by the primary agency of mechanisms inherent in the organism; and since I have found only two such mechanisms, hunger and sex, my use of the term "will" is necessarily confined to motor settings of appetitive and sex types and origin. Of course, it would be absurd to contend that each separate, individual will-setting is the direct result of the operation of one of these mechanisms; but it is my thesis that the two distinctive will-patterns of self-enlargement and self-depletion are formulated by the inherent mechanisms, respectively, of appetite and sex; and that responses within these will-patterns, thereafter, become conditioned upon environmental stimuli both adequate and inadequate to the original responses, selected by the organism upon the general principle, found within the inherent mechanisms, of escaping the unpleasant, and seeking the pleasant. Since my method in this preliminary exposition of emotional theory is an attempted simplification by analysis, it becomes necessary, first, to consider the inherent mechanisms upon which the fundamental will-setting types are postulated; and, secondly, to analyze these biological mechanisms further for the purpose of discovering, if possible, an inherent basis for pleasantness and unpleasantness.

Appetitive Response

By the use of the term "appetitive response" I do not intend to draw a distinction between appetite and hunger, but rather to include both, as defined by Cannon[10], within the inherent supply-mechanism of the organism. I have used the term "appetitive mechanism," since this name would seem to possess more psychological significance than would the term "hunger mechanism." Hunger pangs have been proved by Cannon, Carlson[11], and others, to consist of those sensations which occur during strong contractions of the empty stomach, and possibly of the oesophagus. These contractions seem to be set up, partially by a "hunger hormone" generated by body tissues in need of nourishment, and partially by a specific nervous automatism, both central and peripheral, and independent of afferent impulses.[12] Such a mechanism as is thus described seems totally independent of environmental stimulation, and does not, in fact, depend upon any single factor outside the organism for its origin or development. That the hunger mechanism is inherent is clearly indicated by studies made by Carlson and Ginsburg,[13] upon new-born infants, and upon two pups born 8 to 10 days before the term. Typical periods of tonus and hunger contractions were found, resembling those of adult organisms, with the exception that the contractions of the

infants' stomachs showed relatively greater vigor and frequency. These hunger pangs appeared shortly after birth, and before any food had been taken into the stomach. We have here, clearly, a mechanism designed to initiate appetitive response, which is both inherent and automatic.

Proceeding with the action of this appetitive mechanism, then, we find that it initiates a type of behavior directed toward enlargement of the organism, before the infant is 9 hours old.[13] Cannon describes the action of the organism resulting from hunger pangs as follows:[14] "Hunger, in other words, is normally the signal that the stomach is contracted for action; the unpleasantness of hunger leads to eating; eating starts gastric digestion, and abolishes the sensation." Contained in this succinct exposition of the behavior initiated by hunger pangs, are a considerable number of psychophysiological reactions of the organism, all fitting together to form integral portions of the total appetitive response. Four principal psychophysiological units compose the complete appetitive mechanism: (1) an unpleasant internal stimulus, hunger pangs; (2) motor or will setting toward enlargement of the organism, set up by this internal stimulus, and psychically represented, following the formation of associative connection between stimulus and food, by the will-image of food; (3) motor impulses, directed by this will-setting, which result in behavior directed toward procuring food; (4) eating and swallowing the food procured, which results (*a*) in the cessation of the unpleasant internal stimulus, hunger pangs, and (*b*) in what Cannon calls "the relatively mild pleasures of sight and taste and smell of food".[15]

Once the second unit above mentioned, *i.e.*, the will-setting toward self-enlargement, has become conditioned upon the sight, smell, or other perception of certain food, or other objects as an effective stimulus, then the first step, or adequate internal stimulus may drop out, and the total appetitive response proceeds as before, except that the effectiveness of the substituted stimulus is the element that drops out in the final stage of the response, after the food or other object has been assimilated by the organism, in place of the unpleasant adequate stimulus. In other words, the sight and smell of delicately prepared food may set off the appetitive response, as Cannon points out[16], long after hunger is satisfied; but there comes a point, in the behavior of the most confirmed glutton, where appetizing food no longer tempts him, and he stops eating. The effectiveness of the substituted stimulus, food, has dropped out, just as the adequate stimulus, hunger pangs, ceases after sufficient afferent taste, smell, and swallowing impulses, with reflex motor outlets through the cranial autonomic, have been set up by the natural process of eating to inhibit the hunger pangs; aided, very pos-

sibly, by other reflex mechanisms set off by the filling of the stomach with food.

No "instinct," or other inherited cortical pattern, seems to me to be necessary as prerequisite assumption to an adequate explanation of this extremely simple, yet absolutely basic, appetitive mechanism. The new-born infant needs only a normal alimentary canal, afferent nerve connections between canal and central nervous system, and efferent nerve connections between central nervous system and skeletal muscles, for a bodily reaction to occur in response to hunger pangs (which, in the first place, have been initiated, very probably, by automatic chemical reaction to reagents contained in the blood-stream). The first bodily movements of the new-born infant are, as we know, of the "random" or uncoordinated variety[17]. Many different types of random movements are doubtless set off in response to infant hunger pangs, some observable, and others not readily accessible to inspection. There may, possibly, be prenatal dispositions tending to connections between the sensory center where the afferent impulses from the stomach are received and the motor nerves leading to the larynx and effector organs necessary to produce crying, and other recurring infant responses; although, personally, I doubt it. The "birth cry," caused, of course, by the mechanical inrush of air to the infant lungs, seems to me sufficient to lower the synaptic resistance in the motor centers and afferent paths necessary to the crying response, which would then tend to result from any other unpleasant stimulus, internal or external. Gradually, touch sensations, and later taste sensations, from mouth and lips, would be built into the response, as the infant was nursed; and incidental touch sensations caused by contact between infant hands and mother's breast, or bottle, and coordinated with the grasping reflex, would, still later, gradually carry the appetitive response into a *grasping* type of behavior. From this to the mature adult appetitive behavior, directed toward food and other objects, is a simple, and comparatively short step; and would naturally be expected to develop by the ordinary learning or conditioned-reflex process, without interposition or necessity of any further inherent connective factors. Only the general *type* of motor setting, *i.e.*, a setting to produce self-enlargement, remains unaltered from the first completed appetitive response of life to the last; and it is reasonable to suppose, therefore, that this type of motor setting should become more deeply inwrought as a central, controlling neural pattern of human and animal behavior than any other; as, in fact, it does.

Having suggested a psychophysiological mechanism, inherent in the organism, by which the self-enlargement pattern of behavior characterizing what I term the appetitive response may be built up, it becomes necessary for me to analyze this response from the psychical side, in an effort to determine its conscious emotional correlates. Analysis of my own results shows, briefly, that the appetitive response, in its affective aspect, progresses, if successful, from sharp unpleasantness (hunger pangs), through a mixed pleasantness and unpleasantness (pursuit and capture of food), to the disappearance of initial unpleasantness, and a rather extended pleasantness of slight intensity (cessation of hunger pangs and the eating of acceptable food thereafter). If unsuccessful, appetitive response may progress from unpleasantness (hunger pangs), through a stage of unpleasantness aggravated in both intensity and duration (failure in pursuit, or defeat in attempt to capture

food), to a final condition of extreme unpleasantness (progressive weakening of, or injury to, the body). The distinctive emotional quality accompanying successful appetitive response, throughout, may be designated as *appetitive emotion* and that accompanying unsuccessful appetitive response, if the anti-appetitive situation is allowed motor expression within the nervous system of the organism, is *fear*.

Appetitive response following the initial stimulus, hunger pangs or a substituted effective stimulus, may be conveniently divided into two parts, or aspects: the active aspect, and the passive aspect. Immediately following the formation of the appetitive will-setting, the organism starts into more or less violent action, toward the end of finding and capturing food. This is the aggressive, fighting, *dominant* aspect of the appetitive response; and in proportion as it succeeds, pleasantness from the perception of its success is added, in consciousness, to the unpleasantness of the original hunger pangs, or awareness of unsatisfied craving. This mixture of pleasantness derived from perceptions of accomplishment, by the organism, of the first part or aspect of that appetitive image toward which the response is directed, and of unpleasantness derived from the original stimulus, or awareness of unpleasant cravings, constitutes the emotional state known as *triumph*. This half-way station in appetitive emotion seems to be found peculiarly pleasant by some types of organism, such as the cat. Playing with the prey, before devouring it, and other prolongations of the triumph-aspect of appetitive emotion, in fact, characterize the behavior of many species, including man.

The final, or passive phase of the appetitive response begins with the eating and swallowing of food. Both the unpleasantness of hunger pangs, and the intensity of the pleasantness derived from perceptions of dominance, now begin to diminish. The hunger pangs completely disappear after a relatively small portion of food has been consumed, leaving, still, some pleasantness of dominance, though this is greatly reduced in intensity. A new pleasantness makes itself felt, now, apparently, from the smell, sight, and taste of the food being eaten. Elation seems to be the phase of appetitive emotion differentiating this final or passive aspect of successful appetitive response from the preliminary, or active aspect. Elation is composed of the pleasantness flowing from a perception of the abolition of the unpleasant stimulus to appetitive response, or escape from unpleasantness, *plus* the positive pleasantness accompanying the food sensations. The fusing of triumph and elation constitute, I suggest, the consummation of the distinctive quality of appetitive emotion, which, as a unit, may be defined as an awareness of successful, willed self-enlargement, with the pleasantness

flowing from such awareness and from the self-enlargement *per se*. Since nomenclature of the conscious correlates of emotional responses is based upon introspective terminology, it seems to me of little importance what names are chosen, so long as the distinctive elements of each emotional aspect are definitely designated, and the same name is always used as a symbol for the same emotion, or emotional aspect. The names "appetitive emotion," "triumph," and "elation," therefore, as used in this paper, should be understood by reference to the psychophysiological elements herein assigned to each, rather than by reference to the individual introspection of the reader, or of some other writer. Bearing this caution in mind, I may summarize, as follows, the two aspects of appetitive response, and their corresponding conscious emotional correlates, together with the related "anti-" or conflict states of the organism.

Appetitive Response
(Will-setting toward self-enlargement)

	Successful	*Unsuccessful*
	Active	
Perceived relation of organism to appetitive object	Dominance	Subjection
Emotional correlate of perception of such relation	Triumph	Shame
	Passive	
Perceived relation of organism to its previous state	Enlargement	Diminution
Emotional correlate	Appetitive Emotion (Triumph *plus* Elation)	Fear

In comparing the objective efficiency of fear and anger, the point was made that flight from danger was primarily an appetitive response, which was accompanied by fear only if motor impulses from the unit-perception of the unsuccessfulness of the flight gained expression in the bodily behavior of the organism; in which case the flight was hampered, and rendered less efficient. During discussion of fear, the relation of fear to flight was expressed by the statement: We are afraid of the bear because we do not run away fast enough. These points should now be made clear by consideration of the primitive or original course of the appetitive response. If the above analysis is correct, appetite consists, primarily, in a flight, or escape from unpleasant stimulation. The entire response is directed toward the removal of disagreeable sensations indicative of threatened depletion or diminution of the body. In short, whether the behavior necessary to bring about the willed result of self-enlargement is capture of food by fighting, or fleeing with stolen food, the biological, as well as the psychological purpose of the en-

largement is to escape the danger of starvation. After the appetitive response has become conditioned upon various specific articles of food, and other objects, as effective stimuli, the primitive nature of the response as an escape from unpleasantness warning of danger is not so apparent; but once this essential characteristic of natural appetitive behavior is considered, it no longer appears a logical, psychological, or biological necessity to correlate flight with fear.

Finally, brief consideration must be given to the enormous extension of the appetitive response in human behavior. Crows, and a few other exceptional species of lower organisms, collect various articles other than food, and behave toward such objects in a manner that may fairly be said to indicate a conditioning of the appetitive response upon these objects as effective stimuli. Human beings, however, have so far transferred the response from its naturally constituted type of edible stimulus, that food is often neglected, maldigested, or even wholly rejected over considerable periods of time, while the subject expends all his energy upon concerted efforts to enlarge himself by the acquisition of property in, or control over, an innumerable number of objects bearing not the slightest relation to bodily nourishment. In some instances, perhaps, a native defect in the digestive or assimilative mechanisms of the body may bring about a very complete reconditioning of this type, and certainly in those instances the appetitive response toward money, and material objects other than food, would seem to be aggravated in its intensity. J. D. Rockefeller, for instance, has suffered with digestive troubles for a long period; but whether such failure of the digestive mechanism is the cause or result of unusually complete transfer of appetitive response to new categories of stimuli could be determined only by study of the individual case. Literature abounds in association between "the lean and hungry look" ascribed to Cassius by Shakespeare[18], and the seeking and attaining of great wealth; whereas such impecunious individuals as Omar Khayyam are currently described as rotund. While such naturalistic observations can possess only fragmentary value, at best, they serve to suggest the possibility that, with the growth of cerebral activity, the human appetitive response has been conditioned upon inedible objects as effective stimuli to such an appreciable extent that insufficient appetitive emotion remains correlated with the processes of bodily sustenance to induce the formation of behavior-patterns directed toward the procuring and proper digestion of food. Again, the very enrichment of cortical connections, due to extensions of human cerebral activity, very probably tends to build up human appetitive responses directed toward objects other than food. Whatever the cause, the average "civilized" human subject feels triumph and elation only in the course of establishing dominance over, or in completing the assimilation of, money, land, economic wealth, and mental or physical possessions of various types other than those suited to nourish the body. This conditioning of the response, with its correspondingly characteristic emotional states, upon diverse non-digestible objects as effective stimuli, however, in no way impairs or alters the primitive type of the appetitive response-pattern; nor does it change, as far as current literature and present human activities afford bases for judgment, the nature of the archaic emotional states of triumph, elation, appetitive emotion, shame, and fear. It is but another indication of man's reluctance to associate himself with his origins, that the simple appetitive response has never assumed its proper place as the mainspring of human behavior. Certainly it has long been recognized as the central pattern in virtually all types of animal activities unrelated to sex; and it would be strange, indeed, if any considerable gap were to be

found, in this particular more than any other, between animal and human responses. The inherent hunger-mechanisms, the flight and fight types of behavior in attempted escape from the unpleasant stimulus, and the types of will-setting directed always toward self-enlargement, are all identical in human and lower animal organisms and behavior. Therefore, the conclusion that these factors are characteristic of an identical appetitive type of response does not seem difficult, or far-fetched.

Sex-Response

The classical description of the innervations of internal and external genital organs is that published by Langley and Anderson in 1895[19], and subsequent experimentation has shown that this exposition is sound. In both male and female, external genital organs are innervated, mainly, through the *nervi erigentes*, while the internal genitals are innervated through sympathetic rather than sacral fibers. Erection is brought about, therefore, through the sacral division of the autonomic, and ejaculation through the lumbar division of the same nervous system. Although, in both sexes, coitus is correlated with intense affective consciousness, the neurological mechanism is wholly reflex, and the act may be completed after all impulses from higher centers have been cut off by section of the cord in the dorsal region[20]. Steinach[21] has shown that sexual puberty is dependent upon the internal secretions furnished by the interstitial cells of Lydig, lying outside the seminal tubules, in the male, and by the interstitial cells of the ovary, in the female. Secondary sex-characteristics, menstrual function, and "heat" all depend upon this internal, or secondary secretion; and are totally abolished if the interstitial cells are removed, although not if the reproductive cells are missing. These results furnish us with the completed picture of an automatic, inherent nervous mechanism, with a chemical stimulus capable of setting it off, designed to initiate sex-response exactly as the hunger-mechanism above considered is inherent, automatic, and capable of initiating the appetitive response.

The action of this sex-mechanism, however, results in a type of behavior exactly the reverse of that following the operation of the appetitive mechanism. The sex-response consists, in brief, of seeking an organism of opposite sex; offering the self to this sex-object as mate; and performing, with the sex-object, an act of coitus, which consists in each party to the transaction stimulating the other in such a way that each ejaculates, or throws off, part of the self. Following our appetitive analysis, above, four psychophysiological units may be discovered in this sex-response: (1) a pleasant internal stimulus, "heat;" (2) a motor or will setting toward depletion of the organism, represented by the image of the sex-object following the forming of association connection between sex-stimulus and an organism of

opposite sex; (3) motor impulses, directed by this will-setting, which result in behavior directed toward finding, and being accepted as mate by, the sex-object; (4) performing an act of coitus with the sex-object, which results in great enhancement of the pleasant internal stimulus, "heat." As with the appetitive response, the adequate or internal stimulus is soon supplanted, or supplemented, by the conditioning of the entire response upon the sight, smell, touch, or other sense perception of the object of the response, once that object has been determined, as effective stimulus. Again, as "heat" tends finally to be eliminated, after the orgasm has consummated the end willed, so the effectiveness of the substituted stimulus, sex-object, is reduced or eliminated at a similar juncture.

While it is, perhaps, of negligible importance whether this sex-response be referred to as an "instinct" or not, I call attention to the fact that no inherited cortical pattern is necessary for the behavior of any organism in this typical sexual pattern. There are, to be sure, inherited nervous connections, afferent and efferent, by which stimulation of mature external genital organs may produce an orgasm, or ejaculation of certain materials from the organism; and there are, also, inherent afferent connections between sensory sex-receptors and the brain, and inherent efferent connections between the brain and the skeletal muscles of legs, arms, and body, and between the brain and the lumbar center controlling the sex-reflexes.[22] But, unlike the case of appetitive response, the sex behavior-pattern is not built up while the organism is in its infancy; and there is still less difficulty, therefore, in supposing that environmental stimulations may be solely responsible for the conditioning and development of the mature sex behavior-pattern, following the initial "heat" set off by a secretory stimulus, from within the body. However this may be, the important point for the purpose of this paper is the fact that both reflexes and cerebral sex-paths never produce any type of behavior other than that directed toward self-depletion.

The affective accompaniment of sex-response, during uniformly successful development toward the end willed, proceeds from a pleasant internal or external stimulus, through an evenly graduated scale of increasing pleasantness, to a final intensity of pleasant affect probably surpassing that accompanying any other response of the organism. The distinctive emotional quality accompanying successful sex-response, throughout, may be designated, with equal accuracy, as sex-emotion or love. If sex-response is interrupted, anger results; and this may vary from a low level of barely noticeable irritation, with very slight unpleasantness, to an opposite extreme of almost wholly thwarted sex, expressed by angry bodily action of great violence, and retrospected, variously, as "slightly" unpleasant and "intensely" unpleasant. Certainly there is some stage or particular sort of anger, with every human subject, that is reported as intensely disagreeable; but the environmental situation under which this most unpleasant anger occurs varies widely with the individual subject. The opinion seems to be unanimous, more-

over, that no type of anger is as unpleasant as is fear of comparatively slight intensity.

Since female organisms contain mechanisms for bodily depletion of three types, menstrual, reproductive, and mammary, not active in the male, and since all of these female depletion-functions are closely associated with, and dependent upon, the activities of the genital organs, it would be natural to suppose that the self-depletion type of response from sex-stimulation would occupy a much greater proportion of the total behavior of females than of males. The result of this more frequent self-depletion behavior, in turn, should be a much more deeply inwrought form or pattern for motor setting directed toward self-depletion, than in the case of male organisms. That this is the case, the relations existing between men and women, from the beginning of history to the present century, amply testify. The mere fact that woman is physically weaker than man, whether this be regarded as a cause or as a result, can never account for voluntary acceptance of a subservient or submissive relationship, unless actual will-settings, or motor responses of a submissive character, had been developed. Such female responses have been built up, I suggest, as a result of the three self-subjecting and self-depleting mechanisms, above mentioned, which form no part in the functional complement of the male organism. No additional type of emotional response, or "instinct," seems to me to be necessary to explain the female's reaction toward her offspring. It is simply a self-depleting type of will-setting, of a true sex-character, which has become conditioned upon the progeny as effective stimulus, mainly through the exercise of the two special female self-depletory mechanisms concerned with the birth and nourishment of offspring.

Male animals, and male human beings, are, I believe, preponderantly appetitive in their total behavior. Lacking the above considered mechanisms for self-depletion which are possessed by the female, and possessing a type of sex-reflex mechanism which leads to much more rapid consummation of the sex-purpose of self-depletion than is the case with the female organism, the proportion of male responses directed toward sex-ends is necessarily minimized. Taking into consideration only the time consumed by the mechanical factors involved in an act of coitus resulting in conception and birth of offspring, it can readily be seen that, whereas the male act of self-depletion is completed in a few minutes, the female response is not completed for nearly two years, including pregnancy, giving birth to the offspring, and nourishing the latter throughout its embryonic as well as its early infantile existence. Throughout this long series of psychophysiological reactions which can, I suggest, be treated as a single, typical, female sex-response, every item of

behavior involved follows the uniform sex-pattern of self-depletion. It is not surprising, therefore, that, in the female organism, the will-setting type of self-depletion is even more deeply inwrought, after the experience of maternity, than is the appetitive response-pattern of self-enlargement. Lacking these female depletion-functions, however, the male organism often possesses such a poorly developed sex-response that the resulting reaction toward the female is a predominantly appetitive or dominating one. Such domination, which is identical whether directed toward mate or food, has probably led, in a literature largely written by men, to the common error of referring to the sex-response as an "appetite," whereas, it seems to me, it might better be called a "feeding." As a matter of fact, once the sex-relation is actually established, the female body is the one built to dominate or surround the male sex organ; and the female body is the one enlarged, at the consummation of the act, at the expense of the male organism. Biologically speaking, therefore, we might well say that the male's sex-behavior is directed toward depleting himself for the benefit of the female, while the female's sex-response is directed toward depleting herself for the benefit of the offspring, or the race. Such a statement holds true, also, of secondary human sex-conduct in so far as the husband furnishes economic support to his wife, and the wife "brings up" the children. If any distinction were to be drawn, therefore, between the degrees of objective dominance actually attained in the sex-relationship between male and female, it would seem to be the female who is the more dominant, despite the appetitive dominance of attitude carried over, by the male, into his sex-response. Psychologically, however, both male and female will to deplete themselves for the benefit of the other, in so far as the relationship constitutes a mutually effective sex-response; the female responds even more strongly toward the offspring; and, in some cases, the male may succeed in conditioning some part of his reponse, also, upon the progeny as effective stimulus.

In human sex-behavior, however, both men and women frequently carry over certain aspects of appetitive response into their sex-relationships; and this phenomenon seems to be traceable directly to the factors of the male's predominance of appetitive development, and the female's biological and economic enlargement by the male, considered above. The male may, if he so elects, assume a dominant attitude toward the female, as he does toward his servants, or other appetitive objects; but he can not avoid depleting his organism if he responds, sexually, to the female as a sex-object. The human female, on the other hand, has found means for dispensing with ultimate self-depletion in carrying out her sex-response with a male, by the simple expedient of preventing conception; and, similarly, she can enlarge herself, economically, at the expense of the male, without any correspondingly completed self-depletion. But, on the other hand, she can not avoid self-subjection, or submission to the male's appetitive dominance, if she succeeds in making herself a sex-object or stimulus to

him; for only by virtue of expressing this initial phase of self-depletion can she differentiate herself from statuary, furniture, or servants as a true sex-object rather than a wholly appetitive one. This frequent mingling of opposite phases of sex and appetitive response in human behavior becomes still clearer in light of an analysis of the two phases of the completed sex-response, which we shall now attempt.

Like appetite, sex-response seems to divide itself, naturally, into passive and active phases, or parts, quite distinct the one from the other. Whereas the active phase of appetite occurs first, however, the order is reversed in the sex-behavior pattern. No real sex-activity can begin until the subject is accepted by the sex-object. The process of seeking, and getting the self accepted by the sex-object may be, and indeed usually is, an exceedingly active one, physically; just as a person after procuring his food may exhibit considerable physical activity in eating it. But during this initial phase of sex-response, as during the final phase of appetitive behavior, the subject remains, primarily, the object acted upon, rather than the moving, or controlling agent. The man who is seeking marriage with a woman must please her in diverse ways; and, in such case, he is submitting his own tastes, time, and activities to the dictates of the sex-object. Similarly, if the situation is reversed, the woman dresses and behaves in the way in which she believes her lover wants her to dress and behave; and her throwing of difficulties in the way of his suit merely constitutes a recognition of his predominantly appetitive male development, which demands continual stimulation by opposition to arouse dominance. In both cases the sex-subject obeys the dictation of the desires and needs of the sex-object; and is, therefore, the thing acted upon, rather than the active agent, just as the appetitive subject becomes the thing acted upon when the food he is eating begins to eliminate his hunger pangs. The type of behavior, then, during this passive phase of sex-response may be characterized as *subjection;* and the accompanying phase of sex-emotion emphasized may be termed *passion.* "There is some evidence," says Woodworth,[23] "of a native submissive, or yielding tendency." I submit that the origin of this tendency, which seems to me very clear in the behavior above analyzed, is the passive, or initial phase of sex-response.

The active phase of sex-response begins with the acceptance of the subject by the sex-object, and ends with the consummation of the sexual self-depletion of the subject. This consummation does not take place, with the female, as above pointed out, until after birth and nourishment of the offspring. The female orgasm, however, although not effecting ultimate self-depletion, yet constitutes, in all probability, an act of preliminary self-depletion, corresponding to male self-depletion simultaneously occurring; and it seems certain that the accompanying female

emotional correlate of this active phase of sex-response more than equals that of the male in both intensity and duration. Although the predominant relation of subject to sex-object during this final phase of sex-response is clearly an active one, yet a certain element of passivity or subjection to stimulation by the sex-object remains throughout, since each participant in the act is at the same time depleting himself, by movements directed by himself, and also is *being depleted*, by stimulation administered in movements directed by the sex-object. While the final, characteristic emotional element predominating during this active phase of the response, therefore, might be called *ecstasy*, the distinctive quality of passion seems to remain almost equally strong, if this element was clearly present during the passive or initial phase. These emotional elements in love, or sex-emotion, and the corresponding elements in anger, are constituted, like those associated with the appetitive response, by an *awareness of the relationship between the subject and the end willed, together with the flood of pleasantness (or unpleasantness) flowing from such awareness.*

Sex Response
(Will-setting toward self-depletion)

	Successful	*Unsuccessful*
	Passive	
Perceived relation of organism to sex-object	Subjection	Subjection less successful than rival's
Emotional correlate of such perception	Passion	Jealousy
	Active	
Perceived activity of organism in relation to sex-object	Depletion	Rival in self's place
Emotional correlate of such perception	Love (Passion *plus* ecstasy)	Anger

With this further consideration of the two phases of sex-response before us, we may analyze more clearly the mingling of appetite and sex found in human behavior. Let us take, as an example, the instance where an appetitively developed man mates with a woman whose appetitive behavior-pattern is at least as deeply inwrought as is her sex-response pattern. The woman begins the drama by furnishing to the man a stimulus of unpleasant deprivation, analogous to hunger pangs. She makes herself an appetitive object, suited to his individual appetitive response, in every way she can devise. She adds to this carefully prepared stimulus an element of inaccessibility, which corresponds to the unpleasantness of the stomach-pangs, driving the man to action in order to escape from this unpleasantness. When the female appetitive stimulus has produced, in the man, a will-setting of self-enlargement directed toward that particular object, he begins to take up his part in the play. The man proceeds to pursue the appetitive object by any means suitable; probably a mixture of propitiation and force, submission and dominance. The woman gradually permits her own capture. The man's response thereupon becomes momen-

tarily one of pure dominance; and the true submissive character of the woman's entire response is now frankly expressed. During this initial or passive phase of the response, therefore, we find the man retaining, throughout, a will-setting of unaltered self-enlargement type, and the woman a will-setting of equally constant self-depletion type. The objective and emotional results gained correspond, respectively, with the will-types expressed; the man has gained an appetitive victory, with accompanying emotion of triumph; the woman has won a sex-victory, having obtained the man as her sex-object; and she experiences, presumably, more or less passion accompanying the process of being pursued as well as during her climactic surrender.

At this juncture, however, the rôles are reversed. The woman has gone as far in her sex-response as she intends to go; from this point on, she adopts an appetitive will-setting, directed toward enlarging herself, physically and economically, at the expense of the man. The man, on the other hand, in pursuing and capturing the woman as his wife, has automatically, by that procedure, placed the woman in the position of his sex-object, no matter how appetitive may have been his attraction for her in the first place. In addition to social, mental, and material relations to the woman, the man himself has steadily created a sex-relation, since he could not buy her, like food, but was obliged to pursue her according to the sex-game, the rules of which require submission to the sex-object. Unwittingly, perhaps, but no less surely, he has built up behavior-habits of subjection to a sex-object, and has conditioned whatever sex-response pattern he may possess upon this woman as effective stimulus. When, upon final consummation of the sex-relationship, this sex-pattern leads to an actual will-setting of self-depletion, a true sex-response is developed which, in so far as it may control the man's conduct, can be utilized by the woman for her enlargement at his expense. Again, each type of will-setting has attained its given end, objective and emotional; the woman gains luxury and ease, and experiences accompanying mild elation, following the removal of the unpleasantness of want; the man, if a sufficient proportion of his activity is devoted to depleting himself to the woman, gains her as his sex-object, and feels an accompanying emotional correlate of love. It is seldom, however, that an appetitively developed male continues to respond to a female after she ceases to behave sexually toward him, since the building up and conditioning of a sex-response pattern which he accomplished, unconsciously, while pursuing the woman from an appetitive motive, is apt to break down almost immediately upon the woman's ceasing to shape her action with reference to his native response-mechanisms.

The foregoing analysis shows, I think, that although sex and appetitive responses may alternate in human relationships between male and female, the two types of will-setting, with their emotional accompaniments, remain, in each case, distinct and opposite in procedure and result. The cat feels no passion for the mouse with which it plays, and no more does the appetitive man feel passion for a woman in proportion as he dominates her; each feels triumph, only, as conscious correlate of willed dominance over captured prey. But the man who succeeds in dominating a woman, may also become submissive to her beauty and charm as a sex-object; and, in proportion as this subjection of the dominant male alternates with or supersedes his dominance toward the woman, he will feel passion. Similarly, the mother animal suckling her young is not led to this act by a motor setting toward the offspring identical with her motor

setting toward her food, but rather by an inwrought behavior-pattern exactly opposite in type and result. The human mother, in caring for and nourishing her child, has no possible purpose of self-enlargement, and, therefore, expresses the purest type of sex-response possible to human subjects.

Sex-emotion, or love, contains no admixture of unpleasantness, as does the entire active phase of appetite. Moreover, sex-response attains, at its consummation, a pleasantness more intense and pervasive than that accompanying any other type of behavior. If, as the present theory suggests, this intense and unalloyed pleasantness of affect is the conscious correlate of voluntarily submissive and self-depleting activities, the practical result is to furnish a simple psychological proof of the Christian maxim: "It is more blessed to give than to receive."

(3) Pleasantness and Unpleasantness

During discussion of the inherent mechanisms of appetite and sex, I offered the suggestion that the general principle of escaping the unpleasant and seeking the pleasant was to be found within these primary mechanisms. Review of the appetitive response, above, discovered that its basis is laid in attempted escape from unpleasantness; and similar consideration of the sex-response has shown, I believe, that its basis is the seeking of the pleasant. Since the initial hunger pangs setting off the first human appetitive response occur during the first few hours of life, the psychophysiological principle of unpleasantness upon which their potency is based must be inherent in the infantile organism. The proof that pleasantness is likewise congenital is not so conclusive. We have no definite means of knowing that the taking of nourishment by an infant organism is accompanied by pleasantness of affect, although we are assured by its resulting change in behavior that the unpleasantness of internal stimulation has ceased. However, some inherent mechanism for unpleasantness, at least, must be discovered, if we are to explain the apparently congenital affect of hunger pangs.

Unpleasantness

From the above analyses of fear and anger, we derive the conclusion that both of these complex emotional states are correlated primarily with conflicts of motor impulses, somewhere within the nervous system, at levels accompanied by consciousness; and we found that both these emotions are strikingly unpleasant. Of the two, fear was found to be more unpleasant than anger, and was similarly found to be correlated with a more fundamental type of motor conflict. These analyses suggest, to begin with, that some relation may be found between conflicts of motor impulses within the nervous system, and the accompanying feeling of unpleasantness.

Affective abnormalities resulting from lesions of the lateral zone of the thalamus have been studied, with striking results, by Head and Holmes.[24] They found that the most important feature, in the 24 cases studied, was an excessive response to affective stimuli. The pleasurable affect of warmth, for instance, was found accentuated, on the injured side, with no lowering in the sensory threshhold; while tickling, and other normally indifferent or mildly unpleasant sensations became very unpleasant. The body was found to react more extensively and more vigorously to affective stimuli in all cases. In short, these investigators have shown that over-reaction is associated with exaggerations of pleasantness and unpleasantness.

My preceding analysis of fear attempted to show that efferent impulses, flowing from sensory stimulation of the organism by a dangerous environment, were competing, within the nervous system, against antagonistic motor impulses directed by a central setting, or will-image, for control of a common efferent path. My analysis of anger postulated a struggle or competition between two groups of motor impulses which might be characterized, in the phraseology of H. E. Hering,[25] as "pseudo-antagonists," or efferent impulses simultaneously utilizing a "final common path" to produce somewhat opposed muscle movements. This pseudo-antagonism of motor impulses, therefore, is much more peripheral than is the true antagonism of fear, and the accompanying unpleasantness is found to be of a much more superficial and sporadic nature. The most fundamental type of unpleasantness, on the other hand, and that of most uniform persistence, would seem to be that state of fear where the competing impulses are of almost equal strength, producing an alternation in control of their common motor path, resulting in trembling of that part of the body or limbs innervated. In the light of this correspondence between the nature and intensity of competition or antagonism between efferent motor impulses having a final common path, and the nature and intensity of the accompanying affective state of unpleasantness, I venture to suggest that the increased unpleasantness of sensory stimulation, found in the results of Head and Holmes, was *caused by* the increased intensity and volume of efferent impulses, due to release from the inhibitory influence of the cortex, which were available to compete against existing motor impulses for control of the common efferent paths. Motor antagonism, in short, seems to be the neurological accompaniment of unpleasantness. In thalamic lesion cases, there was greater volume and intensity of efferent discharge from sensory stimulation, as indicated by increased extensity and vigor of bodily reaction. There was, therefore, greater intensity and duration of motor conflict, wherever antagonistic

impulses were brought into competition for control of the final common path. Moreover, with the removal of cortical coordinations, a larger number of such antagonisms might be expected to occur, since many allied, or pattern groups of impulses would necessarily become disconnected with the removal of their cortical termini. This would tend greatly to increase the proportion of unpleasantness in the total affective accompaniment of sensation, which was, in fact, the precise result reported by Head and Holmes.

If, then, we define unpleasantness as *the conscious correlate of motor antagonism between impulses or groups of impulses seeking outlet over a common efferent path*, we find, according to Sherrington, that such antagonisms may occur between spinal reflexes[26], or between reactions elicitable from the motor cortex[27]. There seems to be no good reason for limiting the definition of unpleasantness to the cortical antagonisms; and, in fact, there is considerable evidence against such a limitation. It was long ago demonstrated that only thalamic connections are necessary to spontaneous movements, and to the "instincts" of appetite and sex[28]; and this work has subsequently been verified and extended[29] to such a degree that an inference of affective accompaniment of thalamic activity is plain. On the other hand, these results also show, as does the work of Head and Holmes above referred to, that the thalami are necessary to the presence of affective accompaniment of sensation, as well as to spontaneous movements in pursuit of food or sex-object. It would seem, therefore, that affective tone is an actual accompaniment of sensation, since the thalamic sensory centers are necessary to the existence of pleasantness and unpleasantness; but, if the above analysis is correct, the blocking or competing of impulses must occur in the neurones of the cord[26], or in still more peripheral neurones of the "final common path"[25]. We have, then, a competition of two or more motor impulses in the cord, with partial or complete blockage of one of these impulses inevitably resulting; and we have an accompanying feeling of unpleasantness associated, consciously, with the sensation accompanying that sensory impulse which wins through to motor expression.

There seems to be good authority for the assumption that any impediment to normal discharge of any definitely elaborated nervous circuit is unpleasant.[30] We find ourselves, however, in the predicament of supposing, if the foregoing theory be adopted, that the competition of impulses partially blocks the motor discharge of the victorious sensory impulse, if the accompanying unpleasantness is to be attributed to any stasis in the nerve centers, or unrelieved summation of stimuli in the successful impulse path. This, we know, need not occur, since the bodily responses to unpleasant stimuli, in the thalamic lesion cases,

were over-reactions, indicating that the successful motor impulses were not in the least reduced, either in volume or intensity, by competition with antagonistic impulses, if such competition had occurred as I have supposed. There remains the possibility that *unpleasantness is correlated with unrelieved, thalamic, summation of sensory impulses, in the sensory circuits of the unsuccessful competing impulses.*

This conclusion has the advantage of removing that element of mystery which remains if we suppose that affective tone is somehow inherent in the sensation which is felt as unpleasant. Almost any sensation may be pleasant or unpleasant, under differing environmental stimulations, and differing motor sets of the organism. Moreover, the same sensation may change its affective tone, abruptly or gradually, with no discernible alternation in sensory stimulation, or in bodily reaction. Again, it is by no means true that unpleasant sensations are less vivid, intense, or enduring than pleasant ones; and if any change in the resistance or nervous energy in the path of the impulses accompanied by unpleasant sensation occurred, correlated with its unpleasantness, it is almost inconceivable that there should be no corresponding change in the inherent qualities of the sensation above mentioned. If, on the other hand, the unpleasantness is correlated with a piling up of unreleased nervous energy in an entirely different sensory circuit, no complication of known sensory mechanisms is necessary for its explanation. The successful sensory impulse blocks its rival. The defeated rival impulse piles up in its thalamic path, without motor outlet. The denial of motor outlet deprives the defeated sensory impulse of any discernible sensory quality, since the intensity of the impulse passes above the thalamic threshold of sensory sensitivity. And the resulting over-intense thalamic activity is accompanied by a vague abnormality of sensory awareness, which we term "unpleasantness."

Applying this suggested mechanism of unpleasantness to our problem of initial appetitive response, no argument need be adduced to prove its inherent character. Any unrelieved summation of sensory stimuli in the infantile thalami will be immediately felt as unpleasantness. Hunger pangs may be supposed to set up sensory impulses of much greater intensity than those resulting from environmental stimulation, since the peripheral sensory end-organs are not yet completely sensitized, and the other portions of such sensory circuits may be supposed to be similarly in a state of incomplete development. Even if the stomach pangs do not produce pain, the resulting sensory impulses, predominating in volume and intensity over other groups of impulses simultaneously obtaining in a normal infant, may be expected to deprive these other impulse-groups of

their proper motor outlets, wherever common efferent paths are involved. Although the extent of such motor competition might well vary considerably, in individual cases, as environmental and internal stimulation must vary with different infant organisms, yet it is difficult to conceive a normal case where insufficient motor conflict would be developed, as a result of hunger pangs, to cause the unpleasantness necessary to initiate the appetitive response.

Given inherent hunger pangs, of inherent unpleasantness, the tracing of the final course of the adult appetitive response becomes a simple matter. A behavior-pattern is gradually formed, as a response to the unpleasant stimulus, directed toward obtaining and eating food, as heretofore outlined. When the food is obtained, its sight, smell, and taste add positive, sensory impulses to the organism's existing complement. These sensory impulses find motor outlet in the cranial autonomic, and the result of this motor discharge is to start gastric digestion, and to abolish the stomach contractions set up by the chemical hunger-stimulus.[14] With the cessation of hunger-pangs, these sensory stimulations caused by the pangs similarly cease, and the supposed motor conflict set up between these sensory impulses and other existing sensory impulses, for common motor outlets, which was found correlated with the initial unpleasantness of the appetitive response, also ceases.

Thereafter we found, in the foregoing discussion of appetitive response, that positive pleasantness began to accompany the food sensations, as the subject continued to eat. If the infant, in the consummation of its first appetitive response, experiences this pleasantness, a congenital basis for pleasant affect must be sought, as well as the inherent mechanism for unpleasantness which I have attempted to suggest. As above pointed out, there seems little positive evidence one way or the other upon this issue; but it may be assumed that an hypothesis for pleasantness which discovers a mechanism capable of operation during the initial appetitive response of the organism will better meet the logical possibilities of the situation than a theory which supposes a later development of nervous response correlated with pleasantness of affect.

Pleasantness

In the foregoing analysis of the appetitive response, we found that positive pleasantness was not felt until after the hunger-pangs had been abolished. It would be futile to suppose that no taste, smell, or visual sensations from the food eaten penetrated the consciousness of the organism previous to this cessation of hunger, especially as we know that the motor impulses flowing from such sensations play an important rôle, through

their vagus efferent channels, in the abolition of the stomach-contractions. Yet it appears equally certain that the ravenous consumption of ordinarily disagreeable and even indigestible materials [31] cannot be accompanied by positive pleasantness, although such eating and swallowing may temporarily suppress hunger. Two important inferences may be drawn from these well established facts: first, that pleasantness cannot be felt, under the circumstances considered, as long as a condition of unpleasantness prevails; and secondly, that pleasantness appears correlated with identically the same sensory stimulations which were felt as indifferent while unpleasantness obtained.

It will be remembered that, in the results of Head and Holmes cited above, greatly increased pleasantness was found accompanying certain warmth-stimulations of medium or minimal intensity. The sensory threshold was not materially lowered. We may assume, therefore, that the over-reactions following such warmth-stimulations are not to be correlated with increased sensory awareness of warmth, as a distinctive quality of sensation, but rather with the affective state of pleasantness which accompanied the stimuli. Since the intensity of stimulation was low, as contrasted with the greater intensities of stimulation correlated with enhanced unpleasantness, considered above, and since the threshold for sensations of warmth is continually readjusted to the "physiological zero" of the body,[32] we may suppose that the principal result of removal of cerebral inhibitory influence by the existence of a thalamic lesion was to permit a greater number of subminimal stimuli to win through to motor discharge, rather than to make possible the delivery to effector organs of a greater total intensity of motor impulse accompanied by actual sensations of warmth. Sherrington[33] has proved that subminimal motor impulses, unable to evoke a given reflex, when conjoined with other subminimal motor impulses from sensory stimulation of another receiving organ, having a common efferent path, suffice to evoke the reflex in question. This mutual reinforcement of certain spinal reflex arcs gives rise to the term "allied reflexes;" and Sherrington finds similar "allied" groups of sensory arcs at the motor cortex[27].

By the "all-or-none" law[34] we know that grading of neural activity by action of the synapse does not occur. We may, therefore, conclude that the same subminimal intensity of such sensory impulses obtains in the thalamic centers as that which is finally discharged into the effector organ, provided the nervous circuit involved passes through the thalamus. In the instance of sensations of taste, smell, and sight of food, as well as in the cases of abnormally pleasant sensations of warmth following thalamic lesion, we know that the sensory paths involved include thalamic units. I suggest, therefore, that *the passing of*

any subminimal sensory impulse to unimpeded motor discharge, over any thalamic nerve path correlated with consciousness, is accompanied by the feeling of pleasantness.

In the case of pleasant food-sensations, above considered, we found that the principal sensory qualities of taste, smell, and vision were probably present during hunger; but we also know that the subtler discriminations of taste and smell are not present in consciousness when an exceedingly hungry man sits down before a hearty meal. Sensory stimulations of low intensity, arising from slight decay of food substances, or from improper cooking and preparation of foods served, fail to register in consciousness while hunger persists; and we may assume that this result is due to the inhibition, or blockage from motor discharge, of such sensory impulses by the antagonistic hunger-pang arcs. Similarly, we find additional pleasant food-sensations present after hunger is eliminated, besides the adding of pleasantness to those major qualities of sensation which accompanied the eating of the first mouthful. The pleasant sensations of which the subject is newly aware arise, altogether, from visual, gustatory, and olfactory stimulations of low intensity, which failed to win through to motor outlet while the strongly opposed efferent impulses from the hunger-pang sensations persisted. These facts suggest that, with the cessation of antagonistic innervation, many sensory impulses which were previously blocked from motor expression pass the efferent threshold, and make their respective qualities felt as true sensations. As eating is continued hereafter, the pleasantness accompanying any food-sensation which can actually be distinguished increases steadily, up to the end of the meal; but the qualities of food-sensations tend, gradually, to become indistinguishable, owing both to fatigue of sensory organs and to other causes. Pleasantness usually continues, however, as long as food is eaten, and even after the subject has left the table a vaguely pleasant affect remains, unaccompanied by definite sensations.

We noted above that pleasantness seemed to be excluded from consciousness by unpleasantness. The mechanism of such exclusion now becomes evident. While blockage of any considerable portion of the efferent path continues, many sensory impulses, both allied and antagonistic as between themselves, and normally of sufficient intensity to pass the motor threshold, are blocked by the more intense efferent group which is antagonistic to all vagus discharge. Simultaneously, then, any existing subminimal food-stimuli must be cut off, by the hunger pangs, from motor outlet. Therefore, if the above suggested definition of pleasantness be accepted, all that free flow of subminimal impulses with which it is correlated is in abeyance, and no pleasantness can occur as long as the major antagonism, or unpleas-

antness, continues. Once the full flow of vagus motor discharge is released, however, by the abolition of hunger-pangs, a continually increasing openness of motor path for allied subminimal impulses may be expected; and that such impulses exist is indicated by the continuation in augmentation of the flow of gastric secretion,[35] after actual food-stimulation of appropriate sensory organs has ceased. This augmented flow of gastric juice continues, in animals, from 15 to 25 min. after the conclusion of an appetizing meal; and Carlson[36] found, in a human subject, that the maximum gastric secretion occurred during the eating of dessert. Such determinations of the quantitative aspect of vagus motor discharge correspond with the accompanying pleasantness of affect, but not with the intensity or extensity of qualitatively distinguished food-sensations. It seems fair to assume, therefore, that both gustatory and olfactory stimulations of insufficient intensity to pass above the thalamic threshold of sensation may, nevertheless, find motor outlet through the aid of their "alliance" with those sensory stimuli giving rise to qualitiatively differentiated food-sensations, as long as these persist; and that, for as long a time thereafter as the chemical stimulation of gustatory and olfactory end-organs continues, such subminimal sensory impulses may find common motor outlet through "alliance" between themselves. During the first period, then, while food-sensations are clearly distinguished, it is these sensations which are felt to be pleasant; but after there remain no individually differentiated sensations to which pleasantness may be attached, those subminimal sensory impulses which are still finding unimpeded motor outlet are merely accompanied by that "diffused unlocalized affective experience of well-being"[37] which is the essence of pleasantness. I would describe this experience of well-being, more specifically, as *a cumulative, harmonious, sensory awareness accompanying allied discharge of sensory impulses of insufficient intensity to pass the lower thalamic threshold of sensation.*

The pleasantness which we found as a constantly enhanced accompaniment of successful sex-response, in the foregoing analysis, may similarly be attributed to a cumulative volume of subminimal sexual sensory stimuli, having final common paths of sacral and lumbar motor outlet, first toward the external genitals, and finally, at the consummation of the response, to those portions of the internal genital mechanism concerned with ejaculation. Since the sex-response begins with pleasantness, we must seek some source of subminimal sensory impulses in the initial internal stimulation. Such is exactly the type of sensory impulses which the initial internal sex-stimulus might be expected to produce, since the distribution of the action of this hormone seems to be very diffuse, and the stimulation at no time

distinctly localized. No single sex-sensation can be differentiated, during this period of internal sex-stimulation, as far as can be judged by the introspection of human subjects; but there is apparent in the behavior of both human beings and animals an increase of general bodily activity, with a distinct tendency toward typical sex-movements. This result would be precisely that expected to ensue upon mutual reinforcement of subminimal sex-stimuli sufficient to obtain motor outlet; and would, according to the theory above suggested, be accompanied by pleasantness.

As the sex-response proceeds, its correlated pleasantness increases cumulatively, up to the final consummation of the act. During this period we find a continually repeated stimulation of the same sensory end-organs, with a perfectly identical type of motor discharge, over common paths, from all the sense organs stimulated. While the psychical sex-elements and the pleasantness of affect are piling up, therefore, the sense-organs are undergoing steadily increasing fatigue, which must result in a progressive rise of the sensory thresholds. Distinctive sensation-qualities may be expected to diminish, then, in exact proportion as subminimal sensory impulses increase in volume. The unbroken alliance between great masses of such impulses, over common efferent paths, ensures unimpeded outlet for the entire group. A conscious correlate of cumulative pleasantness would, therefore, result, as long as sufficient intensity of impulses remained to keep open the common motor outlet. It is this affective element, I suggest, rather than increased intensity or number of erotic sensations, which forms the basis for that cumulative aspect of the sex-response upon which it must depend, reflexly, for its consummation.

The involvement of the cortical centers, also, with still further reinforcement of sacral-lumbar discharge, would seem to depend upon the affective volume accompanying the stimulation, rather than upon the intensity of the stimulation, although this must be constantly maintained at a sufficient intensity to secure the common motor outlet. If the intensity of stimulation of individual sense-organs is increased too greatly, the ensuing thalamic impulses must pass over the threshold of qualitative sensory sensitivity, once more, and the subminimal sensory discharge will be correspondingly diminished. If sensation is thus increased at the expense of pleasantness, central psychical reinforcements of any pleasant response are apt to drop out, in proportion. In short, the foregoing theory of pleasantness would indicate that the attempt to increase pleasure by increasing the intensity of sensory stimulation is not only futile, but actually militates against the end sought. This result, I believe, is a very common one in every-day experience.

Finally, pleasantness must turn to indifference (*a*) if the subminimal sensory impulses which are its true correlate cease; (*b*) if the motor discharge of that sensation which makes possible the outlet of the subminimal impulse is transferred to a foreign channel; or (*c*) if stimulation producing such essential sensation is stopped. The first contingency occurs in the instance cited above, where over-intensity of stimulation eliminates a portion of the total subminimal sensory activity. The second change of pleasure to indifference may be exemplified by the cessation of aesthetic response to any object of art, under pressure of material necessity, and the subsequent use of such object to serve a physically active end. The relationship of "indifference" which thereafter exists between the principal group of sensory impulses stimulated by the object, and the subminimal group which had been furnished with a motor outlet before the change of response occurred, is found by Sherrington[38] to exist as a standard type of motor organization, both in the cord and at the motor cortex, wherever two efferent impulses, or groups of impulses, are discharged over wholly separate and unrelated final paths. The third type of shift from pleasantness to indifference is to be found in any instance of ceasing to look at a "beautiful" object. The sensory stimulation which produces the impulses serving to open a path of discharge possessed, in common, by such stimuli and an existing subminimal group is cut off; and the pleasantness simultaneously ceases, though its true sensory neurological correlate was not to be found, at any time, in stimulation by the object thought of as "beautiful" or "pleasant". Thus indifference takes its place, in contradistinction to both pleasantness and unpleasantness, as a term denoting that affect-less state of the organism's awareness obtaining whenever the controlling groups of sensory impulses are able to find simultaneous motor outlet without passing over a final common path.

Affective States

	Positive	*Negative*	*Neutral*
Name of affect	Pleasantness	Unpleasantness	Indifference
Sensory content	Awareness of free, harmonious, sub-sensation	Awareness of blocked, disruptive, super-sensation	Qualitatively differentiated sensations, only
Correlated relationship of motor impulses	Allied in final common path	Antagonistic in final common path	Unrelated, with separate final paths

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AN EXPERIMENTAL STUDY OF THE AFFECTIVE QUALITIES[1]

By JOHN PAUL NAFE

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INTRODUCTION

We undertook this Study on the assumption that the affective qualities, whatever they may be, must be palpable. It is true that the Würzburg School have sought to introduce into experimental psychology a whole group of impalpables; and it is true, more particularly, that many psychologists have been led, by the results of experiment, to recognise impalpable affective qualities. Yet the continually recurring attempt to identify simple feeling with organic sensation shows that there is a strong current of expert opinion running in the opposite direction. The presence of impalpables in a scientific subject-matter is, indeed,— unless one is ready, with Brentano and the Würzburg School, to define science *ad hoc*,—anomalous, if not contradictory. We decided, therefore, to show the courage of our scientific convictions, and to assume that the affective qualities are palpable.

We thus planned a new frontal attack upon the psychology of affection; and the first thing to do, in working out a technique, was to study the methods of our predecessors. It seemed possible that the diversity of result and of opinion in affective psychology might be due, largely, to some single error of procedure, —to some very natural error that had been passed on, in varying

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degree, from investigator to investigator. We therefore read with an eye to this possible error: and we found something which, if not universal, is at least widespread, and which gave us the needed cue to our own procedure. We thought we found that most experimenters upon affection had made their observers *feelers* rather than *observers of feeling*. They had tried, very naturally, to arouse strong feelings; and by this natural endeavour had, nevertheless, defeated their own object.

Let us take an analogy. Everyone will grant that it is impossible at one and the same time to work hard upon an intellectual problem and to observe the existential processes that underlie the attempt at solution; it is impossible to 'do two things at once,' the exercise in applied logic and the exercise in psychological observation. An experimenter in such case must have recourse to interruption and fractionation, and even so must cut his problems with all care to suit the competence of his observers. The same thing holds of emotion: we cannot both be angry with somebody about something and at the same time observe our anger; and this inability is (on our hypothesis) not due to the impalpable character of the affective core; it is simply, again, the inability to 'do two things at once.' But every strong feeling, set up in an experimental context, tends to be emotive or transcendent; the more effective the stimulus, the more likely is it that the observer becomes frightened, or offended, or amused, or captivated. More than that: when once the error of transcendence has crept in, it will influence the observer's general attitude; the less effective stimuli will still arouse mild emotion.

Here, therefore, we seemed to find a very general source of error. It looked, in fact, as if the error of transcendence in affective work might be as harmful as the stimulus-error in psychophysics. We decided accordingly not to make our observers into feelers; not to try to overwhelm them with feeling; and, especially, not to allow them to become timid or anxious about what was to befall them in the experimental hour. We decided to employ simple stimuli of moderate intensity, such as might be expected to set up affective qualities of moderate intensity, which the observer should then observe and describe as equably as he had been accustomed to observe and describe sensory qualities. Feeling must be aroused,— but feeling as existential process, not as referred emotion.

We may anticipate our detailed statement of method and results by saying, at once, that the plan which we adopted has led us to a definite conclusion. The affective qualities, pleasantness and unpleasantness, turn out, under direct observation, to be modes of pressure: *pleasantness is a bright pressure, and unpleasantness is a dull pressure.*

Observers. Our *Os* were Dr. L. B. Hoisington (H), a member of the instructing staff, with about eight years' experience in observing in psychological experiments; Dr. H. G. Bishop (B), also a member of the instructing staff, with about eight years' experience in observing; Dr. Grace K. Adams (A), a graduate student at Cornell University at the time of this work, with three years' experience in observing; Miss Elizabeth Möller (M), Mrs. John P. Nafe (N), Mr. S. Feldman (F), Mr. F. L. Bixby (By), all doing graduate work in Psychology and all with some previous experience in observing; and Miss Mary Brown (Br), an undergraduate student who had taken the first two courses in Psychology at Cornell University and was, during the time of her observations in this experiment, taking a course in Experimental Psychology. Br was without previous experience.

We thought, at one time, that we could arrange our *Os* in a sort of order of rank, by noting the date of their first positive reports; but on going over the reports, after the conclusion of the experiments, we found that the arrangement was not possible. There are hints of what is coming, for most of the *Os*, in their early reports; but even after a positive report had been given, there were 'relapses' in several cases. F, at the end of his third week, gave as complete a single observation as he ever made subsequently; yet, between that and the next positive report, six weeks elapsed. H and Br, on the other hand, gave positive reports from the first and, especially in the case of H, never faltered. Most of the others worked into the problem gradually, and progressed steadily to the end of the series.

An exhibition of the course whereby the *Os* all fell into line would, we think, be both interesting and valuable; but short of showing the whole series there is no way to bring out the facts; and for that, of course, we have not the space.

Stimuli. The stimuli used, and the catch-word which represents them, are as follows.

Visual: (colored strip) a series of colors painted on a strip of paper, B through R to Y, with three intermediates between B and R and three between R and Y; (flicker) a flicker produced by rotating a disc, half W and half Bk, of 9 cm. diam.; (foetus) the foetus of a dog in a bottle of alcohol; (opal) an opal on a background of beeswax; (citrine) a cut citrine, also presented on the beeswax. The uniform time of stimulation was 5 sec., unless the *O* fulfilled instructions earlier, in which case he ceased looking at the stimulus and began his report.

Auditory: (chord) four forks, C, F, A, C′, struck in the order given with a very light blow. The chord was allowed to continue for about 3 sec., and the forks were then damped in the same order; (loose fork) a screw-fork with the screw loosened. The fork was struck rather violently, and produced a rattling, grating, tone. It was sounded for about 1 sec.

Olfactory: (most of these stimuli are solutions or oils, and were presented in bottles) caryophilline, sweet orange, vanillin, rose geranium, bergamot, jasmin, wormwood, ammonium valerianate, mustard, asafoetida, glue, pyridine, dead flowers, fish soap, apiol, tansy, guiacol, juniper, citronella, the juice from a can of shrimps, and some crackling. We instructed the *Os* to breathe normally. The uniform time of exposure was the time of one inhalation.

Gustatory: sweet chocolate, vinegar, salt solution. There was no control over the time of stimulation. Small amounts were used; in the later experiments one drop of vinegar or salt solution from a camel's-hair brush, or a very small piece of the chocolate.

Tactual: (hot steel) a piece of steel, about 1 in. in diam. at the contact surface, heated to about 50° C; (cold steel) the same piece of steel chilled to about 1° C; applied, in either case, to the volar side of the forearm for 4 sec.; (egg) an egg warmed to about 40°C., rotated in the palm of *O*'s hand for 4 sec.; (fur) a small strip of very soft fur, 12 cm. long, set on the

side of the *O*'s neck and drawn slowly and evenly across it; (glass) a smooth bit of glass moved over the tips of the fingers; (sandpaper) a piece of No. 0 sandpaper, reinforced by a piece of wood, moved across the finger tips; (eraser) a felt eraser, well filled with chalk dust, moved across the tips of the fingers; (needle) a needle pushed about 1 mm. into the volar side of the forearm, never violently; (wool) a piece of heavy wool cloth against which the *O* was instructed to grit his teeth; (string) a piece of heavy string with a somewhat frayed end which was moved across the lightly closed lips of the *O*.

Instructions. The first instruction given was as follows: "I shall give you a stimulus which is intended to arouse either a pleasantness or an unpleasantness. You are to direct your attention, as exclusively as possible, to the affective side of the experience. When you judge that the feeling is at its maximum, break off your observation and give as detailed an account of the feeling itself as you can."

It was soon found, from the reports, that at least some of the *O*s, in the fear of some painful experience, could not maintain a receptive attitude toward the experience aroused, but satisfied themselves as to the nature of the stimulus before they began their observation. The effect of such an attitude is discussed in another place. It is enough, here, to say that it was found advisable to change the instruction to read "to arouse either a *moderate* pleasantness or a *moderate* unpleasantness," etc.

The final version of this instruction was given after the series with the first four *O*s had been completed and the second group was beginning. It read as follows:

"I shall give you a stimulus which is intended to arouse a moderately pleasant or a moderately unpleasant sensory experience. You are to direct your attention as exclusively as possible to the affective side of the experience. When you judge that the feeling is at its maximum, break off your observation and describe the feeling itself as accurately as you can."

The second instruction was as follows: "Continue the effort to get affection palpable under observation for itself. Look for concomitant organic sensations with a view to determining whether affection is simply a meaning laid over these organics."

We also used a few special instructions, which are given with the report of the particular observations.

Procedure. The work here reported was done between October, 1922 and February, 1924. There were, at first, 4 *O*s, H, B, A and F, who worked until June, 1923, with the exception of A who was forced to end her observations in April, 1923. F worked also throughout the summer session of 1923. M, N and By worked from September, 1923, until February, 1924. Br observed for only 3 hrs., all within one week, in November, 1923.[2] The others gave 3 hrs. a week during their series. They began with 8 to 12 stimuli per hour; but, as they increased the length of their reports, the number of stimuli used necessarily decreased. The number varied with the

[2]The casual introduction of this *O* calls for explanation. In a discussion with *E* of the results so far obtained in the present enquiry, Dr. Titchener remarked that he had a vague recollection that mention had occasionally been made of pressure in old Laboratory Reports of affective experiments. It seemed worth while to see if an undergraduate student of good observational promise would give results comparable with those of trained *O*s. We therefore had recourse to Br, who knew nothing whatever of the experiment beforehand, and who simply received the instructions given to the regular *O*s. Our choice, no doubt, was singularly fortunate; Br's reports show, nevertheless, that affective observation under our conditions is not specially difficult.

different *Os*. H never took more than 5 and, indeed, almost always 3 or 4; while F usually took 8 or 9.

At the beginning of the experiment we planned to present three of the stimuli which we expected to be U to one that should be P; but we soon abandoned this order in favor of a haphazard presentation. We found no way to determine, in advance, whether a given stimulus would arouse a P or an U experience. Usually, ammonium valerianate will bring a report of U; but at times the report will be P. Usually, jasmin will be reported P, but sometimes U, or, maybe, P and later U. One *O* may find wormwood P whenever presented, while another will always find it U, and yet another may find it to be indifferent. There is a further difficulty with tickles, itches and light pressures. In the case of the string, for example, the cutaneous quality aroused definitely resembled for some *Os* the quality of P; and this state of things might be confusing, especially if they found the cutaneous experience U. Eventually, the haphazard order was sometimes modified at the discretion of *E; e.g.*, after a run of U, a stimulus would be introduced which from previous reports might be expected to be P.

The Mode of Occurrence of Sense-Feelings

H, B, A and F gave fairly detailed accounts of the manner in which these experiences occur. N, M and By gave a few, not very detailed, accounts; and we therefore wrote for these *Os* a special instruction, as follows: "Given the whole experience, can you attend to the affective part exclusively to the neglect of the sensory part altogether?" We soon found, however, that the new instruction did not bring out as complete a report as we desired; and we accordingly reworded it, as follows: "You are to state with absolute precision what happens when you try to attend exclusively to the affection. Does it come so loose that there is no reference to the sensation, or is it tied?" N received the first instruction at her 16th sitting, M at her 15th, and By at his 27th; N received the second at her 19th, M at her 19th, and By at his 28th sitting. Br made no report on this point, and left the experiment before we gave the special instructions.

The *Os* agree that the affective experience comes as a unit, and that only when the pressure described as bright, etc., is present can such an experience be described as P, and only when the pressure described as dull, etc., is present can an experience be characterized as U. Experience may be neither P nor U; but, if it is either, the experience, as a whole, is different. The pressures add themselves to the sensory core and, observationally, the primary experience becomes more vague, less sharply defined, less clear.

With every quoted report we give, in parenthesis, the name of the stimulus, the number of the sitting, and the affective characterization.

Observer H

(Needle 3 U) What it seemed like was a sort of blurring of the sensory experience, a falling off of the sharpness. It may have been just a

matter of attention; as if a thin curtain hung in front of it that blurred it and deadened it slightly. (Chocolate 3 P) I wasn't able to catch the affection at any moment of appearance; I always came upon it there. I believe the experience as a total is different when the affection is present and when it is not. (Wool 4 U) There is something there, given in experience, when the affective state comes in that is more than the bare sensory stuff. (Salt 5 U) It seemed there that I could almost catch the U coming in, almost observe the change in experience. Experience takes on a more or less perceptual character; but it loosens up, loses a good deal of its sharpness; partly in extensity; or in a shift from quality to quality the sharp corners are rounded, the contrasting intensities and qualities are fused a bit more. The whole thing is a little more volumic, or the rarefying gives an impression of greater volume. Toward the end the affection had gone. It seems then that the experience is cleaner cut, more compact, more sharply defined, though the taste quality, as quality, isn't very dense in and for itself. (Cold steel 5 P and U) That experience does change. It livened up and loosened up. It is less compact somehow, less dense, and certainly seems, with P at any rate, to be more volumic, bigger with much less definite boundaries, or less sharply marked differences in the sensory part. With the abrupt shift to U the thing shrinks. (Bergamot 6 P) Watching the experience as a total what I got was a blurring, dimming of the smell, and a general diffuse lot of brightness, a bit like bright pressure. Just a volumic mass of experience, all more or less bound up together including the smell, of ill-defined extent and not very dense. It is as if the smell quality lay a little off to one side of the total volume of experience, as if the smell was not the center of the mass but rather the liveness, brightness, that made up the bulk. Toward the end the whole experience seemed to shrink a bit and was less lively and less P. (Sandpaper 9 U) The affective quality itself forms the core of the experience; or, if not the core, it joins with the sensory quality in making up the core to the experience. For an instant the sensory quality came out quite dominant for attention. The total mass of experience was then different, it lacked something that was there both before and after. (Mustard 9 U) It was as if it were in a pattern of experience alongside the smell quality, though they weren't separated. I'm taking experience as a beaten up pattern; it seems the only way I can get at anything. I've come to the belief that it's largely extent or the volumic part of the experience that holds them together, is the cement. I can't separate them in extent. (Sweet orange 10 P) There was a knowledge that the experience was olfactory in part; not the quality of the odor, but a knowledge that it was an olfactory experience. The odor seemed to be set into a mass of experience which was not odorous in its totality. There seemed to be more in experience than odor, though the two were fused in experience as a total. There is no sharp line between the components. (Vanillin 11 P and U) The experience came slightly U. It was short; and then there was a shift, P and U alternated a couple of times very rapidly; then P to the end. The set was pretty uniformly for the affective side of the experience, but even so I can't say that there was any complete separation. I tried again at the end to hold the same attitude after the stimulus was removed. The P did continue for an appreciable time. There was still a hint of something odorous, though very vague and indefinite. The most illuminating part of the experience was during that rapid alternation of the affective qualities; but even so, I don't seem to get farther. The total impression of that rapid change is very difficult to describe. It gave the general impression of extreme oscillation in the total complex of experience, shifting from quite a voluminous, light, airy complex, in which the peripheral part was dominant, to something deader, duller, less volumic. The odor itself, the olfactory quality and the other dimensions, are more or less beaten up together; I can't draw any sharp line on the extensive side nor intensive that marks off the affective quality

from the other; yet the olfactory experience doesn't fill up the other, it seems localized in a certain region of the complex. This seems sometimes to be rather centrally placed and at other times off to the side. I got a little something like a visual image in the oscillating part, streaks of lighter and darker gray. In the last part of the experience the olfactory part was very weak; there was something like a reflection or a hint of odor, almost as if the P itself was odorous rather than the odor being something there, tangible in experience. (Tansy 11 U and P) The affection came out more clearly as experience, but bound up with the olfactory part. It's curious about the change in the olfactory stuff, how different it comes in its total setting. Certain characteristics of the olfactory experience seem to disappear. The odor quality became generalized. It seemed to take its character from the affective side rather than expressing its own character. (Tansy 12 U) The affective part of the experience seems to be largely one of volume *plus* this sort of laying over, blotting out, mellowing, softening of the odor itself. The affective part seems to maintain itself better than the odor; it drops off or gets weak, but there is a hang-over. The curious thing is that the affection becomes more dominant, and what odor is left belongs to it rather than it to the odor. In common sense it's like a scented affection. (Glass 12 U) When the affection came there was really a change in the total experience, but it still remained a unified complex experience. There is one interesting feature: the affection, somehow, seemed to fuse more intimately with certain aspects of the sensory experience than other aspects. The sensory stuff, while all there together, was as if split apart. The sensory stuff was at all times dominant, coolness and pressure. They stood there side by side more than fused, and the affection fused much more with the pressure than the cool. The cool stood off more or less. I don't know why I always make a spatial reference. It isn't spatial, or it's prespatial. It's the easiest way to describe it. The cold didn't fuse intimately. When the affective part came in there was an increase in the total volume; and although the pressure didn't increase, the increase was a margin put around the other experience, and in the more or less central part of the pattern the affective part did not stand out in any way from the pressure component. It was there for observation only in the fringe that extended around the pressure. (Tickle 14 P?) The great difficulty in trying to describe is separating the sensory and affective stuff. The only thing I can say is that somehow the sensory stuff is more sharply defined, denser, coarser grained, and the affection is finer grained, looser. Also the affective part was pressury at the core, with arms running out that were bright, lively. More an impression of gross motility in the sensory stuff. The affection was more like a background that the sensory stuff played over; and the change was in the background, as if the affection didn't modify the sensory stuff. (Rose geranium 14 P) At no time did the smell become dominant, at no time was it the principal element in the experience. The affection stood out, dominated, seemed the principal item of experience, and the smell somehow belonged to the affection. I could have said, in a general way, the region in the prism to which the smell belonged, but it never became concrete enough for identification. It was loose, diffuse, unnucleated experience,—and there, like the affection. The affection is unnucleated too. Along in the middle there was a tendency for the smell to collect, condense, but it didn't go far. It did shrink in extent and increase in density a little. The total extent of the experience did not vary much. It was fairly volumic and quite loose, rarefied. It had quite a bit of body, shows no grain, practically, and yet there is a something there. I think it is a matter of degree, fine, alive, internally alive, still without any gross interplay or liveness. It is a much refined liveness. I will try another analogy: where the odor spreads over the total extent it was like a colored light, showing up in a grey mist or fog. They interpenetrate a good deal; no sharp boundaries. It is part of the fog but the light changes it. There

was not much change in the density except when the odor changed; there was a greater density in that part. The whole thing in a way is bidimensional and yet volumic: no thickness experienced; whereas the broadness, spread, is given. Certainly it is not comparable to surface. When the smell condensed, it seemed to condense at one point in the total pattern. The whole experience didn't change much. When the affection got more intense it seemed an increased liveliness in the whole thing and a more complete dropping out of the center for smell; it was completely diffuse. The experience had an aspect we should call smell; but it is so beaten up it seems a shame to break it up and call one part affection and the other smell. (Caryophilline 21 P) To begin with the sensory part was specific, concrete and dominant, not objectified. Quite gradually but rather rapidly, the affection came on top. The change from dominant sensory to dominant affective is not step-wise nor instantaneous, but gradual though quick. As soon as the affective part is the important part, the sensory clings to that, and the change is mostly change in the sensation. The affection expands somewhat in extent; quality is just the same; and density, still soft, fluffy, that suggests thickness but I can't say thick; no change here. It changes primarily in attensity, whereas the sensory part seems to fade out, softens up, becomes texturally more like imagery without contour; so that it melts in, is more intimately fused with the affective thing, and yet stays qualitatively different, though beaten up. One can still say the two qualities are there, but the affection becomes the matrix that holds the qualities, whereas in the first part there are two matrices. Then in the first part they are aways given there together, yet distinct. The character of affection is the prominence of the peripheral part rather than the central part. That seems to be rather constant.

Observer B

(Jasmin 7 P) I could get something like a solidifying of the total experience, a thickening of the experience diffused through the body. (Mustard 15 P and U) The experience I have is modified a bit. It is almost like a congealing, a solidifying of my experience, total experience of the moment, and that solidifying takes place around the odor or sting. (Glue 15 U) A great lot of stuff accrued to that odor, pressure components, and the whole disposition was a mass-formation that seemed to surge and run a temporal course. It doesn't seem to have any specific quality itself. The significant thing seems to be unity, rather than the particularity of the modes and qualities. I haven't any name for that mass-formation. (Caryophilline 16 P) There is something big and expansive about the experience when I report P. It is an odor up until that rapid flux takes place, and then the odor is relatively unimportant though this filling flux is linked right up with it. The experience would not be judged P until the flux has taken place. (Caryophilline 17 P) The experience as a whole may take a new form. The odor is still a factor, but it is an odor with a great deal of accompaniment. The experience swells, and that swelling is attended by or is the P. When I say P it doesn't point to the experience as when I say cold. I'd say a sort of pseudo-pressure, maybe a pressure blend. P is more like wetness that way. Wetness is that integration of qualities, and P is a name for the integration derived from the odor stimulus. (Glass 18 P) There is a curious unification of all the experiences of the moment when it is P. I don't know if that unification gives a level pattern or not, I think it tends to be level; about as much vividness to the flux as to the sensory stuff. (Vanillin 18 P) The experience expands like dust or like smoke when a gun is fired. It is rapid and energetic as they are, not slow diffusion like cigar smoke through a room. The odor somehow seems to go out to the limits of the expansion too. I don't know that it does, but I get an impression it is beaten up through the whole thing. It was a flowery smell, but it isn't the flowery that spreads. (Vanillin 21 P) I don't think it's possible to

break the experience apart into an odor component, as such, and into a sort of pressure-expansiveness. That massive experience is a good deal like an orange in respect to red and yellow. One of them is an odor, in this case flowery, and the other is something related to pressure. (Glue 24 U) The odor had a very definite part in the accruing mass; I think it's fair to say like a flavoring in the pressure experience. (Vanillin 26 P) The thickening was like the change that takes place when a projection machine has been in focus and is then put out of focus. It is a surface with sharp outlines turning to a film without sharp outlines. So, here, the increment seemed to be more like a transformation of the given than like an addition of something new. If it were possible to tease out a pressure as one can ravel a string out into fibers, that kind of transformation is the kind of thing I had; and it seems to me that that transformation is P or P is the name of that transformation, which is a pressure. (Wool 27 U) The experience, as a whole, is duller. I can't find any single thing about it, though, that seems to be U and I don't think U is a descriptive term for it either, just a name it happens to have. Gives me the parallel again of wetness; wetness isn't descriptive of the pressure-cold that wetness has; quite so here, this flux, with whatever reference it has to the gritty, scraping pressure I have between my teeth, those two things taken together seem to be U. (Vanillin 29 P) There is a good degree of unification of odor and pressure-flux. The pressure seems almost odorous and the odor pressury. That was one of the experiences again where the whole thing seemed unified like a tonal fusion. It may be analyzable, but carries on the face of it a mark of unity, or its natural manifestation points toward unity rather than toward components that are easily analyzable.

Observer F

(Chocolate 7 P) The characterization, P, applies to the experienced complex, the predominant components of which were the quality of sweet and a brightness or lightness reminiscent of a bright pressure. The brightness or lightness character was not so much a part of the experience as an aspect of it. It covered the whole of the experience. All I can say is that the appearance of these characters touched off the report P, and with their disappearance there was nothing to which an affective report could be applied.

Observer A

(Cold steel 7?) Right at first, when I felt the cold, there seemed an affective tone to the whole experience that colored it. There was more there than the actual cold and this extra thing seemed to color the cold. (Caryophilline 7 P) There was this coloring all the way through. When I was left with just the smell it was clear and strong. The P came back, but then I couldn't say anything about it except that it made the experience be more experience. There was more at the beginning and end than there was in the middle; but still, in that middle thing, where there was nothing but the smell, the smell was clearer and stronger, and I could have said more about it than, if I'd been asked, I could at the beginning or end; there was more experience the other two times. (Egg 8 U) It seemed that this U belonged to the total experience; when I went to look for it I had to break up the experience. (Needle 8 U) The whole experience was U-toned; it went over the whole thing. It didn't make the prick more or less, but colored it all. Later it wasn't U at all. The quality was all there, but there wasn't so much experience, not nearly so much. The U was very much larger than the sensory quality. It was bigger, more voluminous. It swallowed the prick up. (Sweet orange 12 P) I talk about the attributes of the affection, but they aren't attributes of the affection so much as of the mass. It makes the mass more or less. What I'm saying is that when there is affection in the experience, the experience itself is a different kind

of an experience from the experience when there is no affection, and that although I can use the same terms to describe the two, nevertheless the two are different although it's still on an attributive level; these things go together, not as a product, but more like a product than a sum. It's not really a product, for there are still two things, not so beaten up that you can't tell them apart; and they don't make a simple, they make a complex; but it's a simple complex. (Mustard 18 U) With the going out of the experimental U the experience tightened up, it was more compact and, although it wasn't localized at all, it seemed to have its limits set more; that is, before the change came the total experience, or the part you could call the affection, was without reference to locality or limits; but the odor itself, after the affection went out, though it didn't become localized, seemed as if I could have localized it and I could have said more about how big it was if I had tried. I think these indefinite things are aspects of the affection, and I don't think that they belong to the odor when the affection is there; they are still aspects of the affection, but the odor is the thing you give me, and it's the thing that doesn't go, and so it's the thing that what I'm calling U not only clings to but which it, in a way, envelopes; not like core and context, but as if you had objects in a room and a deep fog settling over them,—and even if you knew they were objects, you can't get them away from the fog; you see them in the fog, but as part of the fog. So when I say these things about large, indefinite, etc., and think I'm talking about the affection, remember the odor is the more definite thing in the experience, the thing you can hang to, and I call the fog the affection.

Observer N

(Caryophilline 6 P) Mild, soft and warm experience, and this soft pressure was the P part of the experience. (Vanillin 6 P) Experience came as a total. (Bergamot 16 P) I'm so used to attending to it as a whole that it is hard to break them apart. I don't know the sensation from the affective part. (Loose fork 16 U) The U formed quickly, but all the time, with that, was the sound too. I don't think they were ever separated; they were together all the time. (Opal 16 U and P) Both the P and U were entirely wrapped up with the sensory experience. (Mustard 19 P) The experience first came as an odor, and then ran its normal course, and the affection came after and it seemed to expand quickly. During that course there was a great deal of movement where the affection and the sensory qualities were bound up or beaten up together, and it's only at the end that they separated and the affection seemed to well up and be enlarged quickly while the sensory seemed more of a flat nature. (Ammonium valerianate 19 U) It came as a total and then loosened up, and the components seemed to stand out and melt out into the affectiveness. The sensory was dominant the first moment, and that always seems to be the way. The affection never precedes the sensory and seldom comes with it. I can recall the sensory stuff and practically repeat the experience. I must recall the sensory, but then the affective is dominant. I can't recall the affection independently of the image. (Loose fork 20 U) Again the affection of this experience was swallowed up. It's drawn in, swallowed up, by the sensory. It comes in after the sensory and goes before the sensory. They are beaten up together, and the U is swallowed in a very determined way.

Observer M

(Mustard 18 U) At first the sensation is dominant, but an affective element is there. Then the affection is dominant, but the sensory part is always there. I don't connect the two, but they are always there. (String 18 P) The tickle was always there at a lower level, even when I was attending to the affection. (Hot steel 19 U and P) I can attend only to U or only to P, but the trouble is there is always sensation there, although I never

think of the sensation as being part of the experience. But when I attend to the affection to see if one affection, only, is there, why then there is sensation right there along with it. (Ammonium valerianate 19 U) It was tied to the sensation in the sense that the sensation has to be there in order for the affection to be there. I don't know whether to say they are beaten up or just lying there side by side; for you can get the affection so clearly and dominantly that you tend to forget the sensation,—though you don't ever. (Asafoetida 20 U) I can isolate it, but it's always there with the sensory. The sensation seems to be the track and the U runs on it.

Observer By

(String 15 U) It's sort of secondary, the sensation is primary. One thinks of the P or U as just coming of itself; but when you try to determine what it is, there is a lot to scrape off. (Vanillin 28 P) The odor experience came first, then the P, and they were attended to for the rest of the experience. The odor experience was there all right, all the way through, but never took the place of the affection as dominant. The two experiences ran along together but the pressure of the affection stood out more.

The Affective Qualities

All of our *O*s, early in their reports, make attempts to describe the qualities of P and U. From first to last the reports are made in terms of touch. The *O* may begin by saying the affective quality is like pressure but isn't one,—yet reports in pressure terms continue. All *O*s eventually describe P and U as 'sensory' pressures. They are "as much alike as if from the same matrix." There are many reports to the effect that there is no qualitative variation, and there is not one report that indicates any such variation.

P, in quality, is a bright pressure or a quality lying between bright pressure and tickle. It is described as bright; sparkling; brilliant; active; like effervescence; like tickle; dancing; shimmering; like points of light pressure; mild; misty; yielding; buoyant; vaporous; diffuse; light; airy; thin; fluffy; ethereal; smooth; soft; oily; welling; spreading; like the pressure component in warmth; like goose flesh, fur and glass; like muscle pressure in quality (not in density or localization), but brighter; like expansiveness of the body.

The quality of U is that of dull pressure or, according to some *O*s, of a pressure between dull pressure and neutral pressure or between dull pressure and strain. It is described as dull; drab; dead; rigid; less lively; somber; inert; stiff; gloomy; more dense; heavy; sinking; leaden; thick; cold; hard; rough; harsh; grating; insistent; condensed; like bodily contraction; like neutral pressure but duller; like dull strain.[3]

[3]It must be remembered that our *O*s were feeling for words; there is no reason at all to think that the various characterisations here listed refer to different modes of experience. Indeed, at the end of the experiment, all *O*s (except A and Br, to whom the question was not put) were ready to accept the designations 'bright pressure' and 'dull pressure.' It must be remembered, also, that all *O*s were familiar with the touch pyramid (E. B

Pleasantness: Observer H

(Chocolate 3) In this case there was a liveness, brightness to it. (Sweet orange 4) P is a bright, lively thing. (Bergamot 6) What I got was a general diffuse lot of brightness, a bit like bright pressure, but I don't want to identify it with that. It was this liveness, brightness, that made up the bulk. (Caryophilline 7) I think what I called the P-aspect of the experience or the aspect of the experience that made it P was that the thing was rather bright, lively. (Caryophilline 9) It is a sort of bright, lively, fluffy, ethereal-like experience. It is more like a pressure stuff than anything else. (Fish soap 10) When it shifted to P the whole mass of experience was brighter, livelier, more like a bright pressure than anything else. (Vanillin 11) There was a rapid alternation of the affective qualities, shifting from a light, airy, complex to something deader, duller, less volumic. (Fur 12) The affection was livelier, brighter, larger. Later a sort of tickle quality came in, bright, lively, but seemed more dense and in streaks. It was difficult to say what was affection and what was tickle. (String 17) A bright, dense experience. On the qualitative side there's a difference. It is hard to put it in words. It is rather bright in quality, and still a different kind of brightness; and the whole mass of the affective part is colored by this peculiar bright thing that makes the integrated mass different. It seems finer grained, it's softer. (Glass 19) I think affection is more like, in quality, some pressure quality than anything else. (Vanillin 27) In quality it's a little like bright pressure, but I can't identify it with any bright pressure I ever had; but it's more like that. It's like bright pressure with the pressure left out. (Sweet orange 28) The experience is very real, positive and just to say 'bright' doesn't mean much. It is almost as if there was a matrix a little like pressure. It is brightish, but there is more, it isn't just sheer brightness. (String 28) There is a sort of conflict between sensory quality and affective quality. The sensory quality is bright, more of the nature of the quality of P, it resembles P much more than it does U. (Glass 31) The quality is like an intermediate, somehow; seems to have a dual reference to something like pressure, though very different from the sensory pressure in that complex, and a slightly bright-like quality. (Sweet orange 32) The quality is pressure-like with a brighter component. (Chord 34) It was bright, I want to say dancing, but that has a connotation of too much violence; shimmering. (Jasmin 34) The affection is a pressury quality. (Fur 35) Not only did the two kinds of experience, sensory and affective, blend intimately, but they were very much alike. There is this difference, the affection is more uniform, in a sense smoother. The affection is bright but not ticklish.

Pleasantness: Observer B

(Asafoetida 2) A general, soft, diffuse pressure. (Ammonium valerianate 3) A very diffuse and weak sort of pressure. (Caryophilline 6) Something like the glow of warmth that I feel in the warm sun. It's deeper than warmth. It is soft like warm, and a good deal like pressure too. (Jasmin 7) It's like expansiveness of the body. (Jasmin 9) The P is like an expansiveness somehow. It is linked up with a bodily state of a pressure sort. (Jasmin 15) That bulky surge isn't particularized very much qualitatively. It's homogeneous somehow, but what it is that's homogeneous isn't apparent. It isn't odor, that's definite. (Caryophilline 16) The massive character is like body pressure. It is bodily, like pressure, and big and spread

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Titchener, this JOURNAL, 31, 1920, 212 ff.), and would naturally look to it for descriptive terms. We cannot, therefore, regard their reports as evidence for the correctness of the Tickle- Bright Pressure-Strain and the Strain-Dull Pressure-Neutral Pressure lines of the figure; we can at most say that the lines, as arranged, served the purpose of the present investigation.

out like warmth, but more like the feel of pressure. (Pyridine 16) It was light like a light pressure, and I think I didn't have P except when that spread was present. (Caryophilline 17) This body reference is a good deal like pressure, very much the same character that the growing pressure of chilly goose-flesh has. The swell is much the same sort of thing in both cases. I'm not ready to say that the P swells; the experience swells and that swelling is attended by, or is, the P. (Vanillin 18) I'd still say pressure, but not any pressure I know. Might say a new form of pressure. It is soft like a warmth or like a touch of cotton wool. (Bergamot 21) There is quite a similarity between goose-flesh and the increment to the P, but there is some difference for goose-flesh is U. (Vanillin 21) I'm not sure if it is the odor that changes by becoming softer and more expanded or if some other experience that is big and soft is there. There is something that hints of an intermediate character, but it is pretty hard to pick the terminals; one of them is odor, in this case flowery, and the other is something related to pressure. (Fur 21) That soft pressure of the fur itself as cutaneous experience, and the slight warmth there is in it, is a lot like the pressure-like thing I've been getting. (Glass 24) I can get the lightest kind of a flux from that thing. It's very light, vaporous. It was P. It was light, lively, bright, and still pressure-like. It feels like pressure. (Caryophilline 26) There was an increment, sort of warmish, soft, pressure kind. (Glass 27) The same bright, easy, sort of flux. There is something lively about the experience. I don't know what that brightness is, I don't seem able to go behind it. Maybe the bright character of the experience could be called the P, and the dull experience I've had before, the U of it. The pressure-like experience, in both cases, is similar in its fundamental quality somehow. One is fluffy, bright, not sparkling quite but active like effervescence, and the other is gloomy, dull, dense like lead. (Asafoetida 28) This expansive, soft, pressure-like experience. (Caryophilline 30) Really overwhelmingly pressury, but the very loose, vapory, kind of pressure. I've tried to relate it to muscle. It's very like muscle pressure, but it's much softer and more diffuse. Taking it from that angle I hardly know why I thought them alike. Only in quality do they resemble each other. (Chord 31) Very soft, diffuse, vaporous kind of experience. It was the usual pressure all right. I'm not able yet to tell if there is any difference between it and the faintest kind of muscle pressure. Can't say what the difference is, unless the muscle pressures are better localized and solider. (Chocolate 32) Like the pressure component in warmth in lots of ways. Localization was the same, and pressure quality was the same.

Pleasantness: Observer F

(Chocolate 7) A brightness or lightness reminiscent of a bright pressure. (Sweet orange 25) Soft like a soft pressure. (Bergamot 30) There was something like points of light pressure, resembling the feeling of a small itchy surface, but lighter and with less body. (Caryophilline 30) Soft and pressury, as when you rest your finger on a heap of flour. (Bergamot 31) There was something that resembled light, lively pressure. The odor resemblance, itself, does not remind one of either P or U, but the texture or pressure component, on the other hand, seems to be identical with the affective feel. (Rose geranium 32) There was a soft, pressury feeling. (Fur 32) Smooth, soft, pressure. (Sweet orange 34) There was a pressure that was both soft and bright. The brightness of the pressure resembled that of tickle. (Rose geranium 34) The pressure was bright and lively like the tickle quality of pressure, and at the same time soft and smooth. (Sweet orange 36) The P of the experience was a soft pressure with a hint of tickle. (Bergamot 40) A very light, soft, pressure, just a little duller than tickle. (41) The difference between P and U observationally is a difference in pressure quality. P is always a soft, light, lively, pressure resembling most

the quality of tickle or bright pressure. The pressure is uniform. (Opal 42) It suggests softness, but there is hardly enough body to it to be called a pressure. (Chocolate 44) A soft, spreading, bright pressure. Then the P-pressure seemed to become lighter and more like tickle, though still a long way from it.

Pleasantness: Observer A

(Rose geranium 14) When the P was there the experience seemed lighter. The soft touches are more like the affections than the smells. This time the P was like lighter air and the U like heavier air. (Fur 16) The P seemed not to be much disconnected from the sensory stuff, it was very much like it. (Fur 17) The P was very weak, but quite a bit like the sensory stuff, more diffuse and not localized. The softness was like the affection. (Vanillin 18) There is something pressury and soft about the odor that is like P itself. (Sweet orange 18) P is always like a soft warmth. (Glass 30) The P was somewhat like this pressure, except that it wasn't so locaized; it was smooth, but the smoothness is from not being rough, not from being in one definite plane as this, and it was much vaguer than this. I think P is smoother than U. (Sweet orange 31) The P seemed to be smooth. I think P is more like U than anything else I can find, and more like the smooth feels than anything else. (String 32) It is a soft pressury thing. (Vanillin 33) It was soft, like the odor but smoother and more oily. (Glass 34) The P is much like the tactual experience, softer, but smooth like the tactual experience. (Fur 35) The P was a lot like the touch this time, soft like the touch, smooth like the touch.

Pleasantness: Observer N

(Sweet orange 8) It seemed to be light, a pressure. There is a brilliance about it in its lightness. (Rose geranium 8) If I know what a bright pressure is, it was a bright pressure. It seemed alive, it grew out. (Rose geranium 10) It's a light pressure. (Jasmin 11) All the time it was active it was P, and all the time it was dead it was U. (Rose geranium 12) Luminous, active and light as if a light pressure. (Opal 13) It's a light pressure; brilliant, decidedly alive. (Chord 14) The P is light, mild, smooth, but a very thin experience. Still it is exceedingly active, active within itself. It's a bright, lively, slight pressure. (Chocolate 14) Not so much bright as light pressure. (14) P is a light, live, bright, thin, pressure. (Chord 15) Mild and soft, and while it lasted I felt live, soft pressure sort of envelope me. There is a brilliance about it, sparkliness. (Vanillin 15) A mild experience, quick, active, bright pressure.

Pleasantness: Observer M

(Fur 11) It was soft but not surfacy. The soft pressure tapered off. (Caryophilline 11) It seemed to be a regular succession of pressures, a band of pressures, in which all the little particles were in movement. I called it a pressure because that is all I could, it's the only way I know to describe it. There was nothing hard as if there were a definite surface, nothing but pressure, but not of anything, on anything. If you can image pure pressure, that was it. It almost seemed soft, yet nothing pressing anywhere to be soft. (Chord 12) It was very light, but still very compact. This is a pressury soft. (Vanillin 12) It was soft, almost a sharp softness. It is difficult to explain; as if it were a surface but yet, if you were to touch it, it would yield rather easily. It's soft as a whole, misty; but in parts of it, particularly in the nucleus, kernel, there is this bright, that is, keen quiet pressure-thing that makes it sharp. It's different from tickle; I'd say tickle was bright and this sharp, but it's only a difference in duration. (Vanillin 13) It came as a soft surface, just in pressure terms. (Jasmin 13) It was soft but continuous. It was light. If you could have a pressure out in the air that would be it, not localized and with no reference to me. Outside of that it would be a pressure. If there could be a pure pressure, that would

be it; that is, pressure, not of anything, on anything or from anything. The fur on the neck gives a pressure too, and that would be described in the same terms as an affective pressure; but you are connected with the pressure on your neck and not with this. (Chord 13) Very light pressure, airy, sort of buoyant instead of heavy and sinking. (Chocolate 14) Soft and vague pressure, misty or cloud-like. It was light, buoyant, as if it could float.

Pleasantness: Observer By

(Salt 22) It is a pressure. (Bergamot 23) A sort of pressure from the inside out, welling, loosely knit experience. It seems to be a pressure, I can't get anything else. They are touches, pressury in quality. (Chord 23) It's that welling, growing, pressure. (Bergamot 24) I feel pretty safe in characterizing it as a bright pressure. It is some kind of a pressure, but it's not tickle, not straight tickle. It's lively like tickle, but a soft pressure. It's liver than neutral pressure. (Fur 24) There came in this bright pressure, soft, loosely knit, filmy; big, bulky sort of thing. (Rose geranium 26) It's a touch quality, would fall somewhere around bright pressure. It's a pressure experience and a very lively experience. It's loosely knit, and gives an impression of soft.

Pleasantness: Observer Br

(Opal 1) The affection is mild. Something like a mild pressure. (Chord 1) It's a mild pressure inside. (Fur 2) It wasn't so much a pressure as tickle, very bright. (Chord 2) It's a bright feeling, very bright. Can't tell you just where it is bright, but all over. It's a bright pressure.

Unpleasantness: Observer H

(Mustard 2) The quality was the same throughout. It is a dull, heavy experience. I don't like heavy so much; dull, dead rather. (Glass 2) U is duller, deader than P. (Hot steel 3) More dense, duller, deader, though it is far from being dense. (Glue 4) Heavy or deadlike, not much alive; it doesn't mount up. (Sandpaper 6) It was a rather dull, pressury quality. (Sandpaper 9) The whole thing is duller, deader, less lively. The quality of that something resembles pressure. (Glue 27) Qualitatively it was much duller than the preceding experience [Vanillin P]. Fundamentally the two things [P and U] are as much alike as if from the same matrix. The former has a brightness touch, and this has a duller, heavier touch. (Crackling 29) A little like pressure, dull, but that's not adequate to the thing. (Vanillin 32) That pressure, something which I've never been able to identify with any particular pressure, slightly dull and heavy. (Wormwood 32) Something given experientially, like pressure, dull, qualitatively characterized by the dull heaviness. (String 34) A dull, drab, sort of thing, of pressure quality. I think the quality was uniform throughout.

Unpleasantness: Observer B

(Rose geranium 20) A big, massive pressure experience. These pressures come in the general setting of this big spread; that is, pressure-like, but not identifiable with any particular pressure I've experienced so far. I've been puzzled for a difference between the spread I call P and that I call U. In U there is a hardness or rigidity; I might say it is more tense. There is a hint of dullness or darkness about it. (Glue 25) This accruing mass of near-pressure seemed to be heavy and dull, rigid; insistent, somehow: sort of leaden, heavy, dead. I want to give the notion of somber character, inertness and hardness, not hard like ice, but hard and tough like lead. Just an impression of that kind of a pressure. I think it was harder, duller, heavier than those for P. They are bright, lively, vaporous, instead of leaden. (Glass 27) The pressure-like experience in both cases is similar in its fundamental quality. It's like two manifestations of a pressure; one is fluffy, bright, not sparkling quite, but active, like effervescence;

and the other is gloomy, dull, dense, like lead as compared with something like snow. (Wormwood 29) I get a tremendous surge of pressure-like experience that is definitely dull and somber and rather dense and heavy; dull seems to be the name for the experience as a whole, or at least for the pressure part of it as a whole. The same kind of difference holds between these U and P as between a dull pressure and a warmth. A warmth sits lightly, while dull pressure is a gripping, ponderous sort of experience; and I think that the P-flux and the U-flux have much the same difference. (Sandpaper 32) Stiff, dull-like pressure.

Unpleasantness: Observer F

(Sweet orange 28) When the hardness and concentrated aspect of the experience became focal it was U. (Ammonium valerianate 28) Something compact and hard, but not small. (Glue 29) The characteristic thing about it was a sort of growing, condensing pressure. (Asafoetida 30) Instead of the softness, something hard, like the feeling one has when a finger is pressed so as to affect the bone. (Glue 32) There was a pressury component in the experience which resembled most the strain I get with a slight wrenching of a finger. (Asafoetida 33) A sort of hard, granular pressure. (Asafoetida 34) There was a hard pressure resembling strain. (Asafoetida 36) There is a compact, hard, pressure, like the quality of strain, which is the U part of the experience. (Glue 39) The pressure here was thin and hard, surfacy. (41) U is always hard, dead, dull pressure, and resembles most the qualities of strain or dull pressure. (Glue 42) There was a dull, harsh pressure. (Needle 43) A condensation, a hardness. (Foetus 44) It was a thin, papery sort of pressure, very much like neutral pressure but duller, with no outline. (Vinegar 44) Pressure, heavier than the U in the previous experience. Nearer dull pressure than neutral pressure.

Unpleasantness: Observer A

(Rose geranium 14) The U is duller than the P. (Eraser 31) Something in the affective experience that was like the sensory experience. I think it had the characteristic of being rough, or of not being smooth; but when the sensory stuff itself is rough, it is hard to tell. (Glue 32) The U was a dull kind of pressury thing and it was vague too, like the P. Dull and cloudy are the only things that differentiate it. (Sandpaper 32) The U was dull pressure, a thickish, cloudy thing.

Unpleasantness: Observer N

(Glue 12) A heavy thickness, impenetrable and more definite form, and a decided pull to it, down, as if it were heavy; and all this means pressure. It was in being confined, harsh, that I knew it was U. (Bergamot 13) It was U, dead, still, thick and cold. U is a solid, heavy pressure, just pure quality. It was still and immovable. (Ammonium valerianate 13) Solid, impenetrable, thick and dark, like a weight, a heavy pressure,—that, altogether, is U. (String 13) I feel the heaviness about it, the decided, deep, dull pressure. (14) U is a heavy, dead, dull pressure. It is small, static, thick and solid.

Unpleasantness: Observer M

(Glass 10) The U-experiences are more compact, occupy less space, wherever that space may be; and they seem darker, blacker; and they are heavier than P is. There is a bright something about the P that U doesn't have. (Glue 11) It was a compact thing, closely pressed together, but not hard as a surface pressure. There was a vague cloudy something all around it. The cloudy part was soft and light pressure, yet not buoyant, for there didn't seem to be any life to it. A dull something like a mist. (Asafoetida 12) It was small, heavy and dull. (Flicker 13) It was a small thing and it seemed hard. There is a buoyancy about the P, a lightness in weight, as if it could float, that isn't in the U. The U wants to sink. (Glue 13) It was only this heavy, dull pressure-something. It seemed misty, out from the

kernel, but this core was very compact, heavy and dull. (Vinegar 14) Heavy and almost like a hard surface. Very small and dull. There is a deadness to it.

Unpleasantness: Observer By

(Salt 22) The experience is of a thin, rather compact, surfacy nature. It seems to be something like strain. Whenever I have to say anything about it qualitatively, I have to put it in the touch-group. (Mustard 23) I'd locate it on the touch pyramid somewhere; seems a sort of dull strain. It's shallow and hard, though the hard may be associative. (Flicker 23) That dull strain, compact, hard and surfacy. (Glue 24) Like a dull pressure but toward strain. This is shallow as compared with the other experience. (Asafoetida 25) A hard, thin, surfacy, experience like dull strain, nearer strain but like dull pressure.

Unpleasantness: Observer Br

(Vinegar 2) A sort of heavy, sinking, grating, feeling. It is compressed, heavy. (Glass 2) A heavy dull feeling, very grating. It's compressed. If there is such a thing as a heavy dull pressure, it is like that. (Sandpaper 2) A sort of a grating kind. I'd call it a very heavy dull pressure. (Mustard 2) I don't know whether you would call it dull or heavy. It was a pressure.

Affective Extensity

Most of our *O*s made their first affective reports on the aspect of extensity. Some described this as "the affective part" of the total experience, and many referred to affection, for some time, as "this volume-thing." The extent reported is bare extensity, not perceptive extent. The extents are never definite; that is, the *O*s report that they are always without boundaries.

P and U are both extended, voluminous; and all *O*s describe P as larger, more extended, more voluminous than U. One affection may be larger or smaller than another, and may itself expand or contract.

The *O*s are agreed that experience in these observations is always increased, extent-wise, upon the appearance of either P or U. They are agreed, also, that P alone is always larger than the accompanying sensory experience; but there is some disagreement whether U alone is larger or smaller. The primary sensory experience always becomes larger with the addition of P or U.

Almost every *O*, in speaking of P, has said that "it fills the whole field of experience." Speaking of U in this connection, they report that there is "no experience there" except the sensory and the U, but that this is "somehow smaller than P."

Many reports state that when P increases, volume also increases; and there is one report to the effect that, the smaller and denser the U-experience, the more U it is.

Observer H

(Salt 1 U) No definite extent. It has extent but not perceptual, no boundaries, but is volumic, not so different from sound. As far as I can say the volume of the thing didn't change. At first I thought it contracted a

little as it got denser, but I believe it was not any less volumic. (Fur 1 U and P) The U was very loose, volumic, without definite limits, rather small; and that seemed to be the affective part The P seemed a little greater in volume, still without limits at all, just a general impression of volume. (Mustard 2 U) It is one of those extents without beginning or end. (Glass 2 P and U) P seems a little more volumic than U for approximately the same intensity. The U seemed rather small in volume. The second P was more volumic than the first. (Hot steel 3 P and U) The P was a bit fluffier than the U, looser and more volumic. (Glue 4 U) The thing is volumic without any definite boundaries. (Eraser 4 U) The affection is an undefinedvolume. (Cold steel 5 P and U) With P the experience certainly seems more volumic, bigger, much less definite. With the shift to U the thing shrinks. (Bergamot 6 P) As if the smell were not the center of the mass but rather the liveness, brightness, that made up the bulk. Toward the end, the whole experience seemed to shrink a bit and was less lively and less P. (Cold steel 7 U) The affection is less defined in extent than the sensory stuff. (Glass 7 U) It is rather volumic. That is what seems to be the difference between the first and the last and the middle part. [Reported U, then neutral, then U again.] In the middle all that stuff was missing. (Chocolate 8 P) That space is certainly a very low order of perceptual space or just dimensional extent, not spread out as a surface; it is voluminous, big, fluffy, indefinite in boundary. (Caryophilline 9 P) The total experience is much greater in extent than the odor quality. The extent of the whole thing increased at the beginning and then ran quite uniformly to the end. (Mustard 9 U) It is largely extent or the volumic part of the experience that holds them [sensation and affection] together, is the cement. (Tansy 12 U) The affective part of the experience seems to be largely one of volume. (Glass 12 U) When the affective part came in there was an increase in the total volume, although the pressure didn't increase. (Fur 12 P) The affection, in extent, went beyond the pressure, it was larger. There was a sort of tickle quality that came in later and that seemed about the same general volume and extent as my affection. (Cold steel 13 U) Something more voluminous than the sensory stuff. (Vanillin 27 P) The P, as such, was very extended, filled the experience, was wholly indefinite in outline; it spreads indefinitely. (Glue 27 U) This experience is less extensive; I couldn't say where it ended, but it came as less extensive than the other [Vanillin 27 P]. (Rose geranium 27 P) The affective part increased in extent somewhat at first and with that was somewhat more dominant in consciousness. It certainly comes as more extensive than the preceding U experience; there is simply no stopping place. It fills the whole of experience. There is something about the experience that makes me say that this gives an impression of less thickness, tridimensionality, than the preceding U experience; not that there is any definite tridimensionality to either, but it is like tridimensional. There is more body to the U. (Sweet orange 28 P) The experience of P doesn't give quite so strong a suggestion of tridimensionality or thickness. It isn't a surface, but somehow comes as thinner. (Fur 28 P) The affection is formless. The P is more extended than the U, but equally formless. (Crackling 29 U) It's quite extended, more than the usual U, but not as much as some of the Ps. (Ammonium valerianate 31 U) The extent seems, in part, to be a function of attensity. When the affective part becomes completely dominant it seems to occupy the whole of experience. (String 34 U) The thing was not definitely extended, though, somehow, less than the whole field of experience. I don't know what the rest of the field is or just what I mean by the field of experience. I seem to have had experiences that were more extended, yet this seemed to fill all that was there. (Opal 34 P) That experience came in practically full-blown, and tended to shrink a little in going out. (Color strip 35 P and U) The U as against the P is less extended, but is as volumic as the P, and sometimes more positively thick.

Observer B

(Needle 4 U) I think of P as large, spacious. (Bergamot 12 P and U) The bigness and the looseness dropped out with the P. (Caryophilline 16 P) There is something big and expansive about the experience when I report P. It is a large odor, but the experience, aside from that, seemed to be big, bulky, dense. (Pyridine 16 P) It struck me that time more as a spread than a thickening, like a thin, vaporous something, rather than a heavy vaporous something. (Caryophilline 17 P) I don't think that the accompaniment is conspicuous quality any more than the odor is. It seems to be an increase in expansiveness, a great expanse, being something that includes a lot, not of qualities, but of conspicuousness. Like a high degree of attensity, like a thing that is vague except for a high degree of attention. (Vanillin 18 P) There was a stage where the experience expanded tremendously. It is the same thing I have called flux. The experience expands like dust or like smoke when fired from a gun. The swell is a change of magnitude and not of quality. (Guiacol 20 P and U) Whatever happened was something built around the somewhat resinous aspect of that odor, or added to it. I think the same kind of thickening or bulkiness I have talked about before. It is like all the experiences pointing toward something; and I think in this case it was a shrinking or a migration toward the center. (Rose geranium 20 U) There was a big, massive pressure experience. Massive is a good word in the sense of spread out indefinitely. It seems to be bigger than my body, and my body is in it. (Vanillin 21 P) The spread is indefinite. There is something expansive without limits. (Bergamot 23 P) That was a good instance of this spread. I got a great mass of experience, it spread all out. (Glue 24 U) Then the experience thickened up, spread out to take in more of a mass of quality of some sort. (Chord 31 P) P is very much like warmth; similar extent, but different qualities. (Chocolate 32 P) It was a big swell. When it came it was typical, but the size was particularly conspicuous. (Sandpaper 32 U) Restricted spread and just like other Us.

Observer F

(Rose geranium 42 P) The affective pressure was heavier than the pressure of the previous observation [Caryophilline P] and also more extended, but without contours. (Chord 42 P) No definite boundaries, but it appeared shaped in so far as it gave the impression of thinness. (Glue 42 U) No boundaries. The experience as a whole gave the impression of surfaciness. (Loose fork 42 U) The sound-experience was bigger in extent and less surfacy than the affective experience. (Fur 43 P) The softness and smoothness was bulky and contourless, and it seemed incongruous with the more or less definitely outlined, superficial, stimulated area. (Needle 43 U) The U was bigger than the sensory pressure-quality. (Chocolate 44 P) The P was bigger than the odor quality. (Vinegar 44 U) There was an U pressure, bigger and heavier than in the previous experience, not as large as but more concentrated than the P pressure. No definite outlines.

Observer A

(Hot steel 5 P and U) The P was greater than the U. (Needle 8 U) The U was very much larger than the sensory quality. It was bigger, more voluminous. (Mustard 9 U) The whole experience, when the U was there, was larger, not more intense or clearer, but larger, more of it. The U had gone before I drew away, and the experience seemed smaller then. This largeness isn't dimensional at all, just a lot of it there. (Vanillin 9 P) The P made the experience bigger and looser. (Citronella 11 P and U) The U was a large sort of a thing, and it was the most prominent thing in the complex, both on account of its intensity and its size. Its size belonged to the whole complex, it and the sensory part together. (Fur 12 P and U) The P made the experience larger in volume, but the U made it concentrate

in. The main effect of P and U is on volume and clearness, and usually makes the experience bigger. (Bergamot 13 P) The P was weaker and more diffuse than the odor. Diffuse is good, I like that word for that. It may have been bigger, but I don't know if the bigness belonged to the whole experience or just to the P. It is in this bigness that they [P and odor] come together. They're bound together so that some attributes can be attended to separately, distinctly for the sensation and distinctly for the affection. (Sandpaper 14 U) The U made the experience more. (String 14 U) The affection was more diffuse than any other part of the experience. (Sandpaper 15 U and P) This P, although it was not little in the sense of occupying little space, was very thin, but still might have had a lot of spread. It was certainly very much larger in bulk than the sensory stuff. (Fur 16 P) The P made it larger and looser, not so closely knit together. The P doesn't seem constricted, and so seems bigger. It is not like what you mean when you say boundless, but it has no hint of restriction or constriction. (Jasmin 21 P) This bigness, vagueness is a positive thing. The odor has more body than the P. (Ammonium valerianate 34 P and U) I think when it got P it got larger. (Wormwood 34 U and P) It got larger when it got P, and I think it got smoother. (Mustard 35 U) This seemed to be more dense and less extended than the Ps, and it seemed more irregular in its extent. (Needle 35 P and U) It seems that when the affection changed from P to U it tightened up, and got more dense and smaller.

Observer N

(Caryophilline 6 P) As the volume came it was P. (Caryophilline 7 P) It was a voluminous experience. (Rose geranium 8 P) The experience lifted, seemed alive; it grew out. (Glue 8 U) There was a thick firmness about the experience. It is a confined experience, whereas the P seems to be free. (Rose geranium 10 P) The first thing I got was volume. It was thick, but a soft thick. It grew in intensity and volume. While there was a lot of volume the experience seemed little, light in weight. (Caryophilline 12 U and P) The U has volume and is restrained, while P is indefinite, absolutely. (String 13 U) The U-experiences are more compact, definite and firm. (Opal 13 U and P) The P came and filled the whole experience. The P is much larger than the U, and there again the size seems to measure the intensity of the P or U. The P isn't confined in any way, it doesn't debar me. (14) P is large, voluminous and diffuse, U is small. (Caryophilline 14 P) It ran the usual course, swelled out, became larger, voluminous and thin. (Loose fork 14 U) More like a flat surface, hard, intense, and of small extent. (String 14 U) The volume seemed to shrink up to a point, small in extent. (Vanillin 15 P) Large volume. Just the same P, nothing different. Some are more intense than others—but that is about all the difference—or larger. I think the opposite is the case with U. The more intense, the smaller in extent.

Observer M

(Asafoetida 2 U) The U itself seemed shorter, more compact, as compared with the P of the former experiment; that is, there wasn't this general, wide-spread feeling. (String 2 P) The P was vague and widespread. (Vanillin 3 P) This sweet and slight pressure had a volume that included all of me in it. (Needle 3 U) It was different from the P in that my feel was limited. (Eraser 3 U) It was a restricted field of consciousness. (Hot steel 6 U) It was condensed, compact, not very big spread. (Asafoetida 7 U) It wasn't a very big experience. It seemed to have volume, but not a volume of anything. (Rose geranium 10 U) It was voluminous and extended back as if a bulk. It wasn't definitely bounded, and yet I wouldn't say indefinitely bounded either. It didn't seem to stop anywhere. I couldn't say here it is and here it is not, it was just foggy and everywhere; and yet it didn't seem very big. (Glass 10 P) P seems to be more vaguely

spread out. I mean, seems to cover a larger area, and to be particularly vague on the outskirts. The Us are more compact, occupy less space, wherever that space may be. The Us are more nearly definitely bounded and are smaller in area than the P. (Asafoetida 12 U) It was small, heavy, dull, and while not absolutely definite in its extent, still it seemed to be there more definitely than the other ones [P ones]. (Jasmin 13 P) There seemed to be a volume to it, and I don't know where I get the volume, for the only thing I get that I can talk about is the soft continuous pressure. It's vague and unlocalized and has no boundaries. Anyhow, there is this volume meaning attached to it. It feels as if there is a lot of stuff behind it, not a surface. It's a deep thing. (Glue 13 U) Not very big, although you couldn't tell any boundaries. (Chord 13 P) It was very vague and big and cloudy. It didn't have any contours, but there were some parts of it more compact than the others. (Vanillin 13 P) It was a big, vague, misty something. It was uniform all the way through, no clouding about it. It again comes as a soft surface, but has this volume attached to it, this deep something. (Chocolate 14 P) There wasn't much there, but it would cover a large extent. There is something odd about the extent, I could never say where it is nor how much, and there isn't any extent outside of the P; but it just seems bigger, especially bigger than U. (Sandpaper 14 U) It was little, very small.

Observer By

(Caryophilline 12 P) It had extent and volume. I could see the edges of it. The edges petered out, but I can't say anything about what it was set in. (Eraser 13 U) The U didn't stop with the sensory part of the experience; it's diffuse. (Vanillin 16 P) It was large, diffuse. (Ammonium valerianate 16 U) Diffuse and voluminous. This is diffuse, but not as voluminous as the P-experience. (Vanillin 17 U) Small in extent, compact. (Rose geranium 17 P) It was a big pressure, big in volume. (Asafoetida 17 U) It was compact and seems to be a surface quality, a flat, hard, experience. The flat and hard were the most noticeable things. (Bergamot 17 P and U) With the P came the welling pressure, with depth, almost tridimensional, bulky rather than surface. (String 18 U) The quality is surfacy, hard and very compact in places, almost form-like. (Wormwood 18 P) It was like warm, not the quality, but the way it wells out, diffuse, large, voluminous, loose, not as compact a thing, seemed to have depth, not the hard shallow quality the other [U] is. (Chord 20 P) Something that was volumic, diffuse, all-over, a swelling out, big and loosely knit. (Glue 20 U) A thin, compact, surfacy, strain-like experience, a diffuse experience, it doesn't grow, swell and expand, but comes in at a maximum all at once after the odor. (Chord 24 P) It's big, voluminous, seems to be all, it occupied everything, fills up experience entirely. It grows, yet you don't see it's bigger, it was big in the first place. (Glue 26 U) It's not big and bulky and voluminous, but it's not a sharp point either. It has a spread, a hard, thin, surfacy spread, not a point. It comes in full-grown and doesn't change after it comes.

Observer Br

(Opal 1 P) It has no shape, it's spread out and diffuse like warmth. (Color strip 1 U and P) U, to me, is sharper, more concentrated, not spread out like P. (Glue 1 U) Not mild and spread out like P, but concentrated. (Chord 1 P) All spread out all over. (Loose fork 1 U) It doesn't go so far. (Chord 2 P) Spread out, just as before. It takes in my whole being. Its spreadoutness is like volume in all the Ps. (Needle 2 U) Not as spread out as in the sound. (Loose fork 2 U) More spread out than some Us. (Chord 3 P) The affection is more spread out than the sensory experience.

Affective Intensity (Density)

We are not at all sure what variable or variables our *O*s reported under the name of intensity. For H, intensity of U is density, intensity of P is volume and brightness,—at least in some of his reports. The same thing is probably true for some of the other *O*s also; but there are many references to intensity that do not fit easily into such an interpretation. Such reports as 'moderately P,' 'intense,' 'fairly intense,' which occur all the way through the reports, are difficult to interpret, unless we are ready to take them at their face value. Usually, but not always, the characterizations 'intense' and 'rather intense' were given for U and 'not very intense' and 'moderately intense' were given for P.

In general, the P and U of our experiments are mild experiences, but may be relatively strong or weak.

The most direct reports are those of H, who says that at weak intensities P and U are very much alike, and that P remains affective quality at fairly high intensities, whereas U becomes perceptive, goes over into emotion. B also remarks that at higher degrees of intensity U is like anger.

The patterns for intensity found by the various *O*s do not agree in detail. For H, who reported upon patterns at much greater length than any of the other *O*s, they are, however, definite and constant to a high degree. In U-experiences he finds a greater density at a central core, although he denies that the experience has any definite form; out from the core the density decreases. P-experiences have no such core. The characteristic, prominent or dominant part of the P-experiences lies toward the periphery; though here again H denies any sort of perceptualized form, or any definite extent of any sort.

Observer H

(Wool 1 U) The U itself was weak in intensity. The intensity was not uniform, however; it fluctuated with the direction of attention. If I attended exclusively to the quality of the touch experience, the U became very weak but not zero. I got the most intense affective experience when attention was directed in part to the experience as object or to the stimulus. (Salt 1 U) It simply became more intense, and by that I think I mean more dense. It really seems that the affection, as affection, was more dense, more compact. It contracted a little as it got denser, but I believe it was not any less volumic. (Mustard 2 U) The U tended to increase in intensity up to the time of withdrawal, and by that I think I mean it became more dense as experience. (Sweet orange 28 P) The affection increased in extent and density, at least there was more of it there. There is a more or less that doesn't seem to be simply intensity. The textural aspect is loose, felty; whatever slight variation of density it shows is slightly stringy and lying roughly in the direction that radii would take; only in the U the thing is more haphazard, like felt, a mass. (Fur 28 P) The very central part of the pattern for P is somehow different; it is the same quality as the whole pattern, but lacks these shifts in density, so it doesn't seem so lively. (Crackling 29 U) It wasn't so very dense, and not quite uniform in density;

but the distribution of density is in little larger patches than for the P, though that variation in density isn't great; no sharp edges of density. The density grades, there are no sharp boundaries, it is more like rolling hills than steep precipices. It seems to be characteristic that it's a bit denser at the center, more of the stuff, a sort of pseudo-density, but the actual stuff itself isn't denser. It is rather the central part of the pattern that seems to dominate. The boundary is indefinite, but it does tail off more or less from the center, though fading out is more rapid at the periphery, which isn't true of P. It increases away from the center and then tails off. (Bergamot 29 P) It is not very dense, but with characteristic slight shadings of density, in which the differences of density were small and tended again to be a stripe-like pattern as against the U which is the hill-effect, hill and hollow, mass-effect. At times the center of the P-pattern is practically blotted out by the sensory component which seems denser at the center. This isn't always true. I get more of that variation of density out from the center a bit. (Caryophilline 30 P) The brightness of this was quite weak, slight. The experience showed very little of the usual density gradations or differences. It was almost uniform, density-wise, not very dense, very little of what I call liveliness. (Glass 31 P) It seemed to get more extended as it got more intense and probably thicker. There is a characteristic there I don't like to call dense, more of it. It thickened up. The sensation, over against this is more dense, more bulky. (Rose geranium 31 P) The P was more intense than in the last experience. Intensity seems to be two things, greater amount and greater brightness, as if it swung along a series toward the brighter extreme. There was very little of the diffuse pressure stuff there. The brightness of quality seems to go along with a change in density. More dense and more streaked, thread-like bits, though the shift in density is gradual, not sharp, and diffused all over the pattern, especially the peripheral part of the pattern. P will remain just simple P at a fair degree of intensity, while U at a certain degree of intensity shifts over to an emotion. Object-consciousness is more apt to come in. I shouldn't say necessarily, but just seems to do it often. (Ammonium valerianate 31 U) At the very beginning very weak, and then P and U are much alike. The difference is more pronounced as they become more intense. There is always an element of likeness, though, even so. (Jasmin 34 P) The intensity weakens toward the margin, and I think the quality is a little brighter. It seems thinner toward the margin, maybe 'less dense' is better than 'thinner.' 'Thinner' seems to imply that it is perceptualized as a found mass, which it certainly is not.

Observer B

(Vanillin 18 P) The swell is largely intensive; at least it's a change of magnitude and not of quality. (Chord 30 P) The P is a little more firm, more developed somehow, more of it, or more intense perhaps. But the increase in intensity is more like a chroma change than ordinary intensity; it seems to thicken up, it was richer more than just more. It may be intensity. (Loose fork 30 U) It was really a pretty intense experience as these things go. It was very U. It was a bit like anger, though I don't know just how.

Observer F

(41) Intensively the affective pressure is uniform, both P and U. (Loose fork 42 U) There was a difference in intensity or insistency.

Observer A

(Tickle 15 U) It is hard to compare the intensities, but easy to compare the extents. (Sandpaper 15 U and P) This P was larger than the sensory stuff, but not as intense. (String 16 U) I've tried to talk about intensity, but intensity isn't right. What it means is not that the thing is very U

or not very U; but the reason I say 'strong' is that in spite of all these things I seem to be more aware of the U than the sensory stuff. (Mustard 33 U) The thickness of P is like oil, transparent, but the thickness of the U need not be heavy, but cloudy.

Observer N

(Mustard 7 U) It was very intense, very U, but it didn't seem to grow. (Rose geranium 8 P) The intensity gradually faded out. The experience lifted. (Glue 8 U) It was intense, seemed an even experience in intensity throughout. (Jasmin 10 P) It was thick, fairly intense. (Asafoetida 10 U) It was intense, thick. (Rose geranium 10 P) It grew in intensity and volume. (Opal 13 P) Not very intensive, the P, though very large. (14) U is usually more intense than P. (Vanillin 15 P) Just the same P, nothing different. Some are more intense than others—but that is about all the difference—or larger. I think the opposite is the case with U. The more intense, the smaller in extent.

Observer M

(Mustard 4 U) The U was a pressure, thin and fine like a pin; that is, it seemed to be most intense at a point, and from there to 'peter out.' (String 5 ?) It was small but intense. (Caryophilline 5 P) It seemed dense in the center and 'petered out.' It was vague, with a more intense concentration, and then tapering off. (Bergamot 6 P) It was a P with a hard kernel, but clouded off. (Chord 12 P) It had a more dense or concentrated core, and the other softness came out less intensely. (Vanillin 13 P) It was uniform all the way through, no clouding about it. (Sandpaper 14 U) It had a hard core, and this core was surrounded with a mist or something soft.

Observer By

(String 16 U) The experience was more intense, more U than any of the others. (Vanillin 20 P) It grows, not in intensity, unless the intensity of a volume. (Glue 24 U) Seems somehow more intense than the P-experiences. I can't think of P as intensive; it gets bigger, but this seems to have intensity, sometimes greater and sometimes less.

Observer Br

(Caryophilline 1 P) It had more intensity or brightness.

Affective Time-Relations

P and U are always coincidental with sensory or imaginal experience. The affective increment usually follows the primary experience, and never outlasts it.

The experience runs a course which may be intermittently affective and non-affective.

Observer H

(Glass 2 P and U) There is, at the very beginning, and this seems to be general, no affection for a moment. It is not there instantaneously. It was then slightly P, and then weak U, and shifted again to P. (Needle 3 P and U) At first slightly P, then slightly U, but with a gap where there was not an affective experience. The U rose to its maximum very quickly. (Cold steel 5 P and U) There was no affection at the very first, then a short interval of deep P followed immediately by U. There was no neutral interval. (Caryophilline 21 P) Quite gradually, but rather rapidly, the affection came on top. The change from dominant sensory to dominant affective is not step-wise nor instantaneous, but gradual though quick.

Observer B

(Pyridine 16 P) So far I have odor and then the affection after a perceptible interval. (Caryophilline 17 P) The flux and the odor are coinci-

dent in time anyway, if not in space. (Vanillin 18 P) The striking thing is that it grows and wanes rapidly. It has a definite course in time as much as a movement has. It disappears like a dissolving view. (Guiacol 20 P and U) This affective spread certainly is a temporal growth, it has a temporal course too. (Wormwood 29 U) The P-flux is active like goose-flesh running a course. It's true that the U-flux grows too, but it doesn't grow in the same way. It drags in or grinds its way in.

Observer A

(Tickle 13 U) The U lagged a little behind the change in sensory stuff. (Bergamot 13 P) The P didn't come immediately; at first I just got the odor.

Observer N

(Sweet orange 17 P) The very first is purely sensory, then strongly affective. (Loose fork 17 U) The affective and sensory came together there. The affection died out with the sensory. (Glue 20 P and U) At first sensory, then P, then U; and the U was gone before the sensory hang-over was gone.

Observer M

(Glue 20 U) The experience comes first as olfactory and then as U. When the odor is gone, the U is gone. (Ammonium valerianate 20 U) The olfactory experience always comes first; and while you may let the olfactory part drop to a lower level, still it is always there.

Observer By

(String 18 U) Shortly after tickle had been in consciousness there came the dull strain. (23) Both P and U come in after the sensory experience and go out with it.

Observer Br

(Colored strip 3 P) I have an image of it. The affection lasts with the image and goes out with it. (Loose fork 3 U) It came with the sensation and didn't last any longer. (Chord 3 P) The hanging-over lasts as long as the affective experience.

Organic Sensations

Our *O*s found organic sensations practically whenever they looked for them; but they failed to find any such sensations that were constitutive of an affection, or covariable with it, if affection was reported.

Many of the following observations were made under the instruction for organic sensations given above. Some of them were made in the general report of experience.

Observer H

(Caryophilline 7 P and U) There was one brief moment when I know there was some organic stuff there, which may have been present the rest of the time; but this organic stuff was someway detached from the odor. (Rose geranium 22 P) Practically the whole of attention was directed toward pressures in the body, mostly respiration. There was very little there even as odor. Then I turned attention to the odor and it came weakly P, but pretty much a meaningful P. No attempt was made to attend to P. I watched for organic change which I already had a sort of standard for. I can't say I found anything more or additional when the P came in, though I wouldn't be very dogmatic about it. (String 22 P) There was practically no affection at first, I was set for the object as an affective object. At the very first there was just contact. Then it turned over to a P-experience, again in a meaningful sense. There was no organic

reaction at all that I could put my finger on, though I gave myself up to it as far as I know how; but the whole meaning of P, the whole participation of P, seemed locked up with the cutaneous experience on my lips; it seemed to be the whole vehicle of the affective experience. I didn't get a thing. (Needle 25 U) At first just pressure, sensory stuff, localized at a point on the arm, then very quickly it became slightly U. Then I took it as a pricking object, and immediately there was a weak kinaesthesis in the arm that meant withdrawal, but did not relate particularly to the affection. It related to the disagreeable object, but in no intimate way to the affective aspect of it. I noticed also a slight shift in breathing but not any particular sensory thing connected with it, nothing I could put my finger on, though I know there was a change in the general bodily tone or feel in the thorax. That was simultaneous with the shift in attitude. Just slightly later I found some dull pressure, almost draggy, localized in the upper abdominal region that was fairly limited in extent though not definitely bounded; the whole thing was heavy, rather persistent; it also meant something like thwarted escape or withdrawal.

Observer B

(String 19) That hangs on, had to rub my lips. I dislike that thing, but I don't know if it's U. There was a lot of motor reaction, all avoiding. I get that from the tickle. There was a very definite meaning of 'getting away from it.' That was positive in it, but I don't know if there is something added on that I can call U. Here's something I don't like, something disagreeable, but I don't find anything I can call an affective state. (String 22) That's a powerful something. I can't decide whether it was U even. There was a lot of involved muscular reactions. I think if I could get away from that it might become U maybe. I can't find any U as such though. Just a motor reaction. The whole emphasis is on getting away instead of what it feels like. (Wormwood 29 U) It always has or generally has the context of facial pressure, as I feel when I turn up my nose at something. I wouldn't call that the P or U either; it seems to be coexistent with it but not related in any sense.

Observer A

(Mustard 22 U) It was U, but I didn't find any organic sensation, except that between my nose and throat I feel a contraction. It's like a swallow, but too high. I got the affection right away, but not the contraction till the odor became strong. This contraction was getting stronger, and the U had practically disappeared. (Fur 22 P) At first I drew away from it. That was before the P came. Then as the P came I relaxed, so far as I can tell. Drawing away was a stiffness in the shoulder. I might say the P was connected with the relaxation; but that was negative, just a letting up of the strain, and the P was strongest after that was over. (Jasmin 23 P) My throat closed. There was a tendency to swallow and some pressure down in my throat. There is no connection between the P and closing of the throat. (Chocolate 24 P) I didn't have anything organic, and the P was palpable. (Jasmin 26 P) A lot of these odors give me a pressure in my throat, the P and U alike. I don't think it means anything; but if I had to put a meaning on, I'd say U. But U didn't really come in, either as meaning or otherwise.

Observer M

(Glue 21 U) There were no organic sensations that occurred concomitantly with the sensation. There are usually organic sensations if you look for them, but there were no changes in these when the U came in. (Asafoetida 21 U) The only organic sensations were those that were there before the U came in. No change in organic sensations, and no organics related to the U. (Bergamot 21 P) There were no organic sensations connected with the P.

The Affective Qualities as Palpable

The affective qualities are palpable, that is, they stand up under observation. All of our *O*s were able to observe both P and U, but none could attend to either to the total exclusion of the sensory experience. The affective quality may be the dominant part of the total experience, however, and the *O* may know more about it for report than he does about the sensory process.

Observer H

(Wool 1 U) The attempt to direct attention exclusively to the U was not successful. The intensity was not uniform and fluctuated with the direction of attention. If I attended exclusively to the quality of the touch experience, very little U was there, or if I disregarded the pressure experience the U became very weak but not zero. I got the most intense affective experience when attention was directed in part to the experience as object or to the stimulus, and in part to the affective aspect. (Pyridine 1 U) It was an object of smell quite U. Then I attended to the smell for the sake of the smell, and the U diminished. Then I shifted back more to the attitude of the first instant, just taking it as something there; the U increased. At no time did the U reach zero intensity. Attention to the object of stimulation in its U-character seems to be most favorable for affective experience. I don't know if that means I'm attending to the affection, but it surely is the object as an U-object,—not just U and not just object, but attention covers the affective nature of the object. (Salt 1 U) I couldn't attend exclusively to the affection, and don't know if I did primarily; but U forced itself to the fore in attention, a sort of an oscillation. Toward the end the affective part was fairly attended to. (Fur 1U and P)Attention shifted to the sensory aspect, and affection dropped out. Then it came as P. The U at first was pretty much focal, but was an U some*thing*. (Mustard 2 U) These [sensory] qualities are not very clear for attention; it is rather the object of experience that I can't get away from, and the U object is what holds attention; so I can't say U dominates; it is just one factor in the dominating complex. So far as quality in the experience is concerned I think the U was the most vivid, most clear. (Glass 2 P) I was unable to hold the affection, as such, exclusively in attention, but it seemed to stand in the dominant complex for attention. (Ammonium valerianate 3 U) I endeavored to get the U focal for attention. It remained U odor throughout. The U of the odor seemed to be as focal as the odor, but the U aside from the odor, in and for itself, was not dominant. (Hot steel 3 P and U) The P seemed to be more or less focal along with sensory experience, but I can't get my attention to take hold of that thing for itself. (Eraser 4 U) I think I can observe the U all right. It is not, of and by itself, focal for attention, but it is more or less for report; that is, it is as if the report attitude was, to a certain degree, independent of attention or clearness or vividness. It is as if I can take an attitude for observation and consequently for report that includes only a part of the total focal complex, and by focal I mean focal for attention. (Salt 5 U) Then came the U under the observational attitude, but attention or the vivid or clear experience was the total complex, though I was set to report on the affective aspect. It really seemed, there, that I could catch the U coming in, observe the change in experience. (Cold steel 5 P and U) It is as if I could attend to the affection if I take it along with the other. (Glass 5 U) I didn't succeed in isolating the affective experience in attention, but I attended to the total complex of which it was a part; and so far as I can say the sensory part doesn't dominate the affective part, it is all on a level. (Glass 7 U) The affection was pretty directly under observation, but not isolated in attensity.

Attensively it was as high as the sensory, it was all on a level for attention, but for observation the affective stuff was clearer. I know more about it for report than I do about the sensory stuff. (Caryophilline 9 P) I think that I had the best and most uniform direction of attention for affection that I've had so far. The quality of the odor was not at any time attended to directly but the odor was always there, somehow beaten up with the affective stuff. (Rose geranium 14 P) At no time did the smell become dominant, at no time the principal element in the experience. The affection stood out, dominated, seemed the principal item of experience, and the smell somehow belonged to the affection. (Glue 15 U) The smell as specific didn't come out clearly at any time. The experience was U, and in a general way I knew about the nature of the smell quality, but more after the experience was over than during it. (Fur 15 P) The affective side of the thing was dominant though the sensory stuff was there. There was almost an extensive separation,—not fully; the cutaneous stuff stood off clear to the side of the affective stuff, still not isolated from it.

Observer B

(Jasmin 15 P) I'm not sure that I noticed the odor that time. (Bergamot 16 P) I don't know what I mean when I say slightly P. I don't know if it means there are degrees of P and U or degrees of conspicuousness of P and U. (Caryophilline 16 P) I think the experience is odor till that rapid flux takes place, and then the odor is relatively unimportant, though this filling flux is linked right up with the odor. (Vanillin 19 P?) The spread varies in extent and something like the vividness of it. I guess that's where the more or less comes from. (Vanillin 21 P) Fairly quickly this massive formation of the experience occurred. It is largely odorous. I don't think it possible to break the experience apart into an odor component, as such, and into a sort of pressure expansiveness. (Rose geranium 23 U) The experience will solidify and thicken up. The thickening is definitely odorous, but so much more than odor. The odor is like a flavoring in it. (Bergamot 23 P) It seems to be dependent on the odor, seems to have an olfactory component, not a component that is observably separate from the great mass of experience. (Glue 24 U) I couldn't center on it too much or it changed. As soon as I set myself to observe it, I found myself facing the odor; but so long as I took a casual, relaxed attitude, then the experience thickened up, spread out to take in more of a mass of quality of some sort. The odor had a very definite part in the accruing mass. I think it's fair to say like a flavoring in the pressure experience.

Observer F

(Sweet orange 28 P and U) When the hardness and concentrated aspect of the experience became focal, it was U. (Rose geranium 28 P) There are those differences of lightness, softness, and brightness as against sharpness and hardness, compactness, etc. and they seem to hang on to the experience, to be a closely integrated part of it, and they seem to be sensory, palpable. (Vanillin 29 P) The softness was more focal than the floweriness.

Observer A

(Chocolate 8 P and U) The P was the prominent thing in the experience at first, and it seemed larger than the sensory qualities, but very closely bound up with them. I was attending to the total thing. The last thing that was affective was U, and at that time the clearest thing was the bitter. The U seemed to come along with the bitter. (Mustard 9 U) The U persisted for quite a while even while I was attending, but I can't say much. There was more there when the experience was U, but the U was closely bound up with the odor itself. I tried to get more, the U had gone away. Then the odor was more clean cut, clearer, more pronounced, clearer as vivid and also as standing out in consciousness as over against anything

else. (Vanillin 9 P) The P was different from the odor but very close to it; they belonged together in the total experience. The quality of the odor was much more distinct when the P wasn't there, still it was the quality of the thing that the odor was bound up together with. When the quality of the odor became clear the P was gone and the odor was more definitely an odor. (Citronella 11 P and U) The U was a large sort of a thing, and though not what you could call clear, it overshadowed the whole experience. I'm not willing to call it clear because it wasn't distinct, but so nearly as I could tell as distinct as any other part and it was the most prominent thing in the complex, on account of its intensity and its size. (Rose geranium 11 U) I wouldn't say the U was clear, it really wasn't; but the U, taking the whole experience, was the big, dominant thing. (22) I can observe the sensory stuff alone or the experience as a whole with the P or U, but never the P or U alone without the sensory stuff. (String 23 P) There was palpable P. (Glue 24 U) This was palpable U.

Observer N

(Ammonium valerianate 19 U) It came as a total, and then loosened up and the components seemed to stand out. The sensory was dominant the first moment, and that always seems to be the way. The affection never precedes the sensory and seldom comes with it; but it may remain after the sensory has gone, for I can recall the sensory stuff and practically repeat the experience. I must recall the sensory, but the affection is dominant. I can't recall the affection independently of the image. (Glue 20 U) That was a complex experience and at first sensory, then it became P, and the affection was practically all. It was independent of the sensory except that it seemed attached in some remote way. Neither P nor U is wholly independent of the sensation. (Loose fork 20 U) It comes in after the sensory and goes before the sensory; but the sensory hang-over brings another U. I can recall it, but the sensory hang-over is there. I can't look at the affection and then go back to the sensory and find it gone. (Wormwood 20 P) I came much closer to having the affection independent; but there is something that ties them together. It may be a hang-over from the sensory. The P wells up and seems above and bigger and decidedly the dominant feature, yet there is a union. I tried to see which left first; I just find both gone all of a sudden.

Observer M

(Hot steel 6 U) It seemed to be hiding behind the sensation; it was there, but it took time to find it. (Glue 19 U) The sensation was always there. I wouldn't say that the U was a smell-U. I wouldn't link it up that way; the U hasn't any smell in it; but I'm always smelling, even though I'm looking at the U. (Asafoetida 19 U) No matter how much you attend to the U there is still that sensation there with it. They are coexistent rather than dependent. (Ammonium valerianate 19 U) I don't know whether to say they are beaten up or just lying there side by side; for I can get the affection so clear and dominant that I tend to forget the sensation, though I don't ever really. (Asafoetida 20 U) I can isolate the U, but it's always there with the sensory.

Localization

Many of the *O*s, in their earlier reports, attempt to localize the affection, and often mention a more or less definite region where it is felt. Later, however, they speak of a 'reference' to a part of the body and deny the localization. All the *O*s finally report affection to be unlocalizable, but they may still refer it to their whole body, and the affection may include the body or be projected out from it.

There is a tendency, at first, to localize affection in the region of the stimulus; but H remarks: "I think it was nothing but localization of stimulus." H further reports that when the affection is dominant there is no localization, but when the sensory part of the experience is dominant, there is always or often localization.

Observer H

(Wool 1 U) There was a tendency to localize the affection in the region of the stimulus, but I think it was nothing but localization of stimulus. (Chocolate 3 P) When I have the attitude of just disinterestedly, or more or less casually, but attentively, letting the experience go on, the whole matter of localization becomes indefinite. (Eraser 4 U) I don't know where it is in regard to localization, except that it belongs together with the sensory complex. (Bergamot 6 P) Not localized. (Cold steel 6 P and U) Localized in the arm; or referred to arm and body, rather than localized there. (Cold steel 7 U) It isn't localized so much in the body as with the sensory stuff. It is hard to get at. (Fish soap 10 P and U) U lacks localization. (Juniper 11 P) The experience was not definitely localized. Hardly any reference to localization, except just 'out there' somewhere. (Tansy 11 U and P) It was as if I were standing off over against the thing almost, looking at it as an outsider. (Bergamot 17 P) Then the active attitude dropped out, and the affection became dominant; the localization dropped out. (Mustard 17 U) Then the affection became dominant and there was a loss of localization. It was everywhere. It reached the total possibility of extent, as if I were immersed in it. I don't mean any reference to my body; there was just nothing in experience but that. (Glue 18 U) I never was rid of the olfactory part of the experience; but toward the end it came to belong to the affection, rather than the affection belonging to it; and it lost all sense of localization, just experience, extended, practically filled experience. (Bergamot 20 P) When the affection becomes dominant there is a lack of localization, even though the sensory aspect is there. It becomes given experience. When the sensory aspect is dominant there is always localization. (Jasmin 21 P) The affection is never the object of localization. (Fur 28 P) When the affection is dominant the whole complex is unlocalized. When the sensory part is dominant, it is often localized. If the P and U are weaker, they cling to the sensation. (Flicker 35 U) I don't know where the U was, out there or where; it just was.

Observer B

(Pyridine 6 U) The U isn't localized. (Jasmin 7 P) Diffuse through the body. (Glue 15 U) It has a positive bodily reference. The odor doesn't seem to be a part of my bodily constitution, but this massing is. (Glass 16 P) What little addition there was wasn't localized as the smoothness. The smoothness was localized in my fingers; this other part seemed more nearly bodily than bound up with the fingers or glass, though not exactly, not specifically anywhere in my body. (Caryophilline 16 P) It seems distributed throughout the body. It isn't localized, but it is bodily somehow, and just the opposite of film-color, which is definite enough with respect to a kind of orientation but is definitely not localized in any one place. The odor isn't anywhere and the flux is there with it. (Rose geranium 20 U) I've sometimes said that affection is localized in my body, but I'm not sure that it isn't projected. It seems to be bigger than my body and the body is in it. I seem to take it as including me, rather than as detached from me. (Vanillin 21 P) There is a general spread that includes a good deal more than the nostril, and I'm not sure that that spread is at all in the nostril. I think I may have only referred it to the nostril. It is of very vague localization. (Mustard 25 P) Vague, localization very uncertain. It belongs

to me, somehow, in my body or in contact with my body, nearer my face but not in any one place. (Wormwood 29 U) Vaguely localized. That flux was widely distributed throughout my body, it was all through my body. (Opal 30 P) In so far as the P was localized at all it was around my face. It is as hard to localize as radiant warmth. Warmth seems to come up against the skin, and the P seems to come like a cloud or vapor around me and otherwise isn't localized at all. (Colored strip 30 P) It takes its orientation about my face very much. It isn't localized there, directly, not in the face muscles. It's like warm.

Observer F

(Rose geranium 42 P) Localized in the smell experience. (Chord 42 P) Just localized in the tone. (Glue 42 U) Unlocalized. (Foetus 44 U) Unlocalizable, surely not localized in the stimulus. (Vinegar 44 U) Indefinitely localized.

Observer A

(Hot steel 5 P and U) I can't say where from or where localized; the P didn't belong just to my arm but to my whole body. When it got U the thing belonged more to my arm, not localized in my arm, but belonged there. (Chocolate 6 P) The pressure was not localized anywhere. (Vanillin 9 P) The P itself isn't definitely placed, not in reference to my body or to anything, so it kept the odor from being definitely localized. (Sandpaper 15 U) It's easier to get it out in the tactual experiences, because the sensory stuff is localized and the affection isn't. In the odors neither is localized. (String 16 U) The U is unlocalizable, doesn't belong anywhere, inside or outside. (Caryophilline 17 P) There seemed to be an indefiniteness of localization of the whole experience. When the P was there, there was a vagueness which made a difference in localization. (Sweet orange 17 P) The experience didn't get localized very well, but better without the P. P and U are both unlocalizable. (Glass 20 P) It seems that the localization is something I get immediately from the touch, and it is important to the touch, as touch, but not to the experience as a whole. (Fur 21 P) The touch was localized, the P wasn't. (Fur 26 P) The P wasn't localized, not with the pressure nor away from it either.

Observer N

(Sweet orange 8 P) I couldn't localize it anywhere, it seemed to be all over me. (Jasmin 11 P and U) While P it was part of me, and when U I didn't have anything to do with it. When P it is more like a pressure, but I can't say in *me*. It's decidedly out of me. (Caryophilline 12 P and U) That completely filled me. I feel it but I don't know where. (Bergamot 13 U) It was in me, I don't know where. (String 13 U) It seems I should be able to say where this is localized, but I can't. (Opal 13 P) It isn't in me, it's out of me; but it's around me, somehow. (Caryophilline 14 P) The pressure is outside of me, enveloping me.

Observer M

(Vanillin 4 P) It was very diffuse as if spread over a much larger area than it possibly could have been. (String 5 P) It wasn't localized, except that it seemed to be in front of me where I could look at it. (Bergamot 6 P) The P is all around, but I don't know where, and I can't say where it ends. (Vanillin 6 P) I don't understand about where it is and I can't tell where it is. (Chocolate 8 P) I don't know where it was spread around; it occupied a lot of space but I don't know what space. (Asafoetida 9 U) These things are odd; they seem so definitely there that I feel I should be able to touch them; but the trouble is you're not connected with them, you are not *there* in the sense that they are. (Fur 10 P) It isn't cut off from anything else, it is just there. (Caryophilline 11 P) It almost seemed soft, yet nothing pressing anywhere to be soft. If I could only localize these

things or place them where I could talk about them,—but they are such vague things I can't talk about them as I can about sensations. (Glue 11 U) I think the thing that bothers me is the way I seem to be disassociated from the experience; that is, it seems just as real as a sensory quality, just as existential and just as independent of me, but as a conscious organism I'm not even there. (Jasmin 13 P) It's as senseless to talk about pressures you don't feel anywhere as about a color volume you don't see. If you could have a pressure out in the air, that would be it, not localized and with no reference to me.

Observer By

(Asafoetida 10 U) Not localized anywhere. (Glue 12 U) Unlocalized. The odor was localized. (String 13 U) Not localized. (String 19 U) It seems equally distributed over the body. (Jasmin 19 P) Localized around the nostrils, but not definitely; it was more a reference than a localization. (Fur 20 P) It's diffuse over the whole body; but when I recall it, it has no relation to the body at all, I don't know where it is. (Rose geranium 20 U) If I look at it and try to localize it I can't, but casually it seems inside and growing. (Glue 20 U) It is diffuse over the body, over the surface of the body maybe, I'm not sure. It's not deep, it's not down in, seems to be over the surface.

Observer Br

(Chord 2 P) I can't tell just where it is bright, but just all over. (String 2 P) The affection is just nowhere, you can't tell where it is; but the tickle, you know where it is. (Vinegar 2 U) I don't know where it is. (Chord 3 P) I don't know just where.

Qualitative Variation

We saw above that the *O*s do not, in these experiments, report qualitative variety of P or U.

It may be worth noting that there is not, either, a single report of qualitative variation within the 'expanse' of a given P or U. There are one or two reports of identity of quality.

Observer H

(Fur 28 P) Qualitatively there is no difference between the central and peripheral parts. (String 34 U) The quality was uniform throughout. It was decidedly more dense at its central part.

Observer B

(Fur 23 P) The P isn't any different, but the experience as a whole unified.

Affection as Tied Experience

The reports already quoted show abundantly that our *O*s do not find affection occurring in independence of sensation. Sometimes, it is true, the affective part of the total experience is so completely dominant that the sensory part has to be looked for, as if by a shift of attentional direction; the *O*s seem to be clear, however, that the looking is for the sake of re-identification and that they were all along aware of 'something sensory' in the background. Very occasionally we get reports that might be interpreted as implying an independent occurrence of P or U; but these reports, aside from the fact that they are few in number, are also in every case equivocal. Under our conditions,

then, affection appears as tied experience. At the risk of repetition we quote a few more reports.

Observer H

(Chocolate 3 P) I don't get hold of that affection in an isolated form. (Sweet orange 4 P) I can't get hold of the affective part in isolation through attention. I don't know why. All the way through I think my attitude is set to carry out instructions. (Glue 4 U) The U belonged to the focal complex. (Glass 5 U) I didn't succeed in isolating the affective experience. (Cold steel 7 U) They were practically separate, but both were there. I couldn't rule out the sensory stuff, but there is no doubt that something gets separated out in the experience. (Caryophilline 7 P and U) The U I was able to get hold of more intimately than the P, but the U was never isolated from the total mass. (Egg 8 P) Then I shifted to the attitude of trying to get P isolated. At all times the sensory part of the experience was there, but it wasn't very clear for observation. (Caryophilline 9 P) The odor was aways there, somehow beaten up with the affective stuff. (Fish soap 10 U and P) It had a slight olfactory tinge, but the major portion of the experience was what I call U. (Vanillin 11 P and U) In the last part of the experience the olfactory part was very weak; there was a hint of odor almost as though the P itself was odorous rather than the odor being there, tangible in experience. (Tansy 11 U and P) The affection came out more clearly as experience, but was bound up with the olfactory part. (13) Question:[4] Can you give any outside analogy to the inseparability of the affection and the sensory content as you report this? Answer: Nothing that is at all times exactly like it. There are times when the fusion of warm and pressure or even cold and pressure is much the same. For the most part there is this difference: that in warm and pressure (warm and pressure and cold and pressure are much the same) these are more unitary; I mean that I can, in the case of the affective-sensory fusion, say that part of it is a specific sense quality, such as smell, and part distinctly not that. It is like the fusion of two different kinds or modalities; so I can break them apart; whereas, with warm and pressure it is more of a piece. One can only say a 'warmish pressure' or a 'pressury warm.' They're bound up together. The nearest approach to being bound up is in the smell-affection; but this differs in that one element seems to predominate.

Observer B

(Caryophilline 17 P) The odor is still a factor, but it is an odor with a great deal of accompaniment. (Glass 18 P) There is a curious unification of all the experiences of the moment when it is P. (Vanillin 18 P) The odor seems to go out to the limits of the expansion too. It is beaten up through the whole thing. (Guiacol 20 P) It was something built around the resinous aspect of that odor or added to it.

Observer A

(Egg 8 U) It seemed that this U belonged to the total experience. (Needle 8 U) The whole experience was U-toned; U went over the whole thing. It didn't make the prick more or less, but colored it all. (Mustard 9 U) Closely bound up with the odor. (Fur 19 U) U stands away from the sensory stuff more than P does, not in terms of space, but in that you can come nearer abstracting it. (String 19 U) It was closely connected with the tactual stuff. (Glass 20 P) The affective experience seemed connected with the touch. The touch is there all the time.

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[4]This question, put at the end of an experimental hour, was the only question asked of an *O* by *E* in the course of the investigation.

Observer N

(Vanillin 6 P) The experience came as a total. (Bergamot 16 P) All the way through I had to attend to the floweriness. I'm so used to attending to the experience as a whole that it is hard to break them apart. (Loose fork 16 U) The U formed quickly; but all the time, with it, was the sound. I don't think they were ever separated. (Opal 16 U and P) That was entirely wrapped up with the sensory experience, both the P and the U. The affective side depended on the sensory experience. (Asafoetida 16 U and P) Sensory was mixed all through; it turned to imaginal. (Chord 16 P) The sound is just as prominent as the P. (Glue 20 P and U) Neither the P nor the U is entirely independent of the sensory part of the experience. (Loose fork 20 U) They are beaten up together.

Observer M

(Glue 19 U) The sensory experience is always there. (Asafoetida 19 U) No matter how much you attend to the U, there is still that sensation there with it. (Ammonium valerianate 19 U) It was tied to the sensation in the sense that the sensation has to be there in order for the affection to be there.

Conditions of Affective Arousal

To arouse an observably affective experience the stimulus must be mildly intensive, not so intense as to arouse a perceptive experience, and the *O* must maintain a psychological, nonperceptive attitude. The attitude may be active within these limits.

Experience from every sense department may take on the increment of bright or dull pressure, that is, may become affective experience.

Every *O*, except A, reported both P and U with sensations from every department of sense. A reported them with sensations of smell, taste and touch. As we stated above, A left the university before the completion of her series, and had no opportunity to observe with visual or auditory stimuli.

Observer H

(Salt 2) Somehow the quality set up an expectation for U, and there didn't seem to be any U there. I rather set myself for the U, to try to get it, or to be sure to pounce on it when it came; not to create it, of course, but get it as soon as it came in. I don't know what the effect of that was. There was then a shift of attitude again, a sort of a naive letting-it-run or something like that. Then U came in. (Needle 3) The experience, as such, caught the attention. The affective experience almost dropped out. Then I shifted again to a naive attitude or a less critical attitude, an attitude of letting a thing be what it will without too close scrutiny; then the U came in. (Hot steel 5) As soon as I turned to the experience, and looked at that experience itself, as if saying 'Is this affection?' there was not much there, if anything at all. (Needle 7) So far as affective it was slightly P; but there was a suggestion that it might become U, and this kept me where I couldn't give up to it; so I can't say anything about it. (Vinegar 11) For a long time I didn't fulfill instructions, I couldn't get it. I was so concerned with the stimulus, I found the taste quality so intense, so demanding, that it was difficult to do much with the affection. (Tansy 11) There was more or less meaning, object-consciousness, mixed up in the whole thing. The affection, as such, didn't become clear. (Rose geranium 31) U, at a certain degree of intensity, shifts over to an emotion. Object-consciousness is more apt to come in.

Observer B

(Chocolate 3) That wasn't even P, when I face it as taste. I never enjoyed good candy less. I believe you have to take these things casually to get anything like P. (Jasmin 9) If I'd let myself go to that, I'd get P. I spoiled it by being analytical. (Caryophilline 15) That was pretty indifferent. There was something in my attitude, an interest in the odor, that spoiled that. (Caryophilline 17) Whatever happens when the odor is P depends a lot on attitude. If I'm too critical, it spoils it, gives me odor only. What I mean by critical there is a predetermination to observe sensory quality; and if I'm set for sensory quality I don't get P. If I take some other attitude, let the quality be hanged, then it is possible for the experience to pass into this stage of expansion, surging. The odor is still a factor, but it is an odor with a great deal of accompaniment. (Ammonium valerianate 19) To get the P it seems necessary to relax or let go. (Caryophilline 21) The odor is a bit weak. I had to observe myself get the odor. That was almost enough to kill the tendency for the P to come. Once there was a shift to the usual conditions and that happened, too, just at the time I let the odor run its own course and took a passive attitude toward it. It depends too on the bodily tension, I think. It gets pretty complicated; but it wouldn't come right if it didn't come easily. (Rose geranium 23) I can feel the increment melt away when I set out to get the odor. Then I can relax, and the experience will solidify and thicken right up. (Flicker 30) That came when I had an unspecific attitude toward the stimulus, and it was soon over. Looks as if the *thing* is damaging to the experience. (Colored strip 30) When I began to pick out the R, Y and B that were best in saturation, it began to lose the buoyant soft pressure that was P, and was just colored stripes.

Observer F

(String 27 U) Very difficult to observe; the experience is so intensive. (Flicker 43) I didn't get any affective experience at all. I was too intent on seeing what the stimulus was. There were so many changes of hue and form that I took the experience as an interesting novelty and failed to observe the affective side of it.

Observer A

(Jasmin 5) All these smells I'm afraid of when I first smell them. I question them first. I'm stiff. The sweetness was the first thing, and then I relaxed. (Glue 6) I can't say anything about the first U, because there was just the stimulus. (Vanillin 9) When I tried to give my attention to the P, I found my attention on the quality of the odor. (Juniper 10) I wanted to give my attention to it and had to find it, get it out of the complex. As I did that I was searching for it. What I got was just the quality of the experience, not other things. (Guiacol 12) I don't think it was either P or U. I used to hate that smell, but it doesn't seem U now. I'd say it was an U odor, but I couldn't find anything in the experience that was affection. (Bergamot 17) I couldn't find anything. The pungency localizes the thing at once, so attention gets on quality of the thing and no affection comes. (Needle 25) As far as I can tell there wasn't any palpable affection there. There might have been, but I withdrew from it, and that was all I could attend to. It very decidedly *meant* U. It meant that it hurt and it would hurt more if I didn't get away. If there was any affective stuff there I didn't know it, but there were a lot of organics, jumping, etc. (Crackling 29) Gee! It meant U; I don't know if there was any palpable U there or not.

Observer N

(Rose geranium 11) Nothing. I identified it the first thing, and could not get anything. (Sandpaper 14) There was a decided sensory experience, but I can't say if P or U. (Colored strip 16) I don't get anything there. I made that, and I did a poor job; the imperfections jump out at me.

Observer By

(Wormwood 20) That was neither P nor U. It's strange and new. (Vanillin 18) I can't give any account. I was too much interested in the new way of presenting it. (Flicker 16) I haven't seen that often enough for the novelty to wear off. Every once in a while it would begin to get U, but it wouldn't stay U; my attention kept going back to the stimulus. (Opal 16) I can't do anything, I don't know why. I know it's P, but I'm looking for something.

Observer Br

(Colored strip 1) I get the affection better if I look just a short time and then stop. It comes after I stop looking at it, and it seems, when I look away, that it has no shape; it's just there.

The Incompatibility of P and U

In all of our experimental work there is not a single case of mixed feeling reported. There is only one reference to such a possibility.

Observer A

(Citronella 11 P and U) "At first I thought I had mixed feelings." The report is very long, but there is no other reference in it to mixed feelings. There was an U experience cutting across an otherwise P experience. The *O* reports positively that while it was U it was not P: "The U would cut across just as the P was gone, so I couldn't compare the U and the indifference."

Conclusions

We have tried, in the foregoing pages, to exhibit our data as nearly as possible in the way in which we received them. We have used the terms 'sensory' and 'affective' in the ordinary sense, as they were used by our *O*s; we mean them to be taken, without prejudice, merely as labels. We do not, indeed, venture at this time upon any sort of systematic discussion of our results; the results themselves—which were as unexpected by us as they will probably be strange to the reader—must, we think, first be tested by other experimenters; and many questions of affective psychology which our investigation does not touch must first be asked and answered. We believe that the reports which make up the body of this paper will help materially to stabilise affective doctrine, and so to settle one of the major uncertainties of experimental psychology; but we do not pretend to have done more than break ground. We therefore postpone systematic considerations;[5] and for the same reason we do not at present attempt to review the work of our predecessors, and to bring earlier results into line with our own.[6]

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[5]We remark only that there is a certain plausibility about the reference of P and U to the modality of touch, since other touch qualities (tickle, itch, pain) are connected with gross reflex movements. We do not know, however, that the bright and dull pressures which are P and U are peripherally aroused.

[6]Cf. E. B. Titchener, *Elementary Psychology of Feeling and Attention,* 1908, Lect. iii.

The outcome of our enquiry may be summarised as follows. Every item of the summary must be qualified by the phrase "under the conditions of the present experiment."

(1) Sensory experience may or may not also be affective; it may be characterized as P or U or neither. If sensory experience becomes P or U, it alters as a whole; something is added to it that is not present while it remains indifferent.

(2) Qualitatively, this increment, in the case of a P experience, is bright pressure or a touch-quality lying between bright pressure and tickle; in the case of an U experience, the increment is dull pressure or a quality lying near dull pressure.

(3) The bright pressure is more extended than the dull pressure of the same general intensity, and is usually greater in extensity than the accompanying sensory experience. Both P and U may vary in extensity. P is very voluminous, without limit or restraint. U is voluminous, but less so than P; it is without limit, but is somehow constrained or contracted. Neither P nor U has definite form, limit or boundary.

(4) The affective increment is sometimes beaten up with the rest of the experience, and sometimes there is a quasi-spatial separation, which, however, is never complete.

(5) P and U are both inherently weak, mild; but they may vary in intensity. The *Os* do not, at all times, report changes in the same variable as intensive changes. The 'intensity' of P may increase as the brightness increases, as the volume increases, with the increase of some variable in the nature of chroma, or with the intensity of the stimulus. The 'intensity' of U may increase as the dullness increases, as volume decreases, as density increases, or, again, with increase in the intensity of the stimulus.

(6) There are patterns of density for both P and U. P shows streaks in a roughly radial direction, and is more dense, more intense, more prominent or more dominant somewhere out from the center, toward the periphery. U is more prominent at the center. Both dwindle at the extreme limits, though there are no definite limits in the sense of boundaries.

(7) At a high degree of intensity affective experience becomes perceptive and, in the case of U, passes over into emotion. This passage was not reported for P.

(8) The affective component of an experience goes on at the same time with the sensory component. It rises after the sensory component; P comes in gradually though quickly, while U comes in at full strength. Both P and U run a course, which may be either continuous or intermittent, and disappear with or before the sensory (or imaginal) component.

(9) Affection is palpable; it stands up under observation.

(10) The affective component is not definitely localized. Usually it is not localized at all. It may, however, be vaguely localized as within the body, or as projected out from the body; or it may be referred to some more or less well defined part of the body. There is some evidence to show that whenever affection is dominant for attention there is no localization.

(11) Neither P nor U appears in isolation. The affective component ties up, under various aspects, to the sensory component of the total experience.

(12) Affection is not simply a meaning carried by organic sensations. Nor is it a blend of organic sensations.

(13) Experience from every sense department may take on the increment of bright or dull pressure, that is, may become affective experience. To arouse an observably affective experience the stimulus must be only mildly intensive, not so intense as to arouse a perceptive experience, and the *O* must maintain a psychological, non-perceptive attitude. The attitude may be active within these limits.

(14) P and U are inherently alike, as if made from the same matrix. Their differences are of the sort that occur within one and the same modality.

(15) There is no qualitative variety either of P or of U. Nor are there qualitative differences within the spread or volume of a given P or U.

(16) P and U do not occur together; they may appear in rapid alternation.

BEHAVIORISM AS A MONISM OF ACTION

By Oliver L. Reiser, Ohio State University

With the current overhauling of the postulates of the physical and the psychological sciences, there is a growing conviction that the time is approaching for scientists to coördinate such results as they have already secured in their respective fields, in an attempt to arrive at that unified outlook which philosophers have termed the synthesis of the sciences. Progress in science is continually demonstrating the truth that no science liveth unto itself, and this further indicates the necessity for coördination, because of the fact that psychology has hitherto been based upon an inadequate philosophy of physics, while the psychology of most physicists is obsolete.

The thesis of the present paper is that the line along which this reconstruction is to be sought lies in what may be termed *universal behaviorism*, the pivotal conception of which is a *monism of action*, rather than a physical monism based upon an antiquated mechanistic materialism. The best presentation of the concept of physical nature consistent with this point of view is that of A. N. Whitehead in his various expositions of the subject of relativity. With the new concept of physical nature, however, we are not at present concerned.

The outstanding hiatus in the biological sciences is the introduction of dualism into psychological explanation. The concept of mind is introduced to explain such complicated activities as volition, purpose, reasoning, *etc.*, which are difficult to reduce to monistic terms. The most effective way of understanding why human beings do the things that they do is to view consciousness and human behavior in the light of their developmental sequence; and a survey of the evolution is therefore the best approach to the establishing of a monism of action in the field of psychology. This is the purpose of the present essay.

The Evolution of Behavior

In some way, probably through the influence of sunlight, the simpler organic colloids have been synthetized into the more complex protoplasmic systems, which form the basis of life. We may obtain some notion how living matter may have taken its origin in the sea from the work on the formation of formaldehyde and the sugars by the action of light of short wave-length on carbonic acid and water. Assuming that the characteristic phenomena of living matter, irritability, metabolism, growth, reproduction, *etc.*, are forms of chemical behavior which accom-

pany protoplasmic systems,[1] we may next consider the evolution of these behavior-complexes, with the end in view of discovering the principles underlying the synthesis of mind.

In the face of the mechanistic assumption that structure determines function, it is usually held that the study of the evolution of behavior is less advanced than the knowledge of the evolution of structures, and that the correlation between structure (morphology) and function (physiology) is still to be worked out. In the present essay we are interested mainly in the evolution of behavior as dependent upon neural integration; but since this form of correlation arose naturally out of chemical regulation, with its axes of polarity, *physiological gradients*, or *growth potentials*, we must observe in what manner it has profited organisms to possess this added perfection.

The irritability of living tissue is the beginning of the stimulus-response relation, and irritability is a property of unstable chemical compounds. This relation, as a fundamental property of living matter, has its origin in the unicellular organism wherein specialization of tissue has not yet taken place. The manner in which protozoa or unicellular organisms developed into metazoa or multicellular organisms is not understood, although it is at once obvious from the principle of cell division—that the division-spindle of the cells tends to divide at right angles to the longest mass of the protoplasm—that the inequality of growth in the three possible dimensions of space will account for all the forms which organisms can take. Why cells should divide and multiply is a matter which colloidal chemistry may some day reveal.

It is well known, of course, that the less specialized a cell is the greater is its versatility in assuming the obligation of different functions. The simpler organic forms still preserve this plasticity of functional adaptation. In this connection it is noteworthy that several investigators who believe that they have demonstrated the inheritance of acquired characteristics on the basis of results wherein the sex of an organism has been changed seem to be overlooking the fact that, while sexual dimorphism took its origin in the protozoa, a number of the lower organisms are potentially bisexual, and that it takes but a small change in the developmental factors—nutrition, for example,—to make them male or female.

The Evolution of the Nervous System

In the lowest metazoa the coördination between the different cells of the colony is secured through chemical means. Thus the sponges have no nervous system, but they function somewhat as

[1] O. L. Reiser, Life as a Form of Chemical Behavior, *Monist*, 34, 1924, 150-160.

if they had. That is, there are muscle cells, sensitive to stimuli, which play the dual rôle of receptors and effectors. The stimulus is transmitted slowly by what is termed "neuroid" conduction (G. H. Parker). In the sea-anemone we find both nerve cells and muscle cells, thus providing the basis for reflex action.

The 'nerve-net' type of organization, consisting of a continuous net of nervous tissue, which is found, for example, in the coelenterates, is the more primitive type of neural coördinating mechanism. From the evolutionary point of view, the nerve-net seems to have given way more and more to the synaptic system of nervous organization, which is held to allow anatomical independence to the individual neurones. But the two systems are by no means exclusive of each other, for they may both exist together within the same organism. The nerve-net still persists in vertebrates, and controls the vegetative processes, such as are connected with the smooth musculature, where the autonomous character of the action is still preserved. Indeed, some investigators argue that the central synaptic system is still subordinate to the autonomic nervous system, and is only concerned with securing a maximum gratification of the emotions, which seem to be largely dependent upon the activities of the smooth-muscle and glandular systems regulated by the autonomic ('sympathetic') nervous system. With the development of the synaptic system, with its neurones functioning as relatively independent units, there is provided the basis for reflex action, consisting anatomically of a sensory, a motor, and a connecting or correlating neurone in the spinal cord. The ganglionic system which develops in the segmented worms increases in size at the anterior end to form the brain. The belief that consciousness appears with the development of the central system, and arises at the juncture of the cortical neurones of the cerebrum, rests upon the fact that the processes controlled by the autonomic nervous system of the vertebrates, which corresponds to the nerve-net of the lower organisms, are normally unconsciously performed.

Unlike the development of the nerve-net in the invertebrates, which develops from within, the evolution of the neuronic system of the vertebrates, or the cerebro-spinal system, is formed by an invagination of the ectoderm to form the neural tube. Three vesicles are formed at the anterior end of the central nervous system, with the ventricles thus produced filled with the cerebro-spinal fluid, while the posterior end, the spinal cord, preserves a segmental character, with the spinal nerves issuing in pairs along the vertebral column. The bending of the vesicles, the fore-brain, mid-brain, and hind-brain, gives a greater length in a smaller volume, and the anterior vesicle is thereby permitted

to develop to such an extent that the cerebral hemispheres, in time, come to cover the rest of the brain enclosed within the cranial cavity. The cortex, the outer rind of the cerebral hemispheres, is the wall of cell bodies constituting the "gray matter" of the brain, which is somehow formed by an evagination of the neural tube.

The foregoing may serve as a rough sketch of the development of the structural aspect of the central integrating mechanism of the higher vertebrates. The functional importance of the nervous system will be considered after we have surveyed in an equally brief fashion the evolution of receptors.

The Evolution of Receptors

Organisms maintain their existence by adapting themselves to changes in the external world of things and events. They are the better provided for their adaptations to their environments in proportion to the number and variety of external phenomena to which they are capable of responding.[2] A perfect organism would have a solution to every problem, as adequate response to every stimulus; the whole universe would constitute its environment. In man alone do we find any approximation to this condition of universal adaptability.

All higher organisms are protected by some sort of shell or cuticle from excessive stimulation from without. Later on individual parts of the surface are then differentiated in such a way as to be sensitive to only one group of excitations, while remaining insensible to all other forms and frequencies. The lower organisms are sensitive to chemical, thermal and light changes, and their responses, known as *tropisms*, are impelled by these forms of excitations. The stimuli that are of an injurious nature were probably responded to first of all. A current theory has it that in human beings these receptors for nocuous stimuli have survived in the free nerve endings in the skin, which give us pain sensations.

Since the most primitive organisms lived in the sea, they would naturally be exposed to a variety of chemical substances dissolved in water. It is found, for example, that the skin of a fish is sensitive to acid, alkali and salts; in many cases the hydrogen ion concentration is the important factor. The senses of taste and smell are higher developments of this primitive chemical sense. Touch receptors were later developed into the present elaborate apparatus for cutaneous sensations.

In a general way, advance to higher levels of response is correlated with an increasing prominence of vision and hearing,

[2]In the following account of the evolution of sense-organs I am indebted to the treatment in W. M. Bayliss' *The Principles of General Physiology*, C. S. Sherrington's *The Integrative Action of the Nervous System*, and C. J. Herrick's *An Introduction to Neurology*.

with a diminution of importance of the other modes of sensations. The eye and the ear are the principal "distance receptors," and the development of vision, which has been called "anticipative touch," has so accelerated the progress of evolution that man has well been called a "space-eater."

In its origin the function of the sensory apparatus was probably to serve as an adjunct to the motor system; but in the present highly elaborate sensory system, in which vision and audition are the most important modes, sensory stimulation has become an end in itself. Aesthetic experience, its own excuse for being, illustrates the general detachment of human activity from its original utilitarian, overt behavior-value.

As we have seen, the function of the receptive cells is to pick out from the mass of different movements in the environment constantly impinging upon the organism those to which the specific sense-organ is attuned. Each sense-organ is irritable to its own adequate stimulus. In this way, the environment is split up so that only parts of it need to be reacted to at any one time. The whole process of the evolution of receptors consists in the development of sense-organs for purposes of action, so that the objects of nature take on sharper outlines and distinctness. This is the position which Bergson defends in *Creative Evolution*, when he argues that the geometrizing intellect has been developed for purposes of action upon objects in space. If we take the cerebrum, which has been evolved around the distance receptors, as the seat of "thinking," this view gains plausibility. The paradox which any such evolutionary theory encounters is this: Did the brain, which is the center of reference for perception and conception, thus, in a measure, "create" the external world, as we know it, or did the external world create the organism and its sensory and motor apparatus? This, of course, raises the whole question of the "causes" of evolution. Lamarck, Driesch and Bergson believe that we must assume that evolution of structure can not be explained without postulating some inherent, striving principle, which, through its adaptive efforts, causes the individual structural modifications, which are preserved and passed on. Orthogeneticists also point out that these variations may persist, in spite of the fact that they have no apparent use at the time of their inception, and only later take on survival value after a number of such variations, persisting in a definite direction, have assisted each other, so to speak. One illustration of this is the gradual modification of the jawbones, so that one of the bones drops out from the structures used in chewing, and comes to be pushed up into the middle ear cavity to function in the transmission of sound-waves from the ear drum to the inner ear.

To the present writer the most tenable doctrine is that neither the sense-organ, nor the brain, nor Bergson's geometrizing intellect at home in a world of solids, has created the sharply delimited bodies of the external world. On the other hand, the organism is not the passively moulded product of the external forces of the universe. Rather, both the organism and externality, as we humans know it, have developed together by a give-and-take in which conceptual interpretation, supplementing perceptual experience, has partially created the forms of external nature, and, in turn, the objective order has forced us to readjust our cerebral behavior ("ideas") to its (the environment's) semi-determinate texture. Through the vast span of time in which this process of mutual intercourse and interadaptation of organisms and their environments has progressed, the environment has continued to take on a more articulate character, while the organism has continued to develop an apperceptive synthesis which it has superimposed upon the external universe, the ultimate structure of which may be unknowable. I assert this not as a student of biology, but as a bystander who watches with interest the attempts of the relativity-physicists to determine whether or not the objective universe is a non-Euclidean space-time continuum, and the attempts of the psychologists to ascertain whether the ability to build up a meaningful universe arranged according to the forms of space and time is innate or acquired. This is not saying that the mere presence of a thinking organism makes the form of electrons different from what it would be if the scientist did not exist; but it does imply that the question of what the shape and mass of an electron-in-itself may be, independent of the presence of the physicist, with his sense-organ and precision-instrument equipment, is an insoluble riddle.

Before dismissing Bergson's teleological conception of evolution it may be profitable to discuss the question of vision as a special case of functional adaptation, inasmuch as it may throw some light upon the mechanism, the human body, the superlatively intricate operations of which have always elicited the wonder of those with vitalistic prepossessions.

Functional Coördination

As an illustration of how the vital impetus, ramifying through the divergent lines of evolution, may manufacture like apparatus by unlike means, thus transcending the mechanistic principle that like effects must have like causes, Bergson cites the development of the eye of the Pecten, a mollusc, and the vertebrate eye. Here we have homologous structures developed in two species which had separated from the parent stem long before the appearance of the organ of vision. The problem which this presents to the mechanist is that of explaining how acciden-

tal causes (random variations), occurring in an accidental order, can have repeatedly come to the same result, the causes being infinitely numerous and the effects infinitely complex.

The eye is complex, being constituted of a number of parts so functionally interdependent as to seem to have been designed for the very purpose of vision. How is this correlation of functions to be explained? The theory of selection from chance variations in all directions does not explain how the parts of the visual apparatus remain so coördinated that the eye continues to function effectively. To speak of sudden variations or mutations only makes the problem more difficult. That a change in the germ-plasm influences the formation of the retina, cornea, iris and lens at the same time is conceivable; but that all these simultaneous changes should occur in a way to improve or even maintain vision is not admissible.

Another way of accounting for the appearance of the eye is to suppose that variations are due, not to accidental inner causes, but to the direct influence of outer circumstances. Though the molluscs and vertebrates have evolved separately, both have remained exposed to the influence of light. The resemblance of the two effects may be explained by the identity of causes: light acting directly upon organized matter so as to change its structure, and somehow to adapt this structure to its form. But, Bergson argues, the term adaptation is ambiguous. In one sense it means the receiving of an impress from the outside; in another sense it is a positive reaction, a solving of a problem. It is in this sense of the word that we say that the eye has become better and better adapted to the influence of light. Nature herself appears to invite a confusion of the two kinds of adaptation, for she usually begins by passive adaptation, where, later on, she will build up a mechanism for active response. Life adapts itself to matter at the outset, and later directs the movement it adopted.

When we say that the eye makes use of light we do not merely mean that the eye is capable of seeing; we mean the precise relations that exist between this organ and the apparatus of locomotion. The retina of the vertebrates is prolonged into the optic nerve, which is continued by cerebral centers connected with motor mechanisms. No one would hold that light has physically caused the formation of the nervous system, or of the muscular system, all of which is implicated in the usefulness of an eye. According to Bergson, whether we will or no, we must appeal to some inner directing principle in order to account for the convergence of effects.

I have entered into this exposition of Bergson's theory to afford a starting-point for a few remarks concerning the problem of correlation of functions of different structures. Accepting

Bergson's criticism of mechanism, in so far as mechanism rests upon a confusion of the abstract time of physics, which is a "phantom" of space, with the concrete duration of what A. N. Whitehead calls "the creative advance of nature," we must, at the same time, reject the *élan vital* as the cause of evolution. Bergson's arguments for vitalism rest upon the inability of science to explain certain mysterious results, and illustrate the danger of this mode of argument,—for the progress of science consists in finding "mechanical" solutions to the problems which were formerly taken as an excuse for the introduction of non-natural agencies. (I use the term "mechanical" under protest, for it is doubtful whether it can be defined in a satisfactory manner.)

More recent work on the mechanism of correlation and functional interdependence may help the biologist to understand many of the phenomena which have hitherto seemed baffling. The results have been summarized by A. Keith[3] in his Huxley Lecture on "The Adaptational Machinery Concerned in the Evolution of Man's Body." In the case of vision the researches of W. H. Lewis have shown how the optic cup, which ultimately forms the retina of the eye, grows from the wall of the brain towards the embryonic skin or ectoderm. "When this cup comes into contact with the ectoderm, the underlying cells begin to proliferate and arrange themselves so as to form a transparent crystalline lens. Dr. Lewis transplanted the outgrowing optic cups of tadpoles, and found, if they were placed under the ectoderm of the neck or belly, that the result was the same; an optic cup caused the underlying cutaneous cells to alter their nature and form a lens. Dr. Lewis realized the significance of his discovery; in the developing embryo, although only of certain species, one group of living cells can enslave and control the behavior of another group. He gave us a glimpse of the kind of evolutionary machinery employed in fashioning a highly purposive structure such as the eye."

These results on the regulatory influence of one group of cells on another, which holds between spatially distinct cell-groups of the cortex of the brain as well as between nerve fibres and their muscular termini, are of great interest in connection with the phenomenon of chemical substances and ferments (*hormones, autocoids, etc.*) passing from one structure to another to influence the behavior of the latter.

The theory of hormones, which J. J. Cunningham has already applied to the problems of heredity, may have to be extended to include the field of botany; for there is evidence that in plants the transfer of stimuli from the receptive to the motile

[3] *Nature*, 112, 1923, 257-268.

regions is effected by the passage of "chemical messengers." Of whatever value such speculations concerning hormones as "formative stimuli" and as bearers of racial memories máy be, such illustrations of chemical integration and regulation are of value in warning us against the hasty conclusion that no mechanism is conceivable which will enable us to understand the phenomenon of functional interdependence and organic intra-adaptation. In connection with the role of biochemistry in genetics the results of M.F. Guyer[4] and E. A Smith are especially noteworthy. Guyer and Smith injected into pregnant rabbits serum from the blood of a fowl, into which extract from lens-tissue of rabbits and mice was introduced, with the result that the lenses of the embryonic rabbits were attacked and the young showed defects of the eyes, which persisted in the second generation of rabbits in which no injection was made. Aside from the bearing this may have on the question of the possibility of modifications of the individual germ-plasm producing a racial effect, there is the interesting question whether there is any relation between the effects of the toxic action of the anti-bodies and the chemical regulation of the organs of the body by hormones.

Cerebral Behavior

Having surveyed the evolution of receptors and the central nervous system as the correlating mechanism which connects sensory and motor points of the body, thus through its coördinating operations securing harmony of action of complicated receptor-effector patterns, we are now in a position to attempt to fit what may be termed either cerebral or mental behavior into the general notion of a monism of action. The supreme importance of the nervous system in behavior is obvious to all students of biology. It provides for variability of response, for harmony of action in adjustments which involve extremely complicated sensori-motor configurations, and is the basis of "intelligence," which may roughly be defined as the ability to learn by experience. Intelligence is the capacity of bringing relevant past experience to bear in adjustment to the novel situations which constitute the problems of organisms. This seems to be largely a problem of complexity of neural tissue.

That intelligence is not peculiar to man, but has its beginnings early in the ladder of evolution, is evident from experiments on lower organisms such as have been conducted by H. S. Jennings. Intelligence has its origin in the "trial and error" learning or experimental behavior which is found in the Infusorian. Thus the Slipper Animalcule overcomes its obstacles by reversing the action of its cilia, backing away from

[4]*Journ. of Exp. Zoology*, 31, 1921, 171.

the obnoxious stimulus, and advancing again on an angle slightly diverging from the original axis; and the Stentor solves its problems by selecting the appropriate response from its behavior-repertoire.

Higher organisms come into the world with an inherited set of responses, and it only takes the appropriate stimulus to release the adaptive adjustment. There has been and is much dispute concerning the definition of these supposedly inherited modes of adjustment. Instincts have been defined in various ways, but none of the definitions has proved generally acceptable. The consequence is that some students have gone so far as to deny the existence of specific instincts.

There is also the question how instincts, if any, have been built up, the same problem arising again which has refused to be put out of court, *i. e.*, whether instinct is "lapsed intelligence" in the sense that it represents the persistence of a mechanized form of action once purposively and consciously performed, or whether the evolution of behavior is the evolution of structures, determined by principles in which the inheritance of acquired characteristics plays no part. This problem of the relation of intelligence to instinct has been a fertile field for controversy for many years. On the anatomical side we find the main dichotomy to lie in the difference in size of the cerebrum: instinctive behavior reaches its highest development in ants, bees and wasps, whose "little-brained" nervous systems stand in marked contrast with the "big-brained" mammals, with their consequent intelligent behavior. It is for this reason that I have designated *mind* as a form of "cerebral behavior." However, the integrity of this supposedly clearly defined difference between instinct and intelligence is threatened by the development of the doctrine of the *conditioned reflex,* the purport of which is the reduction of all learning to the status of acquired responses which are conditioned on reflex behavior in such a manner that the factor of intelligence becomes a name for neurone and synaptic factors such as "modifiability," "retentiveness," *etc.*

From the point of view of a monism of action, what needs to be pointed out here is the reason why people do the things that they do. If we accept, what many geneticists take to be a fact, that so-called "mental traits" are inherited, then we must start from the position that in some way or other there are potential pathways of response already laid down in the nervous system, even in the child which, in its early infancy, has not modified to any extent these inherited tendencies to reaction by alteration of the resistances of the cortical patterns (if inherited mental traits are there localized) by training or experience. All subsequently acquired reactions modify these pathways, but what

we can learn is, in turn, determined by the original inheritance. What I am, then, is the sum total of all my inheritance *plus* the modifications due to experience. The factors, therefore, which determine which way a nervous impulse will go when it reaches the cerebrum are *heredity*, *habit* and *fatigue*. The doctrine that the mind, as a non-biological entity, acts as a kind of switchman, standing at the cortical synapses, shunting trains of nervous energy over this or that track, meets with insuperable difficulties.

There is another theory which is of value in connection with the doctrine of learning as a process of establishing conditioned reflexes. This is the theory that cerebral nervous impulses tend to irradiate over a number of pathways, and that it is only through repeated behavior that a definite response is linked more or less automatically with a definite stimulus by the formation of a "final common path." The hypothesis of irradiation is interesting because it suggests the mechanism whereby the "collateral ideas," which normally serve as a check upon behavior, are inhibited by "suggestion." This also throws some light upon the matter of belief. It is well known that the judgment of an expert with a wide experience is much more accurate and immediate in his own field than is that of a novice. Both belief and readiness to act depend upon the resistance, or lack of it, which an idea meets with in the individual. This past experience may be stored as brain patterns, if we regard *ideas* as rearoused *percepts*, and under normal conditions any idea tends to call up the relevant ideas associated with it. Suggestion, hypnosis, fatigue, narcosis and toxic conditions are circumstances in which these collateral ideas are temporarily banished from consciousness, and can therefore no longer serve as checks to inhibit ideas or behavior which normally would be regarded as abnormal and ridiculous by this same individual. These conditions eliminate these inhibitory ideas by draining the nervous flux, which ordinarily tends to irradiate and arouse the collateral patterns, into one channel which is thereby strengthened.

While the physiologist does not fully understand *facilitation*, *inhibition*, and the "drainage" of nervous energy, all of which are involved in the psychological phenomenon of *attention*, and can not say just what happens at the synapse, there is no reason for supposing that when the explanation is forthcoming it will be given in any other terms than those which have always been connected with progress in science.[5] The advantage of the preceding view seems to be that it provides a mechanism for the understanding of individuality and of "multiple personalities"

[5]The difficulties which confront the psychologist who is concerned with the intellectual aspects of human conduct I have pointed out in an article "The Structure of Thought," *Psychol. Rev.*, 31, 1924, 57-73.

as complexes of patterns of ideas and feelings, which may split up into separately functioning individuals, if the integrative action of the nervous system is not sufficient to unify these clusters of sub-individuals into a "personality."

In the preceding account I have emphasized the neural conditions of human behavior. This emphasis upon the nervous system as an autonomous organ of central integration will be regarded by the "behaviorists" who stress the response (effector) side of behavior as old-fashioned. The present position is in accord with the statement of G. E. Smith[6] that "the secret of man's most distinctive attribute (intelligence) is hidden in the texture of his brain, and perhaps will never be fully revealed." To me it seems impossible to stress too much the significance of the nervous system, because in the synaptical resistances is to be found the gradation of energy thresholds, or levels of synthesis, which can account for neurasthenia, senile degeneration and allied disturbances of personality, which consist in the dropping down to levels of response requiring less energy expenditure. It is through some such conception of the evolution of behavior as the progressive integration of levels of reactions that the synthesis of mind is to be viewed.

Physiological Integration and Psychic Synthesis

The tenability of the view herein set forth, namely, that the unity of mind is what might be called an overtone of the individuality characteristic of organismic integration, rests upon the validity of the thesis that there is no opposition between physiological processes and the conduct of mind conceived even in intellectualistic terms. To this end I wish to point out that bodily processes bear the stamp of their subordination to the synthetic character of organic integration of function in the "organism as a whole." If this be true, as I shall briefly argue, then the prime fact of consciousness, the synthetic and purposive character of its movements of abstraction and generalization, is a reverberation of physiological integration through the differentiation of structure and integration of function.

This principle of a hierarchy of levels of synthesis, introducing unity of action through the relation of dominance and subordination between the integrating mechanisms of complex organisms, is being recognized on all sides. That it represents the actual pathway of evolution is pointed out by J. A. Thompson[7] who holds that the clue to the maze of animal behavior "is that there has been at level after level a process of automatization or organization, which makes for economy of time and energy, and also, if it does not go too far, leaves the organism

[6]The Human Brain, *Nature*, 113, 1924, 390.
[7]*The System of Animate Nature*, 1, 1920, 194.

free for experiment and initiative." C. S. Sherrington[8] credits Hughlings Jackson with having pointed out the significance of "levels" of neural machinery standing in the relation to each other of drivers and restrainers.

One of the clearest recent statements of this doctrine is presented by H. Head,[9] who adopts the term *"vigilance"* to express this fact of the functional efficacy of the neural hierarchy. As he says: "There is no more difficulty in understanding how an act of consciousness can affect a physical process, than to comprehend how one reflex can control and modify another of a lower order."

On the basis of experiments on the regeneration of nerves Head and his colleagues have distinguished two different kinds of mechanism on the afferent side of the nervous system. These are (1) the protopathic stage, characterized by the absence of any exactness of discrimination or localization, and by the presence of pronounced feeling tone; and (2) the epicritic stage, which marks the return to the normal spatial perception of exact discrimination and localization. It is probable that these two types of sensibility represent distinct stages in the evolution of cutaneous sensibility. According to Head and Holmes the relation between protopathic and epicritic sensibility is analogous to the relation between the cerebral cortex and the optic thalamus. Furthermore, according to W. H. R. Rivers, who discusses the subject in his *Instinct and the Unconscious*, the relation between these two forms of sensibility suggests the manner in which the suppression of experiences in the individual may recapitulate the racial suppression of earlier phases of evolution.

This conception is of interest because, as Rivers argues, the property of repression arises out of the organization of the nervous system into a hierarchy whereby the higher levels control the lower ones. The hierarchy represents strata of potential behavior, the lower levels representing levels of unconscious experience suppressed in something of the same fashion in which the tendency to affective over-response of the thalamus is inhibited by the cortex. It also suggests the problem how the evolution of the brain as the correlating mechanism through which the unification of tactile and kinaesthetic senses with stereoscopic vision, involving eye-movements of accommodation and convergence, has been secured; and how the synthesis of visual space with the motor space given in the handling reactions has provided the basis for the development of a curiosity in

[8]Some Aspects of Animal Mechanism, *Nature*, 110, 1922, 350.

[9]The Conception of Nervous and Mental Energy (II), *Brit. Journ. Psychol.*, 14, 1923, 126-147.

man for knowledge concerning the world about him. One might carry these speculations so far as to assert that physiological correlates for all of the mystical entities of the psychoanalysts are discoverable. Thus, E. W. Scripture[10] claims to have isolated the voluntary activities underlying Freud's tripartite division of the soul, and to have recorded them on psychological laboratory apparatus.

On the functional side this conception of levels of synthesis is being popularized by the adherents of *Gestalt*-psychology, with M. Wertheimer, W. Köhler and K. Koffka[11] as the outstanding exponents. However, the recognition of qualitative wholes and unitary responses to multiplicity of stimuli as expressive of the phenomenon of psychic synthesis is by no means a new doctrine. While the doctrine goes back to Lotze (perhaps even to Plato) it may be recalled that, in the field of vision, McDougall[12] seized upon this fact of synthesis as an argument in favor of his doctrine that the mind can not be reduced to bodily processes. In the field of audition an excellent discussion of the problem of tonal fusion, such as the "micro-psychic" rhythms of Lipps and Stumpf's hypothesis of "specific synergy," is set forth by R. M. Ogden in his book on *Hearing*. It is the opinion of the present writer that psychologists have underestimated the rôle of unconscious synaesthesia, especially in aesthetic perception. Without attempting to enumerate any further the different variations of this theme which Wundt called *creative synthesis*, we may note that the whole movement seems to be part of a general reaction against the fallacy of over-analysis, which is at the bottom of the *emergent* theory of C. Lloyd Morgan and S. Alexander. So far as the present writer is informed, the best biological explanation of the hierarchy underlying the synthesis of unitary responses is presented by C. M. Childs in the theory of mechanical, chemical and neural integration expounded in his *Individuality in Organisms*, and *The Origin and Development of the Nervous System*.

In the above doctrine is contained the explanation of the relation between consciousness and the nervous system. That is to say, the relation between structure and function, body and soul, and matter and energy, is the homologue of the pervasive correlation between the two mutually complementary conceptions of the static and dynamic, the spatial and the temporal.

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[10]Three Biological Principles Observed in Speech Inscriptions, *Nature*, 113, 1924, 386.

[11]For a summary of this movement see Koffka's article: Perception: An Introduction to *Gestalt-Theorie*, *Psychol. Bull.*, 19, 1922, 531-585.

[12]The Relations between Corresponding Points of the Two Retinae, *Brain*, 33, 1911, 371-388.

THE FLIGHT OF COLORS[1]

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Introduction

Among the most frequently noted visual phenomena is the series of after-images which follows the fixation of a bright light. It is generally known as "the flight of colors." Although these after-images have been more than an occasional object of experimentation for several centuries, the results have been far from uniform. Berry, in an historical summary of the data, states: "The sequence of the colors in the after-image has varied from one observer to another. Only in one or two cases has there been close agreement."[2] The following color sequences obtained by several experimenters verify this statement:

Goethe—bright, purple, blue, dark[3]
Fechner—blue, green, red, blue[4]
Purkinje—bright, bright red, dark, bright, gray[5]
Scoresby (1)—green, yellow-green, yellow, orange, red, scarlet, crimson, brown or olive
Scoresby (2)—dingy orange, olive, yellow, gray or blue, black[6]
Washburn—blue, green, red, blue-green[7]
McDougall—green, red, blue, green, red, blue[8]
Honmouth—blue, purple, violet, yellow[9]

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[1]From the Psychological Laboratory of Wellesley College. Communicated by C. A. Ruckmick.

Titchener has suggested that the sequence of colors may be due to an alternating excitation of the B-Y and R-G substances with the intermediate effects due to the operation of laws of color-mixture among some of the colors aroused in the the after-images.[10]

The disagreement between the above sequences is typical of other items in the reports. Several authorities have asserted that a stimulus of low intensity does not give colored images,[11] while one of them concluded that the images of any white light were always colored.[12] Goethe, Fechner, and Honmouth reported the advance of colors in the image from periphery to center, while Helmholtz described colors in an image of moderate illumination as irregular in appearance, often advancing from one side of the image to the other.[13] Honmouth, opposed to Goethe,[14] concluded that there are no gradual transitions from one color to another in the image, but that constant rivalry or kaleidoscopic displacement exists among them. Fechner framed the general rule that, whenever the duration and intensity of the stimulus have reached a certain degree, any increase in either does not alter the constancy of the color series.[15]

The present investigation was undertaken partly in view of the disagreement in results of previous experiments, and partly because the data, though divergent, suggested nevertheless certain underlying factors. Seguin, Scoresby, Fechner, Hodges,

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[2]W. Berry, Flight of Colors in the After-Image of a Bright Light, *Psychol. Bull.*, 19, 1922, 333.

[3]J. Goethe, *Zur Farbenlehre*, Werke 1833, 52.

[4]G. T. Fechner, Ueber die subjectiven Nach- und Nebenbilder, *Pogg. Ann.*, 50, 1840, 445-70.

[5]J. E. Purkinje, Beobachtungen und Versuche zur Physiologie der Sinne, *Beit. zur Kennt. des Sehens in subj. Hins.*, 1819, 92.

[6]W. Scoresby, An Inquiry into Some of the Circumstances which Regulate the Production of Pictures on the Retina, with their Measure of Endurance, their Colors, and Changes, *Proc. Roy. Soc. Lond.*, 7, 1854, 117-22.

[7]M. Washburn, The Color Changes of the White Light After-Images, Central and Peripheral, *Psychol. Rev.*, 7, 1900, 39.

[8]W. McDougall, Some New Observations in Support of Young's Theory of Light and Color Vision, *Mind*, N. S. 10, 1901.

[9]Honmouth, Beiträge zur Kenntnis der Nachbilderscheinungen, *Arch. f. d. ges. Psychol.*, 26, 1913, 181-238.

[10]E. B. Titchener, Experimental Psychology, Instructor's Manual, Qualitative, 1901, 48.

[11]E. W. Brücke, Untersuchungen über subjective Farben, *Pogg. Ann.*, 84, 1851, 418-52.

[12]E. Seguin, Recherches sur les couleurs accidentelles, *Ann. de Chim. et de Phys.*, 41, 1854, 413-31.

[13]W. Berry, *op. cit.*, 334.

[14]*Op. cit.*, 330, 311.

[15]*Op. cit.*, 313.

Washburn, McDougall, and others are agreed that changes in the after-image accompany variations in the intensity and duration of the stimulus.[16] They do not agree upon the order of these changes. Our problem was to study the after-image and to analyze it with reference to specific alterations in the intensity and duration of the stimulus.

Experimental Procedure

Observers. Of the 11 persons who served as *O*s throughout the experiment, eight, Fr, Ma, Mc, Mo, Sc, St, and Tr, were undergraduate students taking courses in the advanced laboratory and with some training in introspection. Sh was a graduate assistant, and Ru and Ga were professors in the department. The writer acted as *E* throughout. With the exception of Ga, the eyes of all *O*s were in good condition. By means of lenses Ga had about one half normal vision.

The number of observations varied from 3 to 10 at one sitting, according to the duration of the fixation period and the intensity of the stimulus fixated. Seven hundred and fifty four observations were completed in the experiment, which lasted five months.

Apparatus. The experiments were conducted in a dark room, 2.8 by 5.6 m., which had no windows. All surfaces of the room and the furniture in it were painted black. The *O*s reported "total darkness" during the periods of adaptation and at all times when they were not exposed to stimulation. During the experiment *O* sat with her back to *E* and faced the stimulus, which could be raised or lowered to the level of *O*'s eyes. *E* sat at a table, 2.5 m. behind *O*, where she controlled the apparatus and recorded *O*'s reports. A large black screen, 1.2 by 1.5 m., was in front of *E*'s desk, to prevent *O* from seeing any reflection from the small desk-light.

A wooden box painted black and lined with white cardboard contained the stimulus-lamp. This box, which was placed on the shelf 1 m. in front of *O*, was 26.8 cm. high with a 14.7 cm. square base. A removable sheet of black cardboard, held in place by black thumb-tacks, with a Greek cross[17] cut out of it, formed the front covering of the box. The aperture was cut, not from the center of the cardboard sheet, but two-thirds of the distance to the top. The cross was 36 mm. in length and the width of the arms was 6 mm. An aperture of the same shape, though much wider, was discarded after a few trials during the preliminary procedure, because the edge of the cross did not appear "clear cut." One thickness of architect's paper was

[16] *Op. cit.*, 334.

[17] The particular form of the cross was the form known in heraldry as the "cross humetté or couped."

pasted across the back of the aperture. A black dot was drawn on the paper in the center of the cross as a point of fixation.

The box contained one of four "daylight" mazda bulbs rated at 25, 50, 75, and 100 w. The lamp-base was screwed to a flat piece of wood 1 cm. thick and small enough to slip easily into the floor of the box. It was necessary to attach the lamps to this movable piece in order that the smaller lights might be raised to keep the filament of the lamp directly behind the aperture which *O* fixated. Within the box there also was a 2 v. "flashlight" bulb which, when quickly flashed on and off by *E*, served as a 'Ready' signal to *O*. It indicated the approximate location of the stimulus in the dark room, and still it was too weak and too brief for after-image effects to arise. Three holes were bored in the top of the box for ventilation, and a small black tin shield, tacked around these, prevented any leakage of light. The wires connecting both the stimulus-light and the 'Ready' signal came through holes bored through the back of the box. These wires passed to switches on *E*'s table. A transformer provided current for both the 'Ready' signal and the table-light. This 2 v. table-light had a black shield around it to prevent reflection of light. A stop-watch was used to record temporal changes.

Conditions. All *O*s were able to fixate the dot without difficulty except Ga, who was requested to fixate the center of the cross. Several *O*s did not see the dot immediately when the 100 w. stimulus was exposed, but saw it under less intense stimuli. The fixation point became more distinct as the duration of the stimulus increased. The color changed somewhat with variations in intensity, from white to a slightly yellowish hue. Occasionally *O*s reported that the paper was granular when the stimulus was weakest (25 w.). For the most part the quality of the paper was not noticed. The *O*s were allowed to rest at intervals for varying periods of time if they reported that the stimulus was becoming painful.

We carefully avoided the inclusion of hypnagogic imagery in the flight of colors. Mo and St gave accounts of this subjective imagery which occasionally follows the after-images. The hypnagogic images were distinguished in that they were more vivid, much larger, and seemingly nearer the eyes than the other images. A vivid reddish purple was the color commonly experienced in this type of imagery.

Method. *E* read the following instructions to *O* during the 5-min. adaptation period before each experiment:

"At a given ready signal followed by the flash of light, you will steadily fixate the black spot on the Greek cross and continue to do so during exposure to the stimulus. As soon as the cross is illuminated, report all visual

changes noticed. After exposure, describe as fully as possible all visual effects and changes, including quality and form. Proceed from the center of image outward. Signal as soon as you experience any image."

A short preliminary series preceded the main experiments. The object of this series was to give the *O*s practice in describing the images, and *E* practice in technique. The preliminary series was begun with a 20 sec. exposure to a 50 w. stimulus. The duration was lengthened to 40 sec. as Ru and other *O*s believed a 20 sec. exposure too short a period on which to base the intensive series.

A. Intensive Series

The intensive series was begun with a stimulus of 50 w. and a duration of 40 sec. The 50 w. stimulus was used first because it was of medium intensity. After the adaptation period, in which the directions were given, *E* said "Ready," pressed the signal button, said "Now," and switched on the stimulus and started the stop-watch. At the end of 40 sec. *E* turned off the current and reset the stop-watch immediately to time the appearance of the image, the changes within it, and the length of the total image. *E* noted in a column at the left of the report the sec. at which each statement in the observations was given.

The following symbols were used for recording the colors: *R*=red, *B*=blue, *BK*=black, *G*=green, *g*=gray, *P*=purple, *PK*=pink, *Br*=bright, *Bn*=brown, *L*=lavender and *Li*= light. After each observation *O* reported other details which the rapid passage of the images had made it impossible to report at the time. This procedure was repeated several times during the observation hour. After each *O* had been stimulated 6 or more times by the 50 w. light, the intensity was changed to 75 w., and the experiment was carried on as before without change in the length of fixation. The lamp was then replaced by the 100 w. lamp and later by the 25 w. lamp. The duration of fixation for 40 sec. was retained throughout this series.

Although *E* noted that the *O*s usually described the images in the same terms, she had no exact means of ascertaining that they were all referring to the same qualities. And, on the contrary, although they sometimes described images in unlike terms, *i.e.*, bright *R* and bright cerise, they may have been experiencing the same quality. *E* therefore referred *O* to a chart of colors after every observation, and asked her to compare and contrast the colors in the image with those on the chart.[18] The chart consisted of 44 inch-squares of colored papers, numbered and arranged in a graduated series on a gray cardboard sheet 1 m. by 1 m., in the order of the hues around the base of the color

[18]The chart was made from the standard Milton Bradley papers.

prism. Upon seeing these colors *O* frequently mentioned characteristics of the images not noted before. For example, St said: "The color I saw was not a *R* like this one, but a luminous color, as if an electric light were shining behind a thin film of *R*." With practice the *O*s were able to remember the images until after they had seen the chart. This improvement in the procedure was of added importance, in that *O* attended to the images carefully in order to remember them as accurately as possible.

A method somewhat similar to that just described was used to determine if the sizes and shapes of the images were the same for different *O*s. Each *O* was asked to draw in outline the relative sizes and shapes of the images as compared with the sensation immediately after observing them. Usually *O* drew four or five figures representing the successive stages of the image. These drawings enabled *E* to know what *O* meant by "a pointed cross," "a diamond," or "a cross beginning to lose its ends," and were used to supplement and check all verbal reports.

B. Durational Series

The latter part of the experiment was devoted to the durational series. The 50 w. stimulus was exposed for 1 min. 30 sec., 60, 20, 10, and 5 sec. The 5 sec. interval was then repeated and the 10, 20, 60 sec. and 1 min. 30 sec. intervals, while the intensity of the stimulus remained fixed at 50 w. The results of the 40 sec. periods of stimulation were transferred from the intensive series to this. Had the longest fixation period been 1 min. 20 sec. rather than 1 min. 30 sec., the scheme of 20 sec. intervals would have been complete. The 1 min. 30 sec. period was first taken out of curiosity on the part of *E* to learn how *O* would react to it; and as the time for experimentation was limited, and several *O*s had already been exposed to the stimulus for this length of time, this period of stimulation was continued.

The *O*s were exposed to the 25, 50, 75, and 100 w. stimuli and were asked to report according to the directions first given. Because the *O*s could not keep in mind all the colors of the images resulting from the varying fixation periods, they were not required to compare the images with the colors on the chart, nor were they asked to draw the size and shape of the images, unless they were unusual. To make sure that the *O*s were correctly distinguishing one color from another they were given the Jennings "Color-Blindness Test." No *O* was found to be color blind.

Results

The results obtained from experimenting both with the intensive and with the durational series have been divided ac-

cording to the qualities of the images as indicated by the *Os*. These qualities reported with and without questioning by *E* are indicated by the following headings: (1) hue, tint, and chroma of image, (2) rise and subsequent temporal course of image, (3) fluctuations in image, (4) size and shape of image, (5) position of image, (6) rivalry and movement of color. The various observations have been considered with reference to the average reports both of the individual and of the group.

The usual and average time of appearance of the image after stimulation by the 25, 50, 75, and 100 w. was about 3 sec. after the removal of the stimulus, as is shown by the following chart.[19]

Stim.	*Number of Observations*	*First Appearance of the Image (Sec.)*			
		1	2–3	4–7	7–10
I	59	25	28	5	1
II	55	16	17	22	
III	60	8	36	15	1
IV	54	4	49	1	

As is indicated by the chart, a greater number of the images of I than of II, III, or IV appeared immediately. The more intense the stimulus, the less likely was the image to come as soon as the stimulus was removed. There is closer agreement concerning the moment of appearance of the image in IV than in I, II, or III. Seven of the 15 images that appeared after 3 sec. in III were Ga's. *O* occasionally experienced the image more than 3 sec. after the removal of one stimulus if she saw it occasionally 3 sec. after another stimulus had been removed. Five of the 11 *Os* always saw the image before 3 sec. after the disappearance of the stimulus, regardless of its intensity.

The average length of the images of I is 1 min. 10 sec.; of II, 2 min. 50 sec.; of III, 3 min. 35 sec; of IV, 3 min. 35 sec. The duration of the after-image of a relatively weak stimulus, *i.e.*, of I, differs from that of II by an amount twice as great as that by which the duration of II differs from that of IV, although the difference in the intensities of the former stimuli is but one-half that of the latter. The stimulus IV is 25 w. greater than III, moreover, although the average lengths of the images are about equal. The longest of the images of III and IV are but little longer than the longest of II, but II has many more "short-time" images. For instance, 14 images of II while only one of III and 3 of IV ended before 2 min. 15 sec. On the whole, the duration of the images of I and IV shows less variation than that of the images of II and III.

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[19]The Roman numerals I, II, III, and IV are used to indicate respectively the stimuli of 25, 50, 75, and 100 w. Note that 40 sec. is the constant period of fixation throughout the intensive series.

The reports indicate that, if an *O* were subject to unusually long images after exposure to one stimulus, she was likewise subject to unusually long images following exposure to other stimuli.

INTENSIVE SERIES

Hue, Tint, and Chroma of the Images

In the images resulting from I, *B* and *P* were the colors that appeared most often and were most sustained. *B* appeared in all observations except those of Ma and Mo; and *P* was experienced by all *O*s except three, Sc, St, and Mc. *G* or *YG* was observed by 4 *O*s, *Y* by 2, and *R* by one. These colors were seen with few alterations, as is shown by the fact that half the *O*s saw only *P* and *B* with no other changes. Four was the maximum number of colors experienced in any observation. All *O*s, with the exception of three, reported only one image without a halo or flare of any kind around it. These three *O*s described *B*, *G*, or *P* halos in the period between 7 and 25 sec. after the removal of the stimulus. Every *O* except St pointed to the same *B* color on the chart as being most like the *B* of the image. St pointed to the color nearest it. Although Ma was one of the 2 *O*s who did not report *B* in the image, she did, nevertheless, say that the *V* background was like the *B* color on the chart. The *P* and *BP* when imaged were usually reported as being like or near a *P* on the chart.

The following remarks are typical of those made concerning the tint and chroma of the image.

Fr "*P* cross, well saturated, gets darker gradually. Cross more saturated than background. *R* brilliant and fleeting."

Jo "Colors very bright at first, get darker. *B* in the cross darker and has more *P* in it than the *B* halo."

Ma "Colors dark and heavy. Do not change much."

Mc "Colors all darker and less saturated than in the images following more intense stimulation."

Mo "*G* well saturated. *BK* closes in upon dark *P*."

Sc "*B* a very dark color. Background becomes darker."

St "Background rather good. Colors in image *g*-ish."

Tr "Colors unsaturated. Cross gets darker. *B* very dark and poorly saturated."

In her various observations, *O* usually reported the same colors and the "shifts in colors," if there were any, at the same intervals.

The images following the fixation of II may be separated into definite periods in which particular hues predominate. *Y*, usually with one or more margins and halos, was the color most often imaged first. Thirty-three out of 60 observations had *Y* for the first after-image. Seventeen of the rest which began with *G* or *YG* changed to *Y* within a few sec. Consequently, in 50

out of 60 observations *Y* was reported as appearing immediately or almost immediately when the stimulus was removed. *B* appeared first in 3 images and *O* in 2. Halos were reported around the first image in all but 10 observations. Many times they appeared a sec. or two after the main image, and stayed about 20 sec. The usual *Y* image that appeared first was surrounded by a *R* margin within a light *G* halo. This *R* and *G* appeared in two-thirds of the observations. A dark *B* was reported many times between the *R* and the light *G* around the *Y* center. *BK*, *P*, and *L* were the colors experienced infrequently in the halos in place of the *R* and light *G*.

All but two *O*s reported that *R* took the place of the *Y* image. The *R* margin increased at the expense of the *Y*, until the *Y* became a mere spot within the *R* cross. Sc experienced no *R*, but continued to image *Y* with a border of dark *G* and either a *Li* or light *Y* halo. Ma, Mc, and Tr usually reported *Y* in the center of the *R* during this period. Other *O*s experienced no *Y* in the *R* image. A dark *B* or *G* border and a *Y* halo around the *R* image were often noted. Never were more than one border and one halo seen at a time.

Approximately 50 sec. after the image first appeared the *B* or *G* border had completely closed in upon the *R* which replaced the *Y* spot in the center. The images of Sc, Sh, and Ga became dark *G*, and those of Mo, *BK*. In the stage previous to this Mo had noted a *BK* outline around the *R* cross instead of a *B* or *G* one. This *BK* outline seemed to push the *R* into a spot just as the dark *B* and *G* outlines did in other *O*s' images. The other 7 *O*s reported that dark *B* replaced the *R* in the image. All *O*s described a light *G* or *Y* or a *YG* halo around the principal color, *B*, *G*, or *BK*.

The *R* completely disappeared, as the *Y* had done, from the center of the *B*, *G*, and *BK* images, and left the images still surrounded by the *G* or *Y* halos. At this time the *B* images which Tr experienced changed to *G*, and the *B* of Ma's images to *BK*, in several observations. This phenomenon did not occur with the corresponding disappearance of *R* in the observations of the other *O*s.

The image gradually darkened and often became *BK* before it disappeared. The images of Sc and Ga alone became light. Sc always reported a light spot, and Ga a light image at the end. Mc, Mo, and Tr visualized a small light halo around the dark color, but the remaining 6 *O*s experienced only the dark spot.

A few statements concerning the tint and chroma are here noted.

Fr "Colors more intense at first. *G* halo light. *R* gets darker. All colors darker at end. Colors of medium saturation."

Ma "First G cross light. Bright R cross gets darker and less saturated. Y cross bright and B dark."

Sc "First Y cross very bright. G crosses, except at last, of very dark tint."

Mo "Y halo very dull. Colors not very bright."

Tr "First cross pale G. Often G crosses very dark."

Sh and Ru "R, color a sort of cerise."

Ga "Colors unsaturated and faint. Dimmer than any on chart."

Summarizing these observations, we may say that the colors in the image became darker, the B and G crosses were always very dark, and the first Y cross was light, sometimes very bright and sometimes fairly well saturated.

A general trend in the images has already been noted. First there appeared a Y image with a R margin and sometimes a dark B or G halo around it. R gradually took the place of the Y and in turn gave way to dark B or G with a light G or Y halo around it. This image slowly darkened until it became BK or almost BK and disappeared.

Of the 74 observations of III, Y appeared first in 33, G in 25, YG in 11, and P in 5. The G, YG, and P images, with the exception of 3, all changed to Y within a few sec. These 3 observations began with a GY hue which did not change until it became R.

When the image of III appeared it had one or more halos, except in 16 observations; 5 of the 16 were Ga's, who saw no halos at any time during the images following this intensity; 5 commenced with a P spot, 4 with a G cross, and 2 with a GY cross. As soon as these images changed to Y they were invariably surrounded by halos. R, G, B, P, BK, and OY were the halos observed most frequently in the order given. Fr usually saw a R or O margin around Y and a P around that. Jo frequently experienced a R margin with a dark B or G background, Ma a BK border in a P background, Tr R and B borders and G halos, Sh O or R borders and G halos.

This first period of the image, in which Y predominates, varied in duration from 12 sec. to 2 min. The average length was about 50 sec.

The R image which took the place of the Y varied in the duration of time experienced. The average length is found between 3 sec. and 1 min. 3 sec. R followed Y in all images except in two of Jo's, one of Mo's, and all of Sc's and Ga's. (Note that Sc and Ga reported no R in the images of II.) Only in 15 of the 75 observations was Y imaged within R. Many Os reported that the R came upon Y very suddenly and not gradually, as it did in images of II.

The border around R varied from a dark G or B to P or an indistinguishable dark color. The G border was most frequently

noted, for it appeared in 26 of the images, the *B* in 14, and *BK* and *P* each in 11 images. These borders, which sometimes were so narrow that *O*s called them "margins," gradually increased and enveloped *R*. In all of the 75 observations, except those of Sc and Ga who saw no *R* in the image, *R* remained for a certain length of time in the center of the dark *B*, *G*, or *BK* image. A light halo of *Y*, *G*, or *YG* was always seen around the principal part of the image.

The *R* disappeared from 5 to 60 sec. after it was reduced to a spot in the center of the image. As it vanished, several times *O*s reported that the *B* image changed to *BG* or *G*. Only 3 *O*s, St, Mo, and Mc, noted no change in the *B*. *O*s who had seen *G* or *BK* and no *B* noted no change in it when *R* disappeared. Fr, contrary to other *O*s who seldom experienced *P* in the image, reported that it frequently alternated with *R* and *G*. *P* was the most constant color Fr saw during the last stage of the image. Six *O*s experienced a *BK* image in the last stage, 2 *G*, 1 *B*, and 1, Sc, "an indistinguishable light color." Eight *O*s reported "no halo" during the last stage of the image.

Generally speaking, the stages in the image were constant, and after one stage appeared the colors that had vanished did not reappear. In 14 of the observations of III *R* appeared occasionally to alternate with the *B*, *G*, or *BK* image.

Mo experienced *G* in the third stage of the images of III in place of the *BK* in those of II. The hues in both the central and peripheral regions of the image varied in small degree in the different observations of an *O*.

The following notations relating to the tint and chroma of images were made.

Fr "The *GY* hue first seen was the brightest color in the image. Colors most vivid during the first stages of the image. *Y* was very bright, *P* very dark."

Jo "*R* a luminous color. *G* border is dull and rather gaseous. This fuses into a darker color. The first *G* that came before the *Y* was light."

Ma "Bright *R* grows dark and less saturated. *B* is dark with a pale *G* background."

Mc "*Y* cross bright. First *B* of medium saturation. Dark *G* border and pale *Y* halos."

Sc "*G* background of *Y* cross became the *G* cross which is not so dim as the background. Images brighter than those of II. *G* predominates. Colors get dimmer and *G* darker toward latter part of the image."

St "*R* is a luminous color. *P* a vivid one. *B* grows darker."

Tr "*G* cross is quite dark."

20 of the 55 after-images of IV began with *G*, 8 with *Y*, 6 with an "unnamed light color," 6 with *YG*, 6 with *P*, 2 with *B*, and one with *R*. The two *B* images were seen by the same *O* and the *P* by two *O*s alone. The majority of the images began with *G* but very soon changed to *Y*. In 48 observations *Y*

appeared within 35 sec. from the beginning of the after-images. Ga, however, saw *Y* in only one observation. The colors she experienced were *P*, *PK*, and *G*. Half the *O*s saw no halo upon first appearance of the image. All *O*s, however, reported one or more halos within a few sec. after the image appeared, usually with the change from *G* or *P* to *Y*. In cases where *G* formed the principal part of the image, a *Y* border surrounded it and took its place. A *R* border with *B* and dark borders around it appeared around the *Y* cross. J, Mo, St, and Tr usually noticed more than one border around the *Y* image, *R*, *B* and *L*; *R* and *B*; or *BK* and *P*. Mo, Mc, St, and Tr were the *O*s who visualized one or more halos when the image first appeared.

In the images where *R* formed a border around the *Y*, the former color increased at the expense of the latter, as it did in the images of II and III. In other images *R* appeared suddenly in place of *Y* about 40 sec. after removal of the stimulus. *R* was present in all images except in those of Sc and Ga and in two of Ma's which were of unusually short duration, and remained continuously the principal color in the image for 30 sec. on the average. Only in the images of Fr did *R* alternate quickly with *Y* and *P*. When *R* appeared, *Y* vanished from the center of the images, except in those of Tr and Ma. Sc, who saw no *R* in the images, saw *Y* within the dark *G* cross after the *G* had partially taken its place. Fr usually experienced a *P* border around the *R* image, Mo a *BK* border, Mc, Sh, and Tr a dark *G* border, and Jo, Ma, St, and Ru a dark *B* border.

In turn, the dark *P* border in Fr's images changed into a *B* or *G* image alternating with *P*, the *BK* border Mo saw took the place of the *R*, the dark *G* border reported by Mc, Sh, and Tr formed a dark *G* image, and the *B* border of three images of Jo, Ma, St and Ru formed the dark *B* cross. The light *G* or *Y* halo that appeared around the *R* image occasionally surrounded the dark *B*, *G*, or *BK* image. Half the *O*s reported that this halo vanished about 1 min. before the rest of the image. All *O*s except Mc continued to observe a diminished *R* spot for about 60 sec. after the *R* commenced to give way to the increasing border of *G*, *B*, or *BK*.

This last stage, in which the *O*s saw only one color or one color in the surrounding light, was much longer than other stages in the image, for it lasted approximately 70 sec. *BK* was the last color seen by Mo, Sh, St, and Ga; *B* by Jo, Ma, and Mc; *P* by Fr; *Y* by Sc and Ru; and *G* by Tr. The *B* stage in these images was more sustained than the *G*, and conversely in III.

The following remarks concerning the color qualities of the images of IV are given to supplement the preceding section.

Fr "First *B* very bright. *P* dark but vivid. *G* cross fairly well saturated."

Jo "Colors become duller after the first appearance. First bright *R* grew very dark. *B* was always very dark. *G* halos pale and gaseous, and differ from surface colors on the chart."

Ma "First *Y* very bright and stays same. *R* very bright at first and fairly well saturated. Grows darker. Colors dull. *B* always dark. *G* halo pale."

Mc "Colors duller than those of III."

Mc "Colors not brilliant. First *Y* brightest. All colors below base of color pyramid."

Sc "*Y* image so bright at first that it burns. It fades. *G* very dark."

St "First *G* pretty bright. Other colors dull and unsaturated. Period of most saturation was a short time after they appeared."

Tr "*Y* cross the brightest color. *G* not so vivid as in images of III. More *BK* in image."

Ga "Colors not well saturated. All colors in image equally bright."

Ru "Best saturation in beginning of image. *Y* brightest of any color. *R* best saturated. There seems to be a reverse relationship between the image and its background; for when the principal part of the image is light, the background is dark, and *vice versa*."

It will be noted that all *O*s, with the exception of Fr, Sc, and Ru, described the images after the first period of time as being rather dull, dark, and unsaturated. The first *G* or *Y* seen was reported the brightest color by all *O*s, except by Fr and Ga who did not refer to it.

If we compare the colors in the after-images of one intensity with those of another we find that those of II, III, and IV may be roughly divided into periods, while those of I may not. The images of the latter are usually *B*, *P*, and occasionally *G*, with *R* and *Y* appearing in a few observations. A much larger proportion of the images of II began with *Y* than those of IV, whose after-images usually began with *G*. In II, III, and IV most of the images changed to *Y* within a few sec. after their appearance. The first images of IV had fewer halos than those of II and III. *R*, the color in all cases most often seen around *Y*, came in upon *Y* more gradually in II than in III, and it usually came very quickly in IV. Ma and Tr, the only *O*s who continued to see *Y* within the *R* cross in IV, were two of the *O*s, Ma, Mc, and Tr, who continued seeing it after exposure to II. In the images of III there are twice as many appearances of *G* borders as *B* borders around the *R* image. In IV the *B* outnumber the *G* borders. Many *O*s who saw a dark *B* image after the *R* in the flight of colors following one stimulus saw the dark *B* following other stimuli. Ma, Mc, and Jo usually reported *B*, Mo and St *BK*, Tr *G*, and Sc light *G* in the last stage of the image, regardless whether it proceeded from II, III, or IV. While only 3 *O*s experienced halos in the last stage of the image of II and III, 5 *O*s experienced them after IV.

In all observations the images were reported as having a tendency to grow darker, those of I as well as those of II, III, and IV. Many *O*s noted particularly that images of IV became very unsaturated after a relatively short period of time. This circumstance was not noted to such an extent by *O*s after less intense stimulation. The chart-references to the images of III and IV closely correspond, and in a measure we may conclude that the qualitative characteristics of images of III do not differ from those of IV.

Rise and Subsequent Temporal Course of the Images

The first appearance of the image following the various intensities of light was reported as quickly as possible to *E*. For instance, the average duration of Mc's images following I is 2 min. 40 sec., II 3 min. 30 sec., III 4 min. 30 sec., IV 5 min. 15 sec. Sc's images of I, II, III, IV likewise were of longer duration than the average *O*'s. St's observations, on the other hand, include comparatively short images following I, II, III, and IV.

From the reports concerning the rise and subsequent temporal course of the image we conclude that, generally, the more intense the stimulus, the more regularly did the images appear within 2 or 3 sec. after removal of the stimulus and the longer did they last. Increasing the stimulus from 75 to 100 w., however, shows corresponding slight change in the time of appearance and no change in the temporal course of the image.

Fluctuations in the After-Images

There were about the same number of fluctuations in the images of one *O* as in those of another. The number of fluctuations per image following I is 1.24, II 2.1, III 1.13, IV 1.55. The brevity of the images of I partially accounts for the relatively few fluctuations; therefore we may conclude that there are more fluctuations per min. after exposure to the less than to the more intense stimuli. The greatest number of fluctuations appeared in the latter part of the images. The majority of fluctuations in I came between 3 sec. and 1 min. 30 sec., in II 1 and 2 min.; in III 2 min. and 2 min. 30 sec., and in IV 2 min. and 2 min. 30 sec. We noted that the length of the images of III and IV was the same.

As there were many fluctuations during the latter stage of the image, there were few fluctuations in the early part of it. In the images of II, III, and IV the fewest fluctuations occurred during the first 30 sec., for there were only 6 fluctuations in the 185 images. In I, 14 fluctuations occurred during the first 30 sec. of the 59 images. Six of these, however, came after 20 sec.

We have so far been considering the disappearance and appearance of the whole image, not of one particular part of it. Following III and IV we find more changes within the image reported than in those following less intense stimulation. For instance after III and IV several *O*s, especially Fr, noted that *R* would go and come but "the dark around it would not disappear." We may assume, then, that many fluctuations of the whole image mean few internal changes.

There were very few fluctuations in the size and shape of the image. Seldom, if ever, was the image reported as changing back to a cross after it had shrunk to a diamond. When unusually shaped images were experienced, *e.g.*, the cross with several bars, they were reported as fluctuating.

Size and Shape of the After-Images

The general progression in the shape of the image was that from a cross, through a diamond, to a spot. This procedure was by no means constant; for in some observations the diamond stage was omitted, in others a spot or halo preceded the first imaged cross, and in a few cases no cross was seen in the image. After I the images of 8 *O*s commenced as a spot which stayed for several sec. before a cross appeared. Half the *O*s reported that the image vanished when it was still in the cross-stage, the others that after 30 sec. the cross disappeared in a spot.

Eight of the 11 *O*s reported a cross when the image following II first appeared. Ga's images, regardless of intensity of stimulation, all began with one or more spots in which the cross did not at first appear.[20] The other two *O*s, Ma and St, reported that the cross formed in the halo immediately. The cross stage in II lasted about 2 min. usually, when 3 *O*s reported "just a spot" and the other 8 a "diamond." These *O*s saw the diamond-shaped image for 30 or 35 sec. when it vanished, leaving a spot in its place. In contrast to the usual appearance of the diamond-stage in II, which indicates a shrinking of the cross, no diamond was reported in any images of I, in which the cross seemed to vanish quickly into the halo.

After exposure to III, 3 *O*s said the cross formed 1 sec. after the halo appeared. Other *O*s reported the imaged cross first. All *O*s, except Jo and Ga, in whose images the cross did not shrink, reported the diamond in place of the cross at 2 min. 15 sec., or 2 min. 45 sec. All *O*s reported that the diamond disappeared in the spot or halo after it had been visible 30 sec. or more.

In IV 7 *O*s saw the halo before the cross. St alone imaged the cross throughout the flight of colors. Other *O*s' images

[20]Unless otherwise indicated, the shape of the imaged cross is the shape of the perceived stimulus.

changed from a cross to a diamond after 3 min. duration. The diamond-stage varied in length from 30 sec. to 1 min., when it vanished into a spot.

Summarizing the above reports on the shape of the image, we conclude that the cross is that most constantly experienced. All *O*s except Ga saw a cross during a part of each image. There is also a relation between the intensity of stimulation and the shape of the image. A halo appeared before the cross in many images of I and IV, but in few of II and III. The diamond-shaped image was absent in I, but usually present in II, III, and IV. The greater the intensity of stimulation, the longer did the image remain cross-shaped. In I the average length of time the cross was imaged is 30 sec., in II 2 min., in III 2 min. 30 sec., in IV 3 min. As color-changes have been found to take place more slowly in long observations than in short ones, so did changes in shape take place more slowly in images of long duration. Ga's images were different from those of other *O*s; for many times she did not see the cross at all, but only reported "spots." Several times, after she had seen the cross for a short time, not more than 30 sec., she declared that it suddenly tipped over like an "x."

In the images following I, all *O*s thought the cross to be of the same size as the perception except Tr, who reported it as three-fourths the perception and St, who sometimes reported it to be of the size of, and sometimes smaller than, the perception. The cross did not have time to alter in size before it vanished, and consequently remained constant in size. The halo around the imaged cross was described by some *O*s as being of the size of the halo around the perceived cross and by others as much larger at first; and it was invariably reported as growing smaller when the cross disappeared within it.

Eight *O*s thought the images of II were smaller than the perception and that they continued to get smaller. No mention of the size of the halos was made. The images of III were usually smaller or "a trifle smaller" than the perceived cross. Mc, Mo and St, however, thought that they were of the size of the perception at first. All *O*s except Sc described the image as "getting smaller before it disappears". In regard to IV, the image was usually reported as being one-half or two-thirds the size of the perception, and as diminishing in size as time went on. Sc and Ga, however, imaged the cross as of the size of the perception, and Mo described it as "fatter than the perception."

While most of the *O*s imaged the cross following I as of the same size as the perception and as remaining so, they reported that the after-images following other stimuli were smaller and became still smaller when they changed from the cross to the

diamond. The change in the size seems to be due to the change in shape of the image. We may conclude that the size of the image of II is the same as that of III and IV but smaller than that of I. These sizes may have been influenced by the supposed distances at which they were seen away from *O*.

Position of After-Images

The image was usually reported to be either at the same distance as the perceived cross or a little farther away. Several *Os* thought the image seemed to move away from them as it grew smaller. Others reported that it moved in a plane at a fixed distance from their eyes. This latter kind of movement, which was swifter and more constant than the former, was partially under the *Os*' control, who could change the position of the images if they brought their gaze back to a certain fixation. Six *Os* who referred to the distance of the image following I reported it to be that of the fixated cross. Only one reported it as nearer than the cross.

Ma "About the distance of perception. Hard to place."

Mo "Same distance as cross or perhaps a little farther away."

Sc "Same position as the cross. Spot gets farther away at the end when it becomes smaller."

St "Imaged cross a little further away than the perceived cross. Seems somewhat to the right of F."

Ga said the images of II were all further down and nearer to her nose than the perceived cross. Other *Os* reported the image to be in the same position as the cross except when it moved around.

Movement of the Images

The movement of the whole image up and down and from one side to another was dependent to a considerable degree upon the attention of *Os*, and because of this fact movement is not an important characteristic of the image.

The movement of colors within the image has been noted by other experimenters as progressing from the periphery toward the center.[21] In the images following I the colors did not seem to move in any direction. When other stimuli were used, *Os* very often reported that "the colors seemed to go in toward the center." This phenomenon was already indicated in this experiment when the hues of the after-image were discussed. In the greater number of observations the hues went in regular order from the edge into the center, where they grew so small that they disappeared. The colors early experienced in the image were reported as moving faster than those in the latter part of the image. During the first min. of the series, *O* found that she had hardly time enough to tell everything she saw.

[21]Berry, *Op. cit.*, 334.

Throbs and waves in the colors of the image were also noted. Tr and Ru especially described a sort of rhythm in the color when the image became dark *G* or *B*.

Tr "The cross is dark *G* with light *G* ends, short and pointed. There is a continual movement of the dark *G* into the light *G* and back again."

Durational Series

Hue, Tint, and Chroma of the Images

Altogether 212 observations of II were recorded with the 5 durations of exposure. As this number of observations is twice that obtained from I, III, and IV, we shall emphasize the durational experiments with II.

The colors of the images of II *a*[22] are similar to those of I which were discussed in the intensive series. *G*, *P*, *B*, and *Y* were the hues reported most frequently; *G* was reported by 4 *O*s and each of the other hues by 3 *O*s. *R* was not once imaged after II *a*. Gray was often observed as a background around the hue or as a late stage in the series imaged. Six *O*s reported *g*. St was the only *O* who did not experience color in the after-images following the II *a*. Only three *O*s, however, experienced more than one hue in a particular after-image. Except in those images where the hues became dark in tint or poor in saturation the image was observed as continuing unchanged to the end.

With the exception of St, all *O*s visualized two colors at some time in the after-image of II *b*. Ru, however, was the only *O* who experienced two colors at the same time; and if halos were reported they were always light or dark *g* with no coloration. We noted that St, who saw only one color in the images of *b*, saw no color in those of *a*. There is little agreement as to the hue itself. *G*, *R*, *P*, and *B* were experienced frequently. *R*, which was not experienced in II *a*, was always included in the images of II *b*, though at no fixed period. We found *R* usually in the middle stage of the images of II *c* and never at the end and seldom at the beginning. Many times *B* or *G* was experienced in the form of a margin or halo around *R*. And from II *c*, all except two *O*s reported one halo of definite hue, *P*, *G*, or *Y*, as being around the main part of the image during some period of time—usually the middle.

The images of II *d* have already been described in the intensive series. We found not only that *R* had a definite period of occurrence, but also that other primary hues, *Y*, *G*, and *B*, appeared at regular intervals. All *O*s except Ga and Jo saw *Y* or *YO* in the first stage of the image, *R* in the second, then *G* or *B* which became dark or black. *O*s reported one or two halos

[22]The symbols *a*, *b*, *c*, *d*, *e*, *f*, designate respectively the durations of exposure 5, 10, 20, 40, 60 sec. and 1 min. 30 sec.

in the beginning stage of the image, and more than half the *O*s experienced a light halo during the last period of the image. These halos and margins invariably were reported as going into the center, enveloping the main part of the image, and increasing until the center previously seen had disappeared. At the same time another margin took the place of the previous one. In the images of II *e*, *R* was experienced by all *O*s except Ga and St, *G* by all except Mo who saw *BK* in place of it, and *B* by all except 4 *O*s, Sc, Mo, Sh, and Tr. Gray was not present in these images. Although the periods of the colors were longer in *e* than in *d*, nevertheless the hues differed little. The main differences in the hues of *d* and *e* were these, *viz.*, following *e*, *G* was reported by 4 *O*s before *Y*, and after *d* only one *O* reported a *G* image before *Y*; and the last stage of the image of *e* was reported as having no halo, while the last stage of the image of *d* was usually described as surrounded by a halo.

The images of II *f* closely resemble these of II *d* and *e* in hue. *O*s who experienced *G* rather than *B* in the images of *e* did likewise in *f*. The stage in which *R* was the prominent hue was about 1 min. 30 sec. in both the images of *e* and in *f*. The last period of the image was 1 min. longer in *f* than in *e*.

Concerning the tint and chroma of the images just described, it has been noted that *g* was reported as being the most prominent quality following II *a*. As the duration of the stimulus was increased, the *g* was reported less frequently and after II *d* it was not reported at all. No hues in II *a* and few in *b* and *c* were reported as being saturated, and practically all the hues in the images of *c*, *d*, *e*, and *f* were well saturated; so we may conclude that, the longer the exposure to stimulation, the better saturated were the images following it.

The tint of the image did not change as much as the chroma when the duration of stimulation was altered. In all images some colors were light and some dark. All had the tendency to grow darker; *e. g.*, *R* which at first was well saturated and of medium tint became dark and poorly saturated, and *G* and *B* often were reported as dark and growing darker.

Mo "Colors very unsaturated and vague. I seem to be seeing it all through a thin gray veil." II *b*.

Jo "Pale background. Colors unsaturated. *B* is a pastel grayish color." II *b*.

If we contrast the hues of images of I *a* and IV *f*, the extremes in intensity and duration, the following facts emerge. Six of 10 *O*s saw no color at any time during the images of I *a*. The other 4 *O*s reported either light *Y* or *V* or dark *B* or *P*. The hues of the images of IV *f* appeared to be like those of IV *d* discussed in connection with the intensive series. The first

colors were very bright and light, and the others were reduced in brightness and lightness.

Rise and Subsequent Temporal Course of Images

After a 5 sec. exposure to any intensity of stimulus, a greater proportion of images appeared in 0.5 or 1 sec. than after any other length of exposure. These images, appearing as soon as the stimulus was removed, were always reported as "positive after-images." The "negative" images were observed as appearing at irregular intervals from 1 to 8 sec. after the "positive" had passed away. *O*s noted that, the longer the exposure to the stimulus, the less frequently did the positive after-images appear and the more regularly did the flight of colors arise within the first 4 sec.

There is a decided correlation between the duration of exposure to the stimulus and the duration of the image which follows it; for the longer the exposure, the longer was the image experienced. The exceptions to this general rule were the temporal courses of the images of *e* and *f* which differed but slightly from one another, although there was a difference of 30 sec. between the lengths of exposure. When we increased the intensity of stimulation from III to IV or the duration of exposure from *e* to *f* we found proportionally slight increases in the length of the after-image.

Fluctuations in the Images

Fluctuations were found to vary according to the duration of stimulation; for the fewest number occurred in the images of *a* and the most in those of *e* and *f*. As the images of *d*, *e*, and *f* are longer than those of *a*, *b*, and *c*, it follows that, the longer the image, the greater was the number of fluctuations. The shorter the fixation period, the earlier did the fluctuations appear. In *a*, *b*, and *c* there appeared one or more fluctuations before 10 sec.; and in *d*, *e*, and *f* no fluctuations took place in the image before 12 sec. Some *O*s, particularly Fr and Sh, always reported more changes in the images than others, some of whom seldom noted them, regardless of the duration or the intensity of stimulation.

Size and Shape of the Images

As a general rule we find that, the longer the stimulation, the greater was the proportion of cross-shaped images in the first period of the flight of colors and the longer did the images remain cross-shaped. Following longer durations of exposure, *i.e.*, *d*, *e*, and *f*, the images usually began in the shape of a cross, remained as such during the greater period of the image, shrank until they became diamond-shaped, and then became spot-shaped, and stayed so during the last few sec. of the image.

Following the 3 greatest intensities of stimulation, *i.e.*, II, III, and IV, the images changed from a cross to a diamond and then to a spot more abruptly in *d* than in *e* or *f* in which the change was gradual. No diamond-shaped stages were reported in the images of I or in *a* and *b* of II, III, and IV, and few in *c*.

The imaged cross was usually reported as smaller than the perceived cross regardless of the length of exposure to stimulation. Seldom did the images from II *a*, II *b*, and II *c* diminish in size during their course, while the large proportion of those of II *d*, II *e*, and II *f* grew smaller during the latter stages. These facts indicate that the image became smaller as it changed its shape, but that it did not usually become smaller, *e.g.*, in *a*, *b*, and *c*, when the image remained a cross throughout its course. Likewise following I *e* and *f* the majority of *O*s thought that the imaged cross did not noticeably alter in size.

Position of the Images

The position of the image relative to *O* and to the stimulus has not been adequately determined for two reasons. Too few *O*s described the position of the image; and some of those that did describe it thought that it did not seem to have an exact location. The varying durations of stimulation had no effect upon the position of the image; for it was usually noted as being at the same distance as the perception, when its position was noted at all.

Movement

The colors in the images following short periods of exposure did not move in any direction. As the exposure increased, movement was reported usually as taking place from the periphery to the center of the image. The only statements of *O*s that indicate any movement in the opposite direction are those concerning the disappearance of the image, when "the image became diffused and faded into the background."

The *O*s did not stress the movement of the whole image after any exposure. Only one movement was reported in all the images of II *a*, one in II *b*, none in II *c*, and 4 or 5 in all the images of *d*, *e*, and *f*. *O* believed that this movement of the image which occurred occasionally was dependent on her own eye-movements rather than due to the effects of stimulation.

Summary and Conclusion

An examination of the various characteristics of the image, as noted in the results of the intensive and durational series, has proved them comparable in many respects.

Fewer hues of poor chroma and of darker tint were reported in images following the shortest exposure to any intensity than

in those following medium exposure to the least intense stimuli. *R*, which was absent in the images of I and *a*, was observed in other images by all except 2 *O*s, and was more regular in its rise and fall than either *P* or *B* of the preceding weakest and shortest exposures. The images following longest exposure to I and those of medium duration of II, III, and IV, were divided into periods of *Y*, *R*, *B*, *G*, and sometimes *BK*. With longer exposures or more intense stimuli, a *G* or *YG* stage preceded the first *Y* stage, but gave way to it in a few sec. The stages of color became longer with the increased duration of exposure, and the *R*-stage became longer proportionally than did the other color periods.

After the intensity equalled III or the duration *d*, the hues, tints, and chroma of the image did not alter with increases in intensity or duration of stimulation.

The after-image usually appeared about 3 sec. after the removal of the stimulus, but it varied both with the intensity and duration of the stimulus. Positive after-images appeared frequently after brief exposures; but the images which followed them were correspondingly irregular in their appearance. The images following longer exposures came regularly but not immediately.

There is considerable consistency in the reports of an individual *O*. If her images were longer than those of the average *O* following a certain intensity and duration, they usually were longer following other intensities and durations. In the observations of all *O*s long images followed both long and intense stimulations. When the intensity of stimulation, however, was increased from III to IV or from *e* to *f*, the maximum intensities and durations used, the increase in the lengths of images was proportionally small. The size and shape of the image likewise varied according to different exposures. The diamond-stage was not present in the images of I, and in those of *a* and *b*. The longer or the more intense the stimulus became, the more gradual was the change from the cross through the diamond to the spot. The image became progressively smaller as the shape changed. In the images of I and in those of *a*, *b*, and *c*, there was no decrease in the size of the cross, just as there was no change in the shape of it.

The longer the stimulation, the greater was the number of fluctuations in an image, but a few *O*s experienced many fluctuations in the image regardless of the intensity or duration of the stimulus. In the images of *a* and in those of I, the fluctuations appeared earlier than in those of *b*, *c*, *d*, *e*, *f*, or II, III, IV. The greater number of fluctuations appeared in the latter stages of the image.

Neither the intensity of the stimulus nor the duration of fixation affected the position of the image. The position was reported as at about the same distance as the stimulus, but often it did not seem to have any particular location.

In the images resulting from short exposure there was no movement of the colors from the periphery to the center. A continuous movement to the center occurred when the duration and intensity of the stimulus were increased to *d* and to II. As the intensity and duration increased, the more rapidly did *R* surround *Y*. The colors following *R* did not advance with such rapidity.

The movement of the whole image was considered as of minor importance. Sometimes, when the image suddenly became smaller, it was reported as farther away.

In general, our results seem to verify the statements of Titchener and Washburn that the images of practised *O*s show fairly close agreement in the characteristics of the image. For the most part there is agreement among *O*s concerning all the factors we have described: the hue, tint, and chroma of the image; its rise and subsequent temporal course; its size and shape; and its fluctuations.

(1) The hues of the images following weakest and shortest stimulations, *i.e.*, 25 w., and 5, 10, and 20 sec., are usually *B* and *P* and occasionally *G*, while those following other stimulations are *Y*, *R*, *B*, and *G*, in spectral order, with the infrequent additions of *P*, *O*, and *BK*. All images have a tendency to grow darker and less saturated before they disappear.

(2) After exposures of slight intensity and brief duration, *i.e.*, 25 w., and 5, 10, and 20 sec., the images usually take their rise the sec. after removal of the stimulus, while after other exposures the images very often appear 3 sec. after exposure. The subsequent temporal course varies with the intensity and duration of stimulation. The average length of the images of 25 w. is 1 min. 10 sec., 50 w., 2 min. 50 sec., 75 w., 3 mi. 35 sec., and 100 w., 3 min. 35 sec. By increasing the stimulus from 75 to 100 w., or the duration of exposure from 60 sec. to 1 min. 30 sec., we find no corresponding increase in the length of the images.

(3) The images usually begin in the shape of a cross, remain as such during the greater part of their course, shrink until they become diamond-shaped, then shrink to a spot and finally disappear. The diamond-shaped stage is usually omitted after brief exposures and those of weak intensity.

(4) As a rule, the imaged cross appears to be of the size of, or a little smaller than, the perceived cross.

(5) The period of fluctuation occurs in the latter part of the image; and usually, the longer the duration of the image, the later does it occur.

(6) Movement and position play an unimportant part in the flight of colors.

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PSYCHO-ANAESTHESIA

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I.

By psycho-anaesthesia is here meant the philosophic tendency to ignore other people while admitting their distinction from oneself. Since this distinction concerns different minds, the term approximately follows the pattern of the compounds "*color-blindness*" and "*tone-deafness.*" Like the words "color" and "tone" the prefix "psycho" names both the things reduced to one—in the present instance inconsistently.

The memory of anything involves a belief in the past occurrence of an event not happening at present, that is, in motion from future to past. If the verification of a belief is taken to consist in seeking the happening of what is believed in, this belief is unverifiable, since the past cannot happen again.

There is a like belief in the present occurrence of events not at present actually happening. This belief is likewise unverifiable, since the present becomes the past when its actual happening is sought. Of these events it may be believed that some are actually happening in a new sense, differing *toto caelo* from the first. In this new sense actual happening is plural. Events may be actual occurrences for, at, to, or in one unit, and not for, at, to, or in some other. Events that actually happen in the first sense together make up one of the units of actual happening in the second sense. This unit is called "oneself," or "I," the remaining units of the plurality believed in being "other people" or "others;" and both together being called "we." Events believed to happen though not actual in the first sense may be called "assumed" events, and the word "actual" restricted accordingly.

It would appear a logical solecism to conceive that the ultimate source of the belief in "others" is an analogy drawn between one's own body and another like it. The argument may be analyzed as follows. In the midst of the mass of events actual at any moment a group may occur which more or less resembles another more persistent group called my body. This first group, or like living body, taken together with other events actual at the moment (*e.g.*, a blow on the other body) brings up a memory of my body in a similar situation, with memories of certain accompanying events (*e.g.*, a sensation of touch or pain). With such accompanying events the resemblance be-

tween the memory and the events that excite it ceases. Only the memory is actual. Nothing like it occurs, and if anything more is to be believed in, it must be assumed. Such an assumption is that of events in another unit of happening, a unique conception which proves nevertheless compatible with a likeness between the events assumed and the events remembered. This likeness analogy teaches, basing itself upon the conception of "others" which it is asserted to create. The supposed argument from analogy is one of the many disguises worn by *petitio principii.*[1]

The doctrine in which the assumption is not made is called Solipsism. Unless the conception of self is not a part of the conception of plurality of happening this term would appear internally contradictory. It means in this case that all events belong to a single self, the conception of which involves that of other units of happening, that is, does not include all events. Whether the doctrine itself is internally inconsistent is a different question.

Assuming plurality of happening, such terms as "I," "others," "we" and "person" or "people" receive unequivocal meanings. "I" is all actual events, or events occurring in the unit "me;" "others" are all events assumed to occur in certain other units, or "persons" believed in; "we" the sum of both. For convenience, an event occurring in a certain unit may be spoken of as

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[1]The Editor has kindly referred the writer to Th. Lipps' *Leitfaden der Psychologie* (1906, 34f.) for another refutation of the analogical argument and for a brief statement of Lipps' theory of the origin of the belief in "others" through Empathy or Self-objectivation. Lipps writes: "What self-objectivation means is this: It happens through an ultimate instinct that in grasping by the senses certain processes and events . . . a vital activity, feeling, volition, or other, awakens in the percipient so strongly as to make a single conscious experience with the act of perception. In that case, although it arises in the percipient, it becomes for his consciousness bound up with the object perceived, that is, resides, or is objectified in it . . . In short, other persons are the result of a multiplication of myself." To this doctrine it may be replied that no process whatever, objectivation or other, applied to the self would make another self out of it; no multiplication of a thing would make the multiples stand alone. Yet "others" do in fact stand alone to our belief, and inexplicably as Lipps observes: "It should be added, as a fact of this objectivation, that objectified experiences and unities of them, in becoming objectified, present themselves, again in inexplicable fashion, as something independent of the consciousness, that is, as something that would exist even when I had no consciousness of it." To the writer this empathic account of our belief in "others" seems but a still further disguise worn by that arch-deceiver, *petitio principii.* It is true that, in a field like this, the power of language to conceal thought, both one's own and others', shows at its best, and disagreement is to be expressed with all reserve.

an event "experienced" by that "person", or happening in "his experience."

Accepting these terms with these meanings, I may believe of a given event that it might happen in the experience of given people and no others. Or, more explicitly, since no individual event recurs, that examples of a given class of events might so happen. Or, I may believe of a given generic event in the experience of a given person that it could not happen in the experience of any other.

Events may accordingly be grouped into classes containing such as can happen in this or that person's experience only, or in the experience of a given selection of persons only, or in the experience of all the people believed in.

People may in turn be grouped into classes containing all the persons who can have experiences of a certain kind. These classes of persons may include, exclude or intersect one another in various ways. If any person can have experiences that no one else can have, that person alone will constitute one of the classes. If any experiences are such as can be shared by all the persons believed in, there will be one class including all persons. Classes containing more than one person may be spoken of as "pluralities."

Still another belief about present happening is possible; namely, that events may be happening without happening in the experience of any one. Like the belief in "others" this belief is unverifiable; but it has a logical standing inferior to that of the belief in "others" in that this belief needs no explanation, while it is difficult to say what is meant by events happening without being experienced by any one.

For example, if it is claimed that the phrase means *events happening without being perceived* it may be argued in reply that these include all events. An event remembered *is* not a part of me as the memory of it is a part of me. Nevertheless I believe it *was* a part of me. Likewise an event perceived may not *be* a part of me as the perception of it *is* a part of me. But unless, against Occam's warning, entities are multiplied beyond necessity, its alien character is the same. It *was* a part of me as the perception of it *is*. Perception, by this reasoning, becomes a case of memory like reflection. *Percipi* is *fuisse*. In so far as an event is perceived it is no longer happening. Accordingly, all events that happen at all happen without being perceived.

Another sense of the phrase *events happening without being experienced by any one* is expressed in a conditional proposition. To say that the other side of the moon happens though no one may experience it means that were conditions other than they are, some one would. But in this sense the phrase does not

mean that events *are* happening without happening in any one's experience, but that events *might* happen in some one's experience.

It would thus appear that a meaning other than either of these two may reasonably be demanded for the difficult phrase. Yet, notwithstanding its difficulty, let the phrase here be used in deference to much philosophical opinion. It will be understood that the phrase means only events which, though they do not at a given time occur in anyone's experience, might at some time occur in some one's experience. Events that could not so occur concern none of us, and are not matter for any discussion between us.

Events may then be happening at any time not only in one's own experience but in that of others, or without being experienced at all. This belief may take either of three forms. All of the events, or none of them, may be such as can happen in pluralities; or some events can happen in pluralities and some in one or other person's experience only. An event may be spoken of as forming part of an outer or an inner world according as it can happen in a plurality or in one person's experience only.

Accepting the first of the three alternative beliefs—that all events can happen in a plurality—there exist, in the sense of happening, no inner worlds. What exists, or happens, is one or more outer worlds. Accepting the second alternative—that no events can happen in any plurality—there exist no outer worlds. What exists is the inner worlds of the persons believed in. Accepting the third alternative—that some events can happen in pluralities and some in one or another person's experience only—there exists at least one outer world, and possibly as many inner worlds as there are persons.

The term mind may be attached in two ways to these conceptions of outer and inner. Every event which can happen in any one's experience may be called mind; or only such as can occur in the experience of one person only.

Taking the wider definition of mind, since every event can happen in some one's experience, mind will include all events. Accepting with it the first alternative belief above stated—that all events are such as can happen in a plurality—all events belong to an outer world as well. Accepting instead the second alternative belief—that every event happens in the experience of one person only—there is no such thing as an outer world. Accepting in turn the third belief—that some events can happen in a plurality and some in one or another person's experience only—outer events and mind interpenetrate; all events are mind and some belong also to an outer world.

Restricting the notion of mind to events which can happen in the experience of one person only; and accepting the first alternative belief, there is no such thing as mind. Accepting instead the second alternative, nothing but minds exist. Accepting in turn the third, outer events and mind together include all events but without interpenetration. They happen side by side, in each unit, or self; or apart from any.

Taking up the most complex of these positions; that is, accepting provisionally the belief that although there are for purposes of discussion no events that *cannot* be experienced by any one, there are events that *are* not experienced by any one; further, taking the definition of mind which restricts it to inner worlds; and finally, accepting the third of the alternative beliefs, according to which some events can happen in pluralities, and some can be experienced by one or another person only; the events of any moment divide themselves for my belief into six mutually exclusive categories.

I. Events actually happening that can happen in a plurality; or outer events of mine.

II. Events actually happening that can be experienced by myself only; or mental events of mine.

III. Events assumed to be experienced by another person that can happen in a plurality; or outer events of that other.

IV. Events assumed to be experienced by another person that can be experienced by him only; or mental events of that other.

V. Events assumed to happen without being experienced by any one, that can happen in a plurality. These might be called "unseen" events of outer worlds. "Unseen" in this use would not have the same meaning as "unperceived."

VI. Events assumed to happen without being experienced by any one, that can be experienced by one person only; or "unseen" mental events of myself or some other or others.

The items of Categories I, III and V—events actually experienced at some moment, or assumed to be experienced by some one or by no one at that moment, that can happen in a plurality—make up the outer worlds of that moment.

The items of Category II—events actually happening at some moment that can be experienced by myself only—make up my inner world or mind at that moment.

The items of Category IV—events assumed to happen at some moment in another's experience that can be experienced by him only—make up the inner world or mind of that person at that moment.

The items of Category VI—events assumed to happen in no one's experience at that moment, that can be experienced by one or another person only—make up the unseen worlds, or minds, of those persons, myself or others, at that moment.

Psycho-anaesthesia, or the philosophic tendency to ignore other people while admitting their distinction from oneself, will

here be interpreted as consisting in confusions and oversights having to do with the first four of these categories.

II.

The saying attributed to the French physician Cabanis—"The brain secretes thought as the liver secretes bile"[2]—is a classical expression of psycho-anaesthesia. The saying affirms an analogy between two pairs of terms; one, a brain and its thoughts, separated in fancy by the unique distinction between an actual outer event and an assumed mental event, events respectively of Category I and Category IV; the other, the liver and the bile, both in fancy actual outer events, belonging to Category I. When the distinction is recognized, the analogy falls to the ground. If it is overloked, other units of happening are ignored.

The standpoint of B. Russell's recent "Analysis of Mind" is apparently likewise that of psycho-anaesthesia. Like Cabanis, Russell considers two pairs of terms, on the one hand a table and its aspect to a supposed observer, on the other a star and its effect upon a photographic plate. Russell offers the second illustration in place of the first, in order, as he writes (p. 99), to eliminate "an irrelevant psychological suggestion" introduced in the first by the reference to our perceptions. Here again, the members of one pair, the table and its aspect to a supposed observer, are separated in fancy by the unique distinction between an actual event and an assumed event, both in this instance being outer events, that is, belonging to Category I and Category III respectively. The members of the other pair again both belong in fancy to Category I. As before, when the distinction is recognized between the two Categories involved, in this instance I and III, this analogy too falls to the ground. If it is overlooked, "others" are ignored.

The question remains whether Russell's analogy still holds in respect to the positions of the pairs of events. These are visual events. What is called the "position" of any one is the complex of relations of the special kind called spatial which a visual event bears to the rest of the visual manifold of which it

[2]E. B. Titchener has pointed out in this JOURNAL (25, 1914, note 64 to p. 443) that the comparison of the liver and the brain is unjustly attributed to Cabanis. It appears half a century earlier (1775) as a caricature of materialistic doctrine in a letter addressed by Frederick the Great to Voltaire. Yet Cabanis' own language at least comports with the psycho-anaesthetic fallacy, as Titchener makes clear in quoting Cabanis' reference to "la secrétion de la pensée" by the brain. Secretion is a process by which something appears in connection with an organ; but thought does not appear in connection with the brain. It has to be inferred from brain activity. As Titchener remarks, something very different from secretion takes place here.

is a part. The aspect of the table has its assumed position in this sense as the table itself, the star and its effect upon the photographic plate have each its actual position. Is this assumed position of the aspect the head of the observer, just as the actual place of the effect of the star is the photographic plate; or does the analogy break down here also?

This question can be decided only by a reference to the source of the analogical argument made possible by the assumption of plural happening. This source is my visual memory.

Most of the elements of my visual manifold may change from moment to moment; but certain scanty and vague elements, spoken of as belonging to my head, are relatively constant as long as any visual manifold at all is actual—except in the form of a blank, as in closing the eyes. The visual manifold apart from, that is spatially elsewhere than, my head may change within limits throughout. The complex of space-relations of my head to the rest of the visual manifold may be called a "point of view." Although the manifold may change with the exceptions noted, there may still be a similarity between certain of the new elements and certain of the previous elements. If the similarity is considerable, the pairs are spoken of as different "aspects" of the same "things." If the space-relations of my head to the new aspects of any two things differ from its space-relations to their previous aspects, the space-relations of these to one another meanwhile remaining the same, my head is spoken of as having moved into a new point of view with respect to these things.

The conception of a supposed observer of a given table presents such a memory of my head moved into a new point of view. Any new aspect of the thing I call a table retains in this memory the space relations it actually has to any other given thing apart from my head. The assumed aspect has in a word the position of the actual aspect. It does not occur in the head of the supposed observer, as the image of the star occurs on the photographic plate, but in the actual position of the table. Russell's analogy appears to break down in the matter of position also.

In a detailed discussion of this matter ("Our Knowledge of the External World", pp. 90 ff.) Russell reaches a definition of the position of anything in space by conceiving a series of appearances of the thing from different points of view arranged according to the size of the appearances. This series, Russell notes, has a practical limit in that a thing cannot appear larger to us than it does when in contact with the eye. Yet it is evident that the series may be theoretically prolonged by conceiving the equivalent of the entrance of the thing into the eye or head. One of the visual manifolds or "perspectives" obtained

by such a theoretical prolongation of the series of appearances of the thing Russell regards as the position of the thing. This statement is equivalent to defining the position of anything in space as approximately the position which the head of the percipient would occupy were it to approach so near the thing that the thing would no longer be visible. This is obvious. On the other hand, it is not equivalent to saying that the appearance of anything to a percipient is inside his head. This is absurd; since his head is in part also an appearance to him and in a different position from any other. Is not this absurdity, nevertheless, what is asserted in saying, as Russell does (p. 92), that our private visual world—the sum of things we see—is in a sense inside our head?

By means of this argument Russell speaks of the aspect of a given table to a supposed observer as occurring at a "passive" place, as an effect produced in the observer's head by the table which itself occupies an "active" place; the relation between the two being that of the effect of the star on the photographic plate to the star itself, these occupying respectively other "passive" and "active" places. How can this assignment of the aspect to a "passive" place be given a rational meaning?

Though the aspect of a visual thing to me is plainly not in my head, I may, at least in a fashion, locate my assumption of such an aspect to a supposed observer, in his head; and in common speech do so locate his mental operations. Thus the thought or absence of thought of another is popularly located in his "long" or "empty" head. If then my assumption of an aspect of a visual thing to another—my thought or fancy of it—be mistaken in reflection for the aspect itself—for the appearance thought or fancied,— as the memory of an event may be mistaken in reflection for the event itself, the analogy holds. Such a confusion is an instance of a subtler psycho-anaesthesia in which Category II is confused with Category III; a mental event consisting in my thought or fancy of the aspect of the table to a supposed observer is confused with an outer event consisting in that aspect itself.

This subtler psycho-anaesthesia continues to ignore "others." They remain in so far a name for our thoughts of them. To such an unequal standing they appear assigned in Russell's broad differentiation between physics and psychology. "Broadly speaking", he writes ("The Analysis of Mind", p. 307), "physics groups particulars by their active places, psychology by their passive places." For if it is not the aspects of visual things to "others" but only my thoughts of these aspects that can be conceived to have passive places at all, this psychology in so far robs "others" of any distinction from oneself. Russell ex-

presses elsewhere ("Mysticism and Logic", p. 157) the great satisfaction it would give him to do this; but in the "Analysis" proposes (p. 133) to "continue yielding to the prejudice" that "things exist outside my own biography." These assumed events in the biography of others Russell nevertheless reasons about as if they were actual events in his own.

Another psychology, that of Behaviorism, appears to fall short of giving full recognition to the first four categories, in part by a denial of fact, in part by a restriction of method. The events of Category II, that is, of my mind, are resolved by Behaviorism into "implicit behavior"[3] ("thinking" is chiefly sub-vocal talking), events of Category I; and in consequence the events of Category IV into those of Category III. These latter are then omitted as objects of study altogether, the doctrine explicitly restricting the field of psychological inquiry to conduct, animal or human, that is, to happenings in living bodies, events of Category I.[4] "Others" become for Behaviorism their conduct, which is part of my outer world. Nevertheless, Behaviorism apparently recognizes that events of Category III may happen. But since any that do happen fall outside the limits of experimental investigation, they are excluded on principle. Behaviorism thus appears to involve a type of psycho-anaesthesia that in part denies and in part ignores the existence of other people as distinguished from oneself.

These are two currents in a tide of psycho-anaesthesia otherwise astir in the pragmatistic and realistic philosophies of the past and present generations.

Pragmatism, strictly interpreted as the doctrine that all and only such propositions as are verifiable are true, implies a denial of the distinction between others and oneself. The proposition, There are various units of happening, is unverifiable, therefore untrue. Or, if Pragmatism be considered a doctrine of the meaning of propositions, to the effect that no proposition neither verifiable nor falsifiable has any meaning, the assertion of plural happening, according to the strict pragmatist, has no meaning. It is not its assailant, so the strict pragmatist might contend, who is afflicted with psycho-anaesthesia,—seeing but one thing where there are more than one,—but its upholder who is afflicted with psycho-strabismus,—seeing more than one thing where there is but one. Such an opinion can certainly not be adhered to; and that it was ever entertained is symptomatic of psycho-anaesthesia.

[3] J. B. Watson, *Behaviorism: an Introduction to Comparative Psychology*, 1914, 19.

[4] *Ibid.*, 1.

The tenet stated first in the platforms of the New Realists is the doctrine that "things known may continue to exist unaltered when not known."[5] Critical Realism, as its name imports, materially modifies this doctrine without abandoning it.[6] In these philosophies the conception here interpreted as that of happening without happening in any unit becomes of foremost concern. Categories V and VI, which together represent it in the present essay, accordingly take precedence over those based on the distinction between other people and oneself. The centre of gravity of philosophic thought moves away from the distinction while still feeling its influence. A sense little used is apt to atrophy; and this would be the fate of the philosopher's sense of personality, were the balance not righted in his further inquiry. The New Realist admits that his doctrine must at first take the form of a protest. "It is perhaps unavoidable, that the new realism must for a time remain polemical in tone."[7] In fact the doctrine itself is a polemic, protestant and not catholic in nature. In so far as a defense of something unrecognized, it demands to be completed by a statement of what there is to recognize. Things sometimes known and sometimes unknown, with any data that constitute the knowledge of them, are not all that may be believed in. Various units of knowing may also be believed in. To apply again the term of the present essay, it would appear that, while realistic doctrine is not the product of psycho-anaesthesia, the realistic mood has an inherent tendency to produce it.

But the tide of psycho-anaesthesia now flowing in philosophy has its chief source without doubt in the imposing modern growth of experimental science. Experiment is verification. Plural happening is unverifiable. Hence either my belief in "others" must be abandoned, or it must be admitted that experimental science has not everything that happens for its province. It is not surprising that at a moment when the word "unscientific" is a term of reproach, an adjective for the nugatory and that which merits no attention, the philosophic belief in other people should waver and suffer eclipse. The psycho-anaesthesia of the present scientific age is like the visual anaesthesia engendered by a brilliant light that for the time obscures other things which nevertheless are there.

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[5]E. B. Holt and others, *The New Realism; Coöperative Studies in Philosophy*, 1912, 474.

[6]D. Drake and others, *Essays in Critical Realism; a Coöperative Study of the Problem of Knowledge*, 1920, 102. "Now it is true that critical realism shares with Locke the doctrine that one's data and the things themselves—such as physical objects and other people's experiences—are not identical."

[7]*The New Realism*, 1.

A CASE OF ACHROMASIA WITH COMPLICATIONS

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It is not frequent, in elementary psychology classes, that one runs upon persons of an unusual type or upon cases deviating greatly from the normal and expected. Especially is this condition true in respect to the matter of color-vision. But the case to be described in this paper happens to be unusual; and the facts concerning it will possibly be of interest and value to persons other than the author; we shall present them for whatever worth they may contain.

Mr. X is a student, about twenty years of age. In ordinary contact with him, one observes that he wears glasses and that these have uncommonly thick lenses. One observes also that, even with glasses, he places ordinary printed matter nearer the eyes than does the normal person in a similar act of reading. If one is still observant of X, one discovers that his eyes are diverged from the usual parallel position, the right turning temporally and downward, and that the left eye bulges somewhat from its socket. Such, then, is the *S* upon whom several tests have been directed; and many of these tests have been in the nature of exploratory or side-issue tests to enable, if possible, a better understanding of his defective color-vision. These collateral tests have exhibited complications which accompany the *S*'s inability to discriminate between hues or chromas. We shall present the facts of his color-blindness first, and then later we shall take a glance at the complicating conditions.

The large set of Holmgren skeins was used in the experiment to test the *S*'s color-blindness. The full set (*minus* those skeins whose numbers are lacking from Table I) was placed on a large grey cardboard on the top of a table. The *S* was comfortably seated at the table, which was in the center of a room uniformly lighted by early afternoon spring sunlight through 6 windows at his right. He removed his glasses. No color being mentioned to him, he was told to sort into a pile all skeins which seemed alike in color or hue if he could do so (of course the word 'hue' was meaningless to him, as will be apparent presently). He was allowed all the time he desired; also he was wholly free to move the skeins as near his eyes as he cared to do. Under such conditions, there resulted from his sortings, discriminations, re-sortings, etc., the following groupings (Table I), each containing those skeins which 'looked alike' to him.

TABLE I

Assortment I
- dark green (poorly saturated) (15)
- dark (blue) green (59)
- dark green (with slight trace of yellow) (13)
- dark, dull blue (74)
- medium rose (blue evident) (21)
- very light pink (27)
- red orange (37)
- (brownish) red orange (39)
- light bluish green (77)
- brownish yellow (S)
- dark green (45)
- medium violet (90)
- medium reddish brown (12)
- dark green (slight trace of yellow) (67)

Assortment II
- standard red (31)
- deep, brownish red (95)
- very dark red (35)
- dark red (29)

Assortment III
- very light yellow (almost white) (R)

Assortment IV
- light yellow (Z)
- very light pink (81)
- very light blue (26)

Assortment V
- dark, yellowish brown (34)
- deep violet (92)
- reddish brown (N)
- very dark green (slight trace of yellow) (69)
- very dark green (19)
- dark blue green (55)
- very dark green (53)
- very dark (blue) green (51)
- dark yellow brown (36)
- medium rose (near red) (33)
- dark blue (trace of violet) (78)
- tannish brown (D)
- reddish rose (89)
- medium tan brown (A)
- light rose (75)

Assortment VI
- red yellow (U)
- light blue (72)
- yellow (trace or orange) (Y)
- yellow green (43)
- medium green (slight trace of yellow) (97)
- yellow tan (64)

Assortment VII
- very light violet (80)
- light pink (71)
- light tan (52)
- blue green (very nearly a blue) (3)
- light violet (84)
- light tan (50)

Assortment VIII
- medium red brown (C)
- dark brown (48)
- dark orange brown (66)
- dark tannish brown (B)

Assortment IX
- dark rose (blue evident) (23)
- very deep rose (91)
- very dark brown (P)
- very deep rose (some tinge of red) (93)
- very dark brown (68)

Assortment X
- dark blue green (63)
- medium violet (some tinge of red) (86)
- saturated blue (76)
- rose (blue evident) (25)
- yellow tan (K)

Assortment XI
- red (103)
- very dark red brown (100)

Assortment XII
- darkish yellow green (49)
- saturated blue (24)
- deep pink (73)
- deep pink (touch of red) (87)

Assortment XIII
- yellow green (65)
- blue green (lighter than 'Jade Green') (57)
- medium pink (85)

Assortment XIV
- deep yellow (O)
- darkish blue green (11)

In addition to sorting the skeins, the *S* was asked to select from a book of grey papers (containing 50 shades of Hering grey, published by C. H. Stoelting Co.) that sheet of grey which matched each of the 14 assortments of skeins which he had just made. This he did very quickly and easily. Assortment I he matched with grey no. 37; II, with a grey darker than the darkest (50) contained in the booklet; III he matched with grey no. 6; IV, with no. 3; V, with no. 41; VI, with no. 13; VII, with no. 7; VIII, with no. 44; IX, with no. 50 (stating that it was not a perfect match); X, with no. 27; XI, with a grey very much like no. 50; XII, with no. 26; XIII, with no. 19; and XIV, with no. 19. If we arrange these Arabic numerals, for the greys, in an ascending series, from the light greys to the darker greys, we get this sequence of the brightness-values of the assortments of skeins as arranged by this color-blind *S*: IV, III, VII, VI, XIII and XIV, XII, X, I, V, VIII, IX, XI, and II.

Scrutiny of the foregoing groupings of skeins, Table I, reveals that the *S* failed to separate reds from greens, yellows from blues, and other hues from their complements; nor did he succeed in segregating one pair from other pairs of complements. The order of his assortments, as well as his skill in matching the groups of skeins with suitable sheets of grey, indicates that his assortments were effected not by hue-discrimination but by brightness-vision. He is hereby revealed to be totally color-blind or an achromate.

In the color-blindness test X was permitted to place each skein as near his eyes as he chose and to take every advantage of the daylight in which he was working. From his behavior in handling the skeins it seemed evident that he was employing *foveal,* as opposed to *peripheral,* vision. The fact that he was not accessible for long-continued and more vigorous investigation prevented his being submitted to campimetry experiments or other techniques designed to differentiate more minutely between his foveal and his peripheral vision.

Also, inasmuch as his sortings were effected wholly by means of brightness-discrimination, the question may be fairly asked whether the distribution of brightness possessed by X is that of daylight or of twilight vision,—a question which naturally bears upon the question of foveal *vs.* peripheral vision. The color-blindness and brightness-matching tests were conducted in a room well-filled with early afternoon sunlight, and no opportunity was offered for submitting him to other tests during twilight or with techniques designed to measure the effects of darkness-adaptation. But in Table I, and in the paragraph which follows it, one finds that the first seven, or the brighter assortments of skeins,—IV, III, VII, VI, XIII, XIV, and XII,—contain exact-

ly 11 skeins exhibiting yellow, and only 6 skeins exhibiting any variety of green, blue-green, or blue; and the second seven, or the darker assortments of skeins,—X, I, V, VIII, IX, XI, and II,—contain almost no skein exhibiting a variety of yellow, and contain a great many skeins which exhibit green, blue-green, and blue. This fact would seem to demonstrate that X's brightness-vision is that of daylight, in which yellow stands out as the brightest hue in the spectrum, and shows a lack of Purkinje's phenomenon, or the relative increase of the brightness of greens, blue-greens, and blues under darkness-adaptation or in twilight vision. Purkinje's phenomenon is thought to be lacking in the fovea, where only cones are present, and to be a function of the rods of the peripheral retina. On the basis of the facts that X apparently used foveal vision and that his seriations of brightness failed to reveal Purkinje's phenomenon, and according to the current theory of the phenomenon, our *S*'s total color-blindness and also his brightness-discrimination were confined, in these experiments, to the cones of his retinas.

We shall next inquire whether or not X's brightness-vision, so useful in the sorting of the skeins, is normal; and if it is not we shall try to measure how defective it is and to ascertain the nature of the defect or defects. Our *S* was next submitted to the binocular, finger-fixation, homonymous-heteronymous images test. He was told to fixate with both eyes the farther of two fingers held opposite his nose at appropriate distances, and was asked to state what he saw. He reported, of course, that he saw the tip of the nearer finger double in the plane of the fixated finger, the far one. While he was still seeing the nearer finger as double, his left eye was shielded by an envelope held in front of it by the *E*, and immediately thereupon the fixated finger, the farther one, seemed to jump to his left. In similar manner, while he was experiencing heteronymous images of the near finger, the *E* shielded his right eye, and in this case the fixated finger seemed to jump toward his left. From the fact that in both of these cases the fixated object, the farther finger, moved to the left, and the additional fact that the right-hand image, that given by the left eye, was the clearer and the more definite of the two images, the conclusion was reached that his left eye was the stronger and that, in binocular use, it dominated over the right. When asked to fixate with both eyes a dot upon the blackboard, while one of his fingers was held nearer his eyes, outside the horopter, and when in this condition his right and left eyes were alternately shaded as in the preceding test, the dot always seemed to move toward his left. When the left was shaded, the dot jumped to the left and upward. The *S*'s right eye, upon examination, was found to be diverged outward,

i.e., temporally, and downward, and was far more diverged from the usual parallel position than was the left eye, besides being also the weaker of the two. One can easily imagine that it was difficult for him to obtain, without his glasses, a good binocular focus and to maintain it for a long period of time.

Having ascertained that X's right eye was weaker than his left, it was in order to attempt to discover really how weak his left eye was and how much stronger than the right it might be. The Snellen test-chart was used for this purpose. It was hung on a wall at a height permitting reading by a person standing erect. The lighting was that afforded by 6 windows on a bright afternoon in spring, and was such as to permit the *E* and four of the five students who engaged in the tests to read the letters on the chart correctly at the usual distances. The *S* stood at points designated on the floor by chalk; these markings represented m.-distances. At each specified distance, he was asked to read aloud those letters which should be easily legible to the normal eye at the given distance. Each eye was tested separately while the other was held shut with one of his hands. From several repeated tests, his vision was determined as being only 1/15 of normal in his right eye and only 1/10 of normal in his left eye. In other words, even if we consider both eyes instead of dealing with each singly, the *S*'s myopia was very great indeed.

Tested for astigmatism, with the Verhoeff chart, the *S* was shown to be astigmatic in both eyes, and the degree of astigmatism for the meridians in which it occurred was very pronounced. To the left eye, the radii numbered 1, 2, and 3 in the upper left quadrant of the chart and the corresponding radii in the lower right quadrant were clearly and definitely visible, while all other radii were almost completely invisible. It should be mentioned here that it was necessary, since he was without glasses in our tests, to have him stand very near the test chart, hung as in the myopia test, for, being 9/10 myopic, he could see nothing on the chart at the usual reading or seeing distance. The right eye enabled him to see clearly and definitely radii 2, 3, and 4 of the upper right quadrant and radii $1\frac{1}{2}$, 2, 3, and 4 of the lower left quadrant, while all other radii were almost wholly invisible to him. His right eye, therefore, seemed to be astigmatic in quadrants the opposite of those which marked the astigmatism of the left eye.

A test of drawing, based upon perceptual discrimination, was next instituted. The *S* looked at a black horizontal straight line 5 cm. long and tried, by referring to it, to duplicate it in a drawing of his own. This was done 10 times, and five other persons were submitted to the same test in order to obtain check

data. The deviations of the drawings, in mm., from the true length of the standard or comparison line were noted; those exceeding the true length were checked as *plus* and those falling below the true length were noted as *minus*. The following Table II presents the 10 trials of the 6 persons.

TABLE II

S	A	B	C	D	E	X
	+ 4.0	+ 1.5	0.0	0.0	+ 2.0	+ 3.0
	+ 5.5	+ 2.0	+ 5.0	− 2.0	− 1.0	+ 10.0
	+ 9.0	+ 1.0	+ 2.0	− 1.0	− 1.0	+ 7.0
	+10.0	− 4.0	+ 2.0	− 2.0	− 3.0	+ 7.0
	+ 8.0	− 3.0	− 1.0	− 3.0	− 3.0	+ 9.0
	+10.0	− 3.0	+ 3.0	− 1.0	− 1.0	+ 14.0
	+ 2.0	− 4.0	+ 1.0	− 3.0	− 1.0	+ 11.0
	+11.0	− 3.0	0.0	− 2.0	− 1.0	+ 10.0
	+ 4.0	− 3.0	+ 2.0	− 1.0	+ 1.0	+ 13.0
	+ 7.0	− 2.0	+ 3.0	+ 1.0	+ 1.0	+ 16.0
Total	+70.5	−17.5	+17.0	−14.0	− 7.0	+100.0
Average	+ 7.05	− 1.75	+ 1.7	− 1.4	− 0.7	+ 10.0
			Ignoring signs			
Total	70.5	26.5	19.0	16.0	15.0	100.0
Average	7.05	2.65	1.9	1.6	1.5	10.0
A. D.	2.55	0.82	1.12	0.8	0.7	2.8

Average for first 5 persons: 2.94

The foregoing Table clearly demonstrates that our color-blind *S* was greatly inferior to 4 other persons in the visual discrimination and drawing of a line 5 cm. long, and was somewhat inferior to the fifth person, A. If his average is compared with that of his 5 competitors taken together, *i.e.*, 10.0 *vs.* 2.94 (even though the latter is weighted with the records of A), it is clearly evident that his inferiority was great. Also his deviation from his average is great, although calculated as a percentage of his average error it is less than that of any other person. This percentage is, for him, 28; for B, it is 31; for A, 36.2; for E, 47; for D, 50; and for C, 59. This fact can be interpreted to mean that his errors, in the 10 trials, were more consistent and constant than those of any other person; were least influenced by chance factors; and, presumably, were most, of those of all *S*s, influenced by defective vision. X's direction or kind of error was, however, not peculiar to himself or to his defective eyesight, inasmuch as A and C made *plus* errors, which fact indicates that they also underestimated the lengths of the lines they were drawing in making them conform to the seen standard. The other *S*s, B, D, and E, erred in the opposite direction, of overestimating the lengths of the lines they were drawing.

All 6 persons were asked 10 times each to draw from memory (that is, with no perceived standard before them except as they

themselves drew each time a line which in the act of drawing may have been a sort of perceptual guide) a straight horizontal line 3 in. or 76 mm. long. The errors were recorded as *plus* or as *minus* errors, in mm., as in the preceding test. The following Table III will illustrate how successful or unsuccessful X was in this task as compared with the other *S*s.

TABLE III

S	A	B	C	D	E	X
	− 9.0	0.0	− 4.0	+ 9.0	− 1.0	− 21.0
	−12.0	− 0.5	− 3.0	+ 1.0	− 4.0	− 5.0
	− 5.0	+ 4.0	+ 2.0	+ 6.0	− 3.0	− 6.0
	− 2.0	+ 5.0	− 1.0	+ 4.0	− 4.0	− 16.0
	− 3.0	+11.0	+ 1.0	+14.0	− 2.0	− 9.0
	− 6.0	+ 4.5	+ 2.0	+ 4.0	− 1.0	+ 1.0
	−12.0	+ 6.0	+ 5.0	+ 5.0	− 2.0	− 7.0
	−13.0	+ 9.0	+ 5.0	+ 3.0	− 2.0	− 8.0
	−22.0	+ 6.0	+ 2.0	+ 3.0	− 3.0	− 13.0
	− 9.0	+ 8.0	+ 2.0	+ 4.0	+ 1.0	− 5.0
Total	−93.0	+53.0	+11.0	+53.0	−21.0	− 89.0
Average	− 9.3	+ 5.3	+ 1.1	+ 5.3	− 2.1	− 8.9
			Ignoring signs			
Total	93.0	54.0	27.0	53.0	23.0	91.0
Average	9.3	5.4	2.7	5.3	2.3	9.1
A. D.	4.36	2.6	1.24	2.62	0.96	4.54

Average for first 5 persons: 5.0

The foregoing Table reveals that X, although inferior to the group of persons competing with him (9.1 *vs.* 5.0), was not inferior to A, nor was he as inferior as he was shown to be in the perceptual-discrimination drawing test. Computing his average deviation as a percentage of his average error, we find that his reactions were just as inconstant and variable as were the reactions of D, and that they were not much more variable than were the reactions of the other 4 persons, regardless of how defective his eyesight was shown to be. It is conceivable that his memory somewhat offsets his visual perceptual deficiency during the act of drawing, reducing his error greatly and making his inferiority not so obvious (especially as his memory of a 3-in. line may be much more accurate than his perception of such a length of line). It is to be noted also that A, who had not been revealed as showing signs of defective vision, was but slightly superior to him in the perceptual drawing test, and in the memory test did as poorly as he. With one exception, all of X's errors in this test were of the *minus*, or overestimation, sort. In the preceding test, they had been *plus*, or underestimation, errors. This change we believe is in itself indicative somehow of the use of memory in this latter test. Two other persons, A and E, likewise made *minus* errors in the memory drawing

test. Of these two, A had made *plus* errors in the perceptual drawing test, while E consistently throughout the two tests made the *minus* or overestimation type of error. B and D, who had erred in a *minus* direction in the first test, erred with underestimations in the memory test. C's errors remain constant in being underestimations of the length of the drawn lines throughout both tests.

In a test of the range of visual perceptual attention, in which an ordinary focal-plane camera-shutter tachistoscope, set with a tension of 1 (G)$\times 1\frac{1}{2}$ (F), was used with 10 stimulus cards bearing geometrical and algebraic signs and symbols each composed of 3 line-strokes and each being .5 cm. square, X gave the results which follow in Table IV.

TABLE IV

No. of Characters on Card (Presented in Irregular Order)	*No. of Presentations before All Characters Were Reproduced Correctly*
1	3
2	2
3	2
4	2
5	9
6	5
7	5
8	7

Judged in two ways, by the average number of items got correctly by the 5 persons who acted as check *S*s, and by the average number of presentations required by these check persons before the unfailing reproduction of all characters was attained, X possessed a visual range approximately one-half of normal. To have been as efficient as this, with an eyesight only 1/10 normal in one eye and 1/15 normal in the other eye, X must possess a most remarkable apperceptive background which can supplement and correct what his retina furnishes during an act of reading. The suspicion that his memory frequently supplements and corrects for him in his acts of perceiving, we have already expressed with reference to his memory-drawing experiences.

In the last test, a test of brightness-discrimination in each eye singly, the brightness-discrimination-box (G. M. Whipple, *Manual of Mental and Physical Tests*, 1914, Part 1, 197) was employed. It carried a 40-watt frosted tungsten lamp in circuit with a 110-volt current. And, although our data are inadequate for presentation in tabular form, X was revealed to be greatly defective in the sort of discrimination required, and his right eye was shown to be inferior to his left eye.

It would be interesting to discover whether or not X's archromasia has resulted from the myopia and astigmatism which are so pronounced in him. Infrequently in the literature one meets the assertion that certain types of color-blindness arise from or are aggravated by the refractive errors which are also present, and that improvement in color-vision results from a correction of the refractive errors. It is to be noted, however, that X was just as color-blind, judging by his own statement and by the testimony of his friends, with glasses as without; and when allowed to place the Holmgren skeins very close to his eyes, and in any position with reference to light that he chose, he remained color-blind nevertheless. Moreover, his brightness-vision was infinitely more acute and sensitive than his color or hue discrimination, which was nil. And, although we were unable to obtain data concerning the heredity of X, we strongly suspect that all four of his defects, namely myopia, astigmatism, temporal and inferior strabismus of rigbt eyeball, and total color-blindness, are due to hereditary influences.

THE INTERPRETATION OF FACIAL EXPRESSION

By Dallas E. Buzby, University of Pennsylvania

Boring and Titchener[1] recently reported a demonstration model for facial expression, especially of the emotions, which renders readily available for class work some of the faces described and pictured by Piderit.[2] By an interchange of a number of mouths, eyes, brows and noses, it is possible to obtain a wide range of facial expressions. Boring and Titchener illustrate their article with 24 typical faces which express most of the principal emotions.

It seemed worth while to determine the amount of scatter one would obtain from judgments on several of the typical faces. The following total expressions were chosen: Anger (not illustrated, but described under 19 by Boring and Titchener); Dismayed (Boring and Titchener No. 11); Horrified (No. 19); Disdainful (No. 22); Disgusted (No. 24) and Bewildered (No. 17). Sheets of paper were prepared, containing six similar lists of descriptive terms, in which were included all of the total expressions given by Boring and Titchener which they described by a single word, *plus* Anger. This list of 18 items will be found in the first column of Table I. The list was read over to the subjects, and they were told that all six lists were identical. The *Ss* were shown the first figure, and were instructed to indicate the name in the first list which best described the expression. Then the second figure was shown, and the *Ss* were instructed to mark the word in the second list, and so on.

The experiment was performed at the University of Pennsylvania during the winter and spring of 1924 on 716 students in the graduate and undergraduate schools. The records of the men and women were separated. The records were further separated on the basis of first course or subsequent course in psychology.

Table I contains the total scatter irrespective of sex or psychological maturity. In the first column are given the descriptive words in the lists. In the subsequent columns are given the number of times each of these terms was judged when the total expression at the head of the column was shown. The numbers

[1]E. G. Boring and E. B. Titchener, A Model for the Demonstration of Facial Expression, this Journal, 34, 1923, 471-485.

[2]Th. Piderit, *Mimik und Physiognomik*, 1867.

in boldface type indicate, in each column, the "correct" judgments.

TABLE I

	Anger	*Dismayed*	*Horrified*	*Disdainful*	*Disgusted*	*Bewildered*
Pleased	169	—	3	—	—	2
Displeased	2	22	5	18	13	1
Stubborn	2	10	1	54	—	—
Attentive	5	330	—	3	—	47
Quizzical	82	134	—	7	—	81
Inattentive	2	9	—	37	—	18
Dismayed	29	**46**	25	11	1	44
Reverential	1	29	—	21	—	28
Affable	46	12	—	—	—	6
Bewildered	111	57	6	4	—	**261**
Amazed	62	16	39	2	—	226
Horrified	41	1	**439**	—	—	—
Raging	22	—	84	—	—	—
Disapproving	6	24	8	94	36	2
Disdainful	50	11	11	**265**	51	—
Anger	**14**	—	12	5	2	—
Contemptuous	65	7	12	152	236	—
Disgusted	7	7	71	43	**377**	—

For the Anger and Dismayed faces the scatter is great, and the number of correct judgments is small. Anger was judged most frequently as pleased, and very frequently as bewildered, quizzical, contemptuous, amazed or disdainful. Indeed, 10 of the 18 possibilities obtained a higher frequency than the correct one. The Dismayed face was judged as attentive with a very high frequency (over 46%), and also quizzical and bewildered were judged more frequently than the correct dismayed.

For all of the other four faces the scatter was much less, and in every case the correct descriptive term was marked with the highest frequency. The percentages of correct judgment are: Horrified, 60; Disdainful, 37; Disgusted, 53; and Bewildered, 37.

TABLE II

	WOMEN		MEN	
	Early	*Late*	*Early*	*Late*
Judgments made	804	864	2202	378
Judgments "Correct"	293	277	708	111
Percentage "Correct"	36.4	32.1	32.2	29.4

In Table II is given the analysis of correct judgments on the basis of sex and of psychological sophistication. In the first two columns are given the results for women,—the first column, marked Early, including the results of those taking the first course in psychology; the second column, marked Late, including those who have already had at least one course in psychology and who, therefore, have a certain degree of psychological sophistication. The results for the men are similarly arranged in the last two columns of the Table.

The results show a slightly greater percentage of right judgments in the case of the women, both sophisticated and unsophisticated. For both men and women there is a slightly greater percentage of correct judgments for the psychologically unsophisticated. None of these differences is great, however, although the tendencies seem to be true without exception.

It seemed worth while to analyse the results on the basis of each of the individual facial features. Inasmuch as 15 of the 18 descriptive terms chosen used the same nose, this analysis was made only on the basis of the mouth, eye and brow. The results will be found in Table III. In every case the values

TABLE III

	Anger	*Dismayed*	*Horrified*	*Disdainful*	*Disgusted*	*Bewildered*
Mouth	36	189	439	413	613	308
Eye	106	47	560	497	664	570
Brow	106	61	560	567	700	576

represent the total number of judgments for the facial expressions in which the correct individual feature was contained. It is apparent that the values for Anger and Dismayed are quite insignificant. The values for all three features for Horrified, Disdainful, Disgusted and Bewildered are all highly significant. It will be noted, however, that the values for the upper part of the face, brow and eye, are much more significant than those for the mouth.

Conclusions. (1) The total facial expressions of Piderit for Anger and Dismayed do not give a high percentage of correct judgments.

(2) The total facial expressions for Horrified, Disdainful, Disgusted and Bewildered give a high percentage of correct judgments, especially when one combines other descriptive terms which closely resemble the expression represented.

(3) An analysis shows that the upper part of the face, eye and brow, is more important for correct judgment of facial expression than the mouth.

(4) Women give a slightly greater percentage of correct judgments than men, and both men and women show a slight decrease in the percentage of correct judgments of facial expression with increasing psychological sophistication.

MINOR STUDIES FROM THE PSYCHOLOGICAL LABORATORY OF CORNELL UNIVERSITY

Communicated by E. B. TITCHENER

LXXII. OBLIVISCENCE DURING SLEEP AND WAKING

By JOHN G. JENKINS and KARL M. DALLENBACH

Ebbinghaus found that forgetting is a function of time:[1] it is very rapid at first, but becomes progressively slower as time elapses. According to the results of his experiments,[2] 41.8% of a series of nonsense syllables, as measured by the Method of Savings, is forgotten after an interval of 20 min.; 55.8% is forgotten after 1 hr.; 64.2% after 8.8 hrs.; 66.3% after 1 day; 72.2% after 2 days; 74.6% after 6 days; and 78.9% is forgotten after 31 days.

The results for the 8.8 hr. and 1 day periods are the least satisfactory; for, as Ebbinghaus himself points out,[3] the difference between the values for these successive periods is not of the same order as the differences between the values of the other successive periods. Between the 8.8 and 24 hr. periods there is a difference of 2.1%, whereas between the 24 and 48 hr. periods the difference is 6.1%; that is, in the later interval of 24 hrs. about 3 times as much is forgotten as during the earlier interval of 15 hrs. "Such a relation," says Ebbinghaus, "is not credible, since, according to all the other figures, the decrease in the after-effect suffers, with increasing time, a marked retardation."[4]

Ebbinghaus did not think the figures credible even under the plausible assumption that sleep—which formed the greater part of the 15 hr. interval and proportionately a much smaller part of the following 24 hrs.—materially retarded forgetting. He sought an explanation among the accidental errors of his experiments and thus neglected the investigation of obliviscence during sleep, the problem that was set by his results. Other experimenters[5] who have traced the curve of forgetting have likewise, in spite of the fact that similar discrepancies were observed by them at the same points, disregarded this problem. We have consequently taken it as the object of the present Study. It is our purpose to compare the rate of forgetting during sleep and waking.

Procedure. Two *Os*, L. R. Hodell (H) and J. S. McGraw (Mc), served throughout the course of the experiments. Both were seniors in Cornell University; neither had had any previous experience in memory experiments. Both worked without knowledge of the nature or object of the problem.

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[1]H. Ebbinghaus, *Ueber das Gedächtnis: Untersuchungen zur experimentellen Psychologie*, 1885, 85-109. English translation by H. A. Ruger and C. E. Bussenius, 1912, 62-80.

[2]*Op. cit.*, Table, 103.

[3]*Op. cit.*, 104.

[4]*Ibid.*

[5]P. R. Radossawljewitsch, *Das Behalten und Vergessen bei Kindern und Erwachsenen*, 1907,1-197. C. H. Bean, The Curve of Forgetting, *Arch. of Psych.*, No. 21, 1912, 1-45. *Cf.* in particular 3 and 42. E. O. Finkenbinder, The Curve of Forgetting, this JOURNAL, 24, 1913, 8-32.

The *Os* were required to learn nonsense syllables which were typewritten on slips of paper and presented, by means of the Spindler & Hoyer memory apparatus, in series of ten. Exposure was made at the rate of 0.7 sec. The syllables were read aloud by the *Os* as presented. The vowels in half of the syllables were pronounced long, and in half short. Pronunciation was differentiated by placing dashes above the long vowels. These marks were distributed irregularly, but were so arranged that the long and short vowels occurred equally often in the various positions within the series. In every other respect Gamble's modification[6] of Müller and Schumann's rules was followed in the preparation of the series.

An adaptation of the Method of Retained Members was employed. The series were repeated by the *Os* until learning had come to the first correct recitation and then, after 1, 2, 4, or 8 hrs., a free reproduction was called for. The amount lost was taken as a relative measure of the amount forgotten.

An absolute measure of the amount forgotten is not to be obtained by this method; for, as repeated experiments have shown,[7] a single recall does not exhaust the amount retained, and the omissions from a free recall cannot consequently be taken as a direct measure of the amount forgotten. Though Finkenbinder found that "the measurement of forgetting made by means of free unaided recall does in some cases correspond somewhat closely with the measurement according to the *Ersparnismethode*,"[8] we should nevertheless have not ventured to use the method of recall if an absolute measure had been thought necessary. Such a measure, however, was neither necessary nor desirable; we were interested, not in tracing the absolute curves of forgetting in sleep and waking, but in comparing the two rates of forgetting; and a comparison could be made as well from a relative as from an absolute measure.

Had we sought an absolute measure, we should have had to use a more complex method, such as the Method of Savings; and we should have had to complicate the procedure by taking the diurnal variations of learning into account. The times of learning and of re-learning, the times of sleep and of waking, must have been varied and interchanged, and the number of experiments would consequently have been greatly increased. Not only did the routine duties of our *Os* forbid this procedure; the additional complications were not warranted by the results to be obtained. The absolute rate of forgetting has been sufficiently well established for waking; the only contribution that we hoped to make to the subject was the comparison of the rate of forgetting during sleep and waking.

The *Os* and the junior author, who acted throughout as *E*, lived in the laboratory during the course of the experiments, and regulated their lives, as far as possible, in accordance with the requirements of this Study. The times of learning at night varied between 11:30 p. m. and 1 a. m.; and the times of learning at day, with single exceptions for each *O* for every time interval, varied between 8 and 10 a. m. The exceptions mentioned were made for purposes of comparison and check, and the times of learning for them varied between 2 and 4 p. m. We were unable to regulate the times of learning more closely, since the time of learning had to be arranged with *O*'s schedule and the length of the time-interval in mind.

The following general instructions were given to the *Os*: "I am going to show you, with the aid of this apparatus, a series of nonsense syllables which I wish you to learn as rapidly as possible. The syllables will appear

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[6]E. A. McC. Gamble, A Study in Memorising Various Materials by the Reconstruction Method, *Psych. Rev. Mon.*, No. 32, 1909, 18 ff.

[7]W. Brown, To What Extent is Memory Measured by a Single Recall? *Jour. of Exp. Psych.*, 6, 1923, 377-382.

[8]*Op. cit.*, 32.

successively and will be visible through the little window in front of you. Give every syllable full and equal attention and pronounce every one aloud as it appears. All the vowels are to be pronounced short unless a long vowel is indicated by a dash over the letter. The syllables are without meaning. Do not attempt to read meaning into them. Do not use a mnemonic system in memorizing. When you can repeat the series in its correct order report the fact to me. Do not on any account repeat the series after you have left the experimental room."

The experimental room was adjacent to the improvised dormitory, and the experiments at night were not begun until *O* was undressed and ready for bed. *O* retired immediately after learning the series; and, as the rule of the sleeping quarters was quiet, distractions and retroactive inhibitions after learning were reduced to a minimum. *O*, after learning at day, went about his routine affairs.

Recall was always made in the experimental room with *O* seated before the apparatus in the position held during learning. During the experiments at night the *O*s were separately waked, after the appropriate interval, by *E*, who was himself waked by an alarm clock, and they were taken one at a time to the experimental room where their reproductions were received. After a few experiments *E* found it difficult to arouse the *O*s and difficult to know when they were awake. The *O*s would leave their beds, go into the next room, give their reproductions, and the next morning say that they remembered nothing of it. Unfortunately, it did not occur to *E* to apply special tests; he judged by the behavior of the *O*s, and assumed that they were awake.

The length of the interval between learning and reproduction was not known to the *O* at night; but during the experiments at day the following sentence was added to the instructions: "You will please report in this room for the reproduction at *x* o'clock,"—the hour depending upon the time of learning and the interval desired. We did not attempt to eliminate this difference between the experiments during waking and sleep. We might have done so by requiring the *O*s to report at the laboratory at every one of the intervals used in the experiment until the particular one de[illegible] was reached, or by informing the *O*s at night that they would be waked for their reproduction at such and such a time; but we thought both methods more objectionable than the existence of the difference. The first was objectionable because it would keep the experiment constantly in the *O*'s mind; because it would only partially accomplish the desired end—if the reproduction was not received at the 1, 2, or 4 hr. interval *O* would definitely know that it would be received at the 8 hr. interval; and because the effect of the blank appointments would probably be more disturbing than specific knowledge of the actual time of the reproduction. The second was objectionable because the attitude of the *O*s toward going to sleep differed according as they knew whether the reproduction would be given after a long or a short interval. The only way of equalizing the experiments during sleep was to keep this knowledge from the *O*s. Since *E* and the *O*s were not together throughout the day, the time of the reproduction could not similarly be kept from the *O*s during the day-time experiments.

After a short practice series the experiments began on April 14, 1923 and continued until June 7. With but few exceptions, on days when the *O*s were indisposed, experiments were conducted every night and every day. Particular care was taken to see that the *O*s' physical condition was constant, and the experiments were postponed whenever it was apparent that a fluctuation had occurred.

The time-intervals between learning and reproduction were varied at haphazard. We planned to have 8 experiments for every interval during both sleep and waking. The series were completed for the experiments during sleep; but H failed to complete two of the waking series, and Mc failed to complete one.

Results. The gross results of the Study are shown in Table I, which gives separately for every experiment with each *O*, and for every time-interval during sleep and waking, the number of repetitions required for the first correct recitation and the number of syllables given correctly in the subsequent reproduction.

TABLE I

Number of Repetitions for the First Correct Recitation and Number of Syllables Correctly Reproduced

	INTERVAL															
	1 hr.				2 hr.				4 hr.				8 hr.			
	Sleep		*Waking*		*Sleep*		*Waking*		*Sleep*		*Waking*		*Sleep*		*Waking*	
O	Rep.	Syll.	Rep.	Syll.	Rep.	Syll.	Rep.	Syll.	Rep.	Syll.	Rep.	Syll.	Rep.	Syll.	Rep.	Syll.
	23	9	23	5	25	6	16	5	15	5	21	4	31	2	20	1
	18	9	23	6	15	3	14	2	20	5	17	3	29	3	17	0
	22	7	22	4	13	5	17	3	16	5	15	2	32	7	28	1
H	14	4	21	4	18	9	26	3	16	5	15	1	20	9	22	0
	21	5	17	2	14	5	20	2	24	5	15	2	16	3	24	0
	19	6	19	4	10	6	12	2	23	5	18	2	12	7	17	0
	18	10	22	6	22	4			23	5	11	3	22	5	16	0
	20	7	14	4	14	5			15	7			16	8	15	1
	36	8	33	7	28	6	17	2	20	8	26	2	22	5	18	1
	29	7	31	3	23	5	20	2	19	5	25	2	28	7	16	0
	24	6	20	3	23	5	16	2	19	5	25	2	19	4	20	2
Mc	22	8	26	5	22	5	13	5	22	5	23	2	24	6	19	1
	18	7	17	5	21	5	17	5	22	7	16	2	20	7	20	3
	16	8	18	6	23	5	16	5	25	4	21	1	20	5	19	1
	17	5			20	7	20	2	22	7	13	2	25	4	22	3
	18	7			19	5	19	4	22	5	15	4	20	8	14	0

In computing the results only those syllables that were correctly reproduced were counted; partial credit was not given. The associative fusions,[9] such as the reproduction of *shog* and *riv* as *shiv*, of *ched* and *jeb* as *cheb*, of *baf* and *jop* as *bap*, of *juk* and *fuv* as *juv*, of *jev* and *jif* as *jef*, etc.; and the partial successes, such as the report of *tich* for *lich*, of *kay* for *kaq*, of *koj* for *foj*, of *tum* for *fum*, of *zuk* for *ruk*, etc.; were counted as incorrect.

Out of a total of 571 syllables recalled by the *O*s only 38, or approximately 7%, were partial reproductions. Each *O* gave 19, which were distributed among the time intervals and between sleep and waking as shown in Table II.

TABLE II

Distribution of Partial Reproductions

		INTERVAL				
O	After	1 hr.	2 hr.	4 hr.	8 hr.	TOTAL
	Sleep	1	3	3	1	8
H	Waking	1	3	5	2	11
	Total	2	6	8	3	19
	Sleep	1	2	4	1	8
Mc	Waking	2	1	4	4	11
	Total	3	3	8	5	19

[9] G. E. Müller and A. Pilzecker, Experimentelle Beiträge zur Lehre vom Gedächtnis, *Zeits. f. Psych. u. Physiol. d. Sinnes.*, *Erg. Bd.* 1, 1900, 159-165; also G. E. Müller, Zur Analyse der Gedächtnistätigkeit und des Vorstellungsverlaufes, *Op. cit.*, *Erg. Bd.* 8, 1913, 497-504.

The total number of partial reproductions is so small, and the difference in their distribution after intervals of sleep and of waking is so slight,—so slight, indeed, that it might be due to chance (2, 6, 8, and 3 cases and 3, 3, 8 and 5 cases occurred respectively for H and Mc at the 1, 2, 4, and 8 hr. intervals; and 8 and 11 cases occurred respectively for both *O*s after intervals of sleep and of waking),—that their omission, we felt, was justified, particularly as we were concerned in this Study only with the relative measure of forgetting.

A summary of the gross data shown in Table I appears in Table III, which gives, for each *O* and for every interval, the averages and the mean variations of the number of repetitions necessary for the first correct recitation and of the number of syllables correctly reproduced, as well as the total averages and mean variations for the sleep and the waking series.

These data show that there is a marked difference in the rate of forgetting during sleep and waking. On an average, as is shown in Table III under the caption "Totals," more than twice as many syllables are reproduced by both *O*s after intervals of sleep than after intervals of waking. On an average, 5.8 and 6.0 syllables were reported by H and Mc respectively in the sleep experiments, and only 2.4 and 2.8 syllables in the waking experiments. The superiority of the reproductions after intervals of sleep is also shown at every one of the experimental intervals. At the end of 1 hr. of sleep an average of 7.1 and 7.0 syllables was reproduced, whereas an average of only 4.4 and 4.8 syllables was reproduced after a like interval of waking. At the 2 hr. interval, both sleep and waking, there is a corresponding drop in the averages of each *O*; but the reproductions after sleep still maintain a decided superiority. This superiority becomes more and more pronounced as the length of the intervals increases; for the average number of reproductions for both *O*s continues to decline at the 4 and 8 hr. intervals of waking, whereas after like intervals of sleep the average of the 2 hr. interval is maintained. Indeed, in

TABLE III

Averages and Mean Variations of the Number of Repetitions Necessary for the First Correct Recitation, and of the Number of Syllables Correctly Reproduced

O		INTERVAL 1h. *Sleep* Rep.	Syll.	1h. *Waking* Rep.	Syll.	2 hr. *Sleep* Rep.	Syll.	2 hr. *Waking* Rep.	Syll.	4 hr. *Sleep* Rep.	Syll.	4 hr. *Waking* Rep.	Syll.	8. hr. *Sleep* Rep.	Syll.	8. hr. *Waking* Rep.	Syll.	TOTALS *Sleep* Rep.	Syll.	TOTALS *Waking* Rep.	Syll.
H	Av.	19.4	7.1	20.1	4.4	16.4	5.4	17.5	2.8	19.0	5.3	16.0	2.4	22.2	5.5	19.9	0.4	19.5	5.8	18.5	2.4
	m. v.	2.1	1.6	2.6	0.9	3.9	1.2	3.6	0.8	3.5	0.4	2.3	0.8	6.3	2.2	3.6	0.4	3.9	1.6	3.5	1.4
Mc	Av.	22.5	7.0	24.1	4.8	22.4	5.4	17.2	3.4	21.4	5.8	20.5	2.1	22.2	5.8	18.5	1.4	22.1	6.0	19.8	2.8
	m. v.	5.3	0.7	5.8	1.2	1.9	0.6	1.8	1.4	1.5	1.2	4.4	0.4	2.5	1.2	1.9	.9	2.8	1.1	3.6	1.4

the case of Mc, the averages at the 4 and 8 hr. intervals increase slightly; and similarly, for H at the 8 hr. interval, the average is slightly greater than at the 2 hr. interval.

Comparative curves are shown in Fig. 1, in which the average number of syllables reproduced after the various time-intervals is plotted separately for each *O* and for the sleep and waking experiments. The curves of the waking experiments take the familiar form: a sharp decline which becomes progressively flatter. The form of the curves of the sleep experiments, however, is very different: after a small initial decline the curves flatten and a high and constant level is thenceforth maintained.

These results are not due to a difference in the depth of learning; for, as far as we were able to determine, this was constant for every experiment: learning was just brought to complete mastery; repetitions ceased at the first correct recitation. The statement that the depth of learning

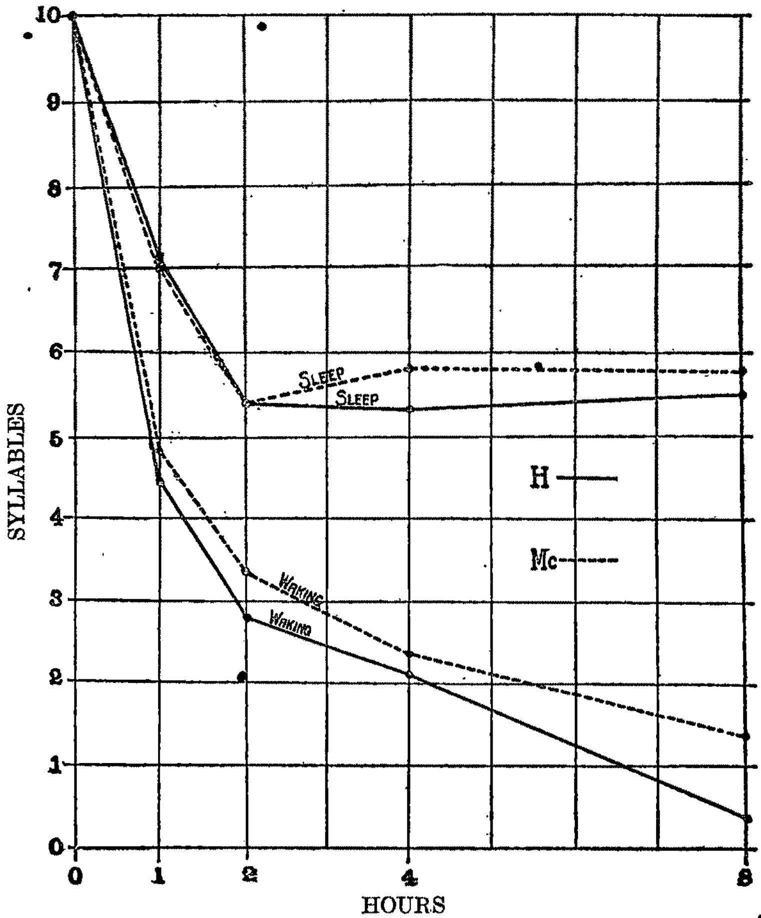

Fig. I. Average Number of Syllables Reproduced by each *O* after the Various Time-Intervals of Sleep and Waking

was constant holds, even if the number of repetitions be taken as the criterion, for the differences between the general averages of the number of repetitions at night and at day are too small to be significant. H averages 1 repetition more at night than at day, and Mc averages approximately 2 more. That these differences are not significant is shown by a comparison of the data for the various intervals. The general averages conceal facts which in this connection are important; the repetitions at the 1 and 2 hr. intervals for H, and at the 1 hr. interval for Mc, average less at night than at day. At the 1 hr. interval, the average for H is 19.4 at night and 20.1 at day, and for Mc, 22.5 at night and 24.1 at day; at the 2 hr. interval, the average for H is 16.4 at night and 17.5 at day. Nevertheless, in spite of the difference in the number of repetitions in favor of the experiments at day, a greater number of syllables were, as we have seen, reproduced after sleep than after waking. If the small differences were significant, opposite results should have been obtained. In view of these facts, we feel justified in re-affirming that our results are not due to a difference in depth of learning, but are due to a real difference in the rate of forgetting during sleep and waking.[10]

The difference in the rate of forgetting during sleep and waking is sufficiently large to account for the variation in Ebbinghaus' investigation. His figures for the 8.8 hr., 1 and 2 day intervals are not only "credible" but are confirmed by this Study.[11]

The constancy of the experimental conditions, or of *O*'s attitude, or of both, was greater after intervals of sleep than after intervals of waking; for, as is shown in Table III, the mean variation from the average number of syllables reproduced after sleep is relatively smaller for H, and both relatively and absolutely smaller for Mc, than the mean variation during waking. What effect the differences in the experimental situation had on our results, we are unable to say; but we feel certain that these differences are inherent in the problem and method.

A few subsidiary experiments were conducted with Dr. H. G. Bishop (B), instructor in psychology, and with W. F. County (C), an untrained *O*.

(1) The experiments with B were conducted to see how closely the results of an experienced *O* would agree with those of H and Mc. B's data, which appear in Table IV, are confirmatory of our general results; after 1 hr. of sleep B reproduced 9 syllables correctly, whereas he reproduced only

TABLE IV

1 hr.				8 hrs.			
Sleep		*Waking*		*Sleep*		*Waking*	
Repetitions	Syllables	Repetitions	Syllables	Repetitions	Syllables	Repetitions	Syllables
35	9	43	6	37	8	43	3

6 after 1 hr. waking; after 8 hrs. of sleep he reproduced 8 syllables, whereas he reproduced but 3 after a like interval of waking. The superiority of the reproductions after intervals of sleep is marked. It should also be noted, as confirmatory of what we have said regarding the number of repetitions required for learning, that B required fewer repetitions at night than at day.

(2) Since the regular *O*s might at night have given their reproductions, sometimes at any rate, in a hypnagogic state, experiments were made with

[10]The advantage of night-study thus becomes evident. Little is forgotten during sleep, and, on waking, the learner may take up the task refreshed and with renewed vigor.

[11]The discrepancies in Radossawljewitsch's investigation (*op. cit.*) may likewise be explained by our results.

C in the hypnotic state. The same procedure was followed in these experiments as in the waking series. When learning had come to the first successful recitation, C was brought out of the hypnotic state and told when to return to the laboratory. Intervals between learning and reproduction of 2, 4, 8 and 24 hrs. were used. When *O* returned he was again hypnotised, and was then asked to reproduce the series that he had last learned. At intervals of 2, 4 and 8 hrs. he reproduced the series correctly, and after the 24 hr. interval he reproduced 8 syllables of the 10. These experiments were performed during the daytime; and, except for the 24 hr. interval, the hours intervening between learning and recall were taken up with routine affairs. Approximately 8 hours of the 24 hr. interval were spent in sleep.

Though these results agree with those of Wienholt,[12] who found that "memory was intensified in the magnetic sleep," too few experiments were conducted with C to permit us to draw conclusions regarding forgetting during hypnosis, particularly as our results stand in disagreement with those of Beaunis,[13] who "found no essential difference in this respect between hypnosis and waking life," and with those of Dessoir,[14] who found "that memory is weakened in deep hypnosis." Our results, however, indicate, so far as they go, that a similar condition of retention and forgetting exists in the hypnotic and in normal sleep.

The results of our Study as a whole indicate that forgetting is not so much a matter of the decay of old impressions and associations as it is a matter of the interference, inhibition, or obliteration of the old by the new.

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[12]Quoted by A. Moll, *Hypnotism*, A. F. Hopkirk's translation, 1910, 122.

[13]*Ibid.* No references are given by Moll, so that we are not certain from what source this quotation is drawn. As the statement attributed to Beaunis appears in every edition of Moll's work, it is certain that Beaunis' study appeared before 1889,—the year of the first edition of Moll's *Hypnotism*,—and therefore before Ebbinghaus' standardization of the methods and technique of memory investigation.

Before 1889 Beaunis had published, according to Dessoir's *Bibliographie des modernen Hypotismus* (1888), the following articles dealing with hypnosis:

(1) Sur deux phénomènes produits pendant le somnambulisme provoqué. *Compt. rend. Soc. de biol.*, Ser. VII, 1, 1884, 362.
(2) Effet de la suggestion sur les actes organiques. *Op. cit.*, Ser. VIII, 1, 1884, 513.
(3) Études physiologiques sur le somnambulisme provoqué. *Rev. méd. de l'est*, 22, 1885, 674.
(4) L'expérimentation en psychologie par le somnambulisme provoqué. *Rev. philos.*, 20, 1885, 1, 113.
(5) Le somnambulisme provoqué. Études physiologiques et psychologiques, 1886.
(6) Un fait de suggestion mentale. *Bull. Soc. de psychol. physiol.*, 1, 1886, 39.
(7) Sur la spontanéité dans le somnambulisme. *Rev. philos.*, 23, 1887, 444.

Moll's quotation is probably drawn from the fourth article, where Beaunis says: "La mémoire (fixation des impressions) ne m'a pas paru être différente de ce qu'elle était á l'état de veille." Here, as the quotation shows, Beaunis is writing only of impression, a point upon which our results have no bearing. We have found that recall is intensified or augmented in hypnosis; and upon this point our results appear to corroborate Beaunis, who says (p. 16): "Dans la mémoire, il y a deux choses: il y a d'abord la facilité avec laquelle une impression quelconque se fixe dans le cerveau et il y a ensuite la facilité avec laquelle cette impression, une fois fixée, revient à la conscience ou autrement dit, ce que les Anglais appellent la *recollection.* Cette seconde aptitude est évidement augmentée dans le sommeil hypnotique. Pour la première, je ne saurais être aussi affirmatif."

[14]*Ibid.* Dessoir's results were communicated privately to Moll. As Moll merely summarizes them we are unable to draw a comparison between Dessoir's experiments and our own.

BOOK REVIEWS

RIVERS, W. H. R., *Conflict and Dream*. New York, 1923, pp. xi + 195. *Psychology and Politics*. New York, 1923, pp. vii + 181.

In these two posthumous volumes certain of the literary remains of the late W. H. R. Rivers have been collected and arranged for publication, with slight alterations and a few notes supplied by his friend G. Elliot Smith. The first volume deals in simple and often repetitious terms with Rivers' modification of the Freudian interpretation of dreams. Documented as it is with a number of dreams recorded both by the author and by certain of his former patients, a good case is made out for the thesis that dreams are not wish-fulfillments but are the expressions of some conflict in waking life. The peculiar character of the dream is not attributable, as Freud maintains, to a latent content derived from infancy and distorted by censorship; instead, the conflict is found to be a present one, although the content of the dream is that of an infantile, or at least of an earlier, level of development. Rivers denies the Freudian censor, and explains the grotesque and symbolic features of dreams by the process of regression to an infantile mode of experience. This transformation, which sleep imposes, is made to account for Freud's processes of condensation, displacement, plastic representation, and secondary elaboration; all these features of disguise, instead of being the effects of censorship, are held to be but a necessary consequence of the infantile mold in which the dream is cast. Although all dreams exhibit this feature of regression, there are also certain means of expression left open at the higher levels which find their way into the dream experience; because special systems of ideas remain active even after sleep has put other recent experiences out of action.

These recent experiences are more prevalent in dreams of light than of deep sleep. In the latter case the regression is more pronounced, and the content is more infantile. Although the conflict which supplies the motive is the same in both cases, the dreams of deep sleep are more distorted or transformed, and for this reason less likely to be remembered on awakening. The nightmare, which furnishes a possible exception to this rule, is remembered because of the greater affect which attaches to it.

The nature of the conflicts which determine dreams is by no means exclusively, or even generally, sexual; for although sex is an important motive, the instinctive nature of man furnishes many others.

A word should be added regarding the author's objection to the Freudian method of free association as applied to the interpretation of dreams; it is a "pure assumption," he writes, "that the experience to which free association leads has the importance universally attached to it by the psychoanalytic school."

The author's own method of analysing his dreams utilizes the state of drowsiness prior to full awakening for a review, more or less critical, of the dream-experience. Rivers found in his own case a special aid toward distinguishing the dream-content from that of waking life in the fact that images abounded in his dreams whereas he rarely experienced any when awake. A suggestion, which would fall in line with Rivers' general position, leads one to think that dream-imagery may be largely of the "eidetic" type recently distinguished by Jaensch and his school as being a chief characteristic of infantile experience. This suggestion might bear fruits if it were to be employed in the analysis of dreams.

The second volume under review consists of six addresses by Rivers, two notes by G. Elliot Smith, and a sympathetic appreciation of the author and his scientific work by C. S. Myers.

The first three addresses were prepared and delivered by Rivers as candidate of the Labor Party for representation of the University of Lon-

don in the House of Commons. They deal in a simple way with the application of instinct and conflict to social problems, and raise, among others, the significant question how far the immense communities of modern times can reach the harmony of social action attained by many small communities without a leader. The supplementary essays deal with *Socialism and Human Nature*, *Education and Mental Hygiene*, and the *Aims of Ethnology*. To the last of these G. Elliot Smith appends a note, in which he draws the consequences for psychoanalysis of Rivers' thesis that the similar beliefs and customs of different peoples are not attributable to uniformity of mind. According to Smith this new teaching destroys the foundations of belief in the reality of "typical symbols" which Freud and Jung have employed in support of their doctrine of instinct and the "collective unconscious."

Taken together, these two volumes, obviously intended for popular consumption, may be expected to do much good. Both Rivers' sanity and the clear and simple method of his exposition should go far to correct the extravagances of much of the popular psychoanalytic writing of our times. From his rich store of ethnological and psychobiological knowledge the author has been able to point the way to a more scientific attitude and interpretation.

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L'année psychologique, vingt-troisième année (1922). Ed. by H. Piéron. Paris, Félix Alcan, 1923. pp. xiii, 644. Price 40 fr.

With this volume the bibliographical part of the *Année* becomes definitely incorporated with French scientific bibliographies. The *Confédération des Sociétés Scientifiques*, which has taken charge of *L'Année biologique*, has also subsidized the *Année psychologique*. The committee on abstracts of the *Année biologique* has abolished its section on 'mental functions,' and the *Année psychologique* is now responsible for its publication. This arrangement is a fortunate one for psychology. For it not only insures financial support of the *Année*, but it also brings to the notice of psychology articles of interest from a dozen periodicals which heretofore have not regularly been represented in the psychological bibliographies. The *Analyses bibliographiques* of this volume contain some 750 abstracts and, in addition, the tables of contents of all important German psychological and psychopathological journals from 1914 to 1920. The bibliographical record broken by the war is thus finally repaired, and the *analyses* will now cover a single (from Oct. to Oct.) year.

The *mémoires originaux* consist of an experimental investigation and theoretical discussion of the Fechner-Benham colors (*Le mécanisme d'apparition des couleurs subjectives de Fechner-Benham*) by H. Piéron; an experimental study of the concomitant external inhibitions during the impression of meaningless words (*Les inhibitions externes concomitants au cours de la fixation des images*) by M. Foucault; an experimental study of the transfer of visual and kinaesthetic perceptions of extent (*Contribution expérimentale à l'étude des phénomènes de transfert sensoriel*) by Mme H. Piéron; researches on memory of forms (*Recherches sur la mémoire des formes*) by G. Dwelshauvers; and a psychotechnical study of certain tests of special abilities (*Étude psychotechnique de quelques tests d'aptitude*) by Mme H. Piéron.

Piéron has, in the interest of explanation, determined in great detail the conditions under which the Fechner colors appear, and has formulated a general theory in a pair of laws. The basis of his explanation is that the visual psychophysical processes are not excited at the same rate, and that when black and white discs are rotated at the proper rates the release of the various chromatic processes occurs at different times, thus producing the successive colors. Despite the fact that Piéron has carefully reviewed the earlier literature, he has unfortunately overlooked Bagley's study

(this Journal, 13, 1903), which anticipated not only some of his facts but also the fundamental idea of his theory. Foucault means by the phrase 'concomitant external inhibition' what is ordinarily called 'distraction.' He reports experiments which show that, in the learning of nonsense syllables, the inhibiting effect of a beating metronome disappears after an interval which varies with the subject, and then becomes facilitative. He fails to mention either Geissler or Cassel and Dallenbach, who found similar results in other fields. Mme Piéron's first paper is a comparison of the average errors in judging visual and kinaesthetic (both active and passive) extents and in transferring the judgment from one modality (or, if kinaesthetic, from one hand) to the other. The results are purely quantitative, and too numerous and complex for a brief review. Their psychological significance is not discussed. Her second contribution is a report of the scores and intercorrelations of six tests: Claparède's permutations, Claparède's memory test, Binet's 'absurd phrases,' Whipple's formation of words, Woodworth and Wells' analogies, and Rybakoff's test of simple changes. Dwelshauvers' investigation, which may be regarded as a continuation of one reported by Piéron two years ago, is of the memory of drawings either of meaningless figures or of geometrical forms variously combined. The method was similar to Philippe's. The author attempts a quantitative analysis of his results, and tends to explain them in terms of the unconscious.

The *Notes et revues* comprise two articles by Piéron, the one a discussion of the difference between tests of special abilities and tests which attempt to measure mental development, the other a critical review of the experimental literature of auditory localization; and a third by I. Meyerson, on primitive mentality, àpropos of Levy-Bruhl's book.

The *Chronique* is, as usual, a useful summary of the psychological events of the year. H. P. W.

Goethe als Psycholog. By Johannes von Kries. Philosophie und Geschichte, Nr. 5. Verlag von J. C. B. Mohr (Paul Siebeck), Tübingen, 1924. pp. 52. Price GM. 1.

This paper is a reprint, with notes, of an address delivered before the *Goethe-Gesellschaft* im Weimar in 1919 and published in their *Jahrbuch* for 1920 under the title of *Goethe als Naturforscher*—the topic originally assigned to von Kries. This double title is not without significance; for, though the author has felt that the narrower one was more appropriate to the partial treatment which the limits of an address made necessary, the reader will find that the first third only of the paper has to do with Goethe as a psychologist and the remaining and more important two-thirds with Goethe as man of science and philosopher.

Every poet, except perhaps the pure lyricist, has of course to be a kind of an empirical and practical psychologist; and when he is interested, as Goethe was, in the precise description of persons and in the characteristics of the sexes, of groups, classes and nations, and of the life-stages of man, we are apt to find him forming empirical generalizations which are at least quasi-scientific. Of such generalizations Goethe's works have their full share, and not a few of them have a very modern sound—evidence of how little empirical and practical psychology has advanced in a century!

The last two-thirds of the address deal with Goethe's psychology only in the sense that they set forth how his own mind worked in his scientific observing and the epistemological conceptions under guidance of which he studied nature. For Goethe the way to arrive at truth is to use the senses carefully and skilfully—to observe, especially to *look at* things—and then let the mind take its own safe and natural course in the apprehension of them. In his own observations he was especially interested in the discovery of types and uniformly changing series of phenomena, which are apprehended in that immediate way. For speculation apart from experience he had no

use, and even of theory he was suspicious. Theories, he thought, we "commonly the hasty output of an impatient spirit, anxious to be rid the phenomena." For mathematics also he had no great liking; and at point, perhaps, is the contrast between his attitude and that of mode scientific workers more sharp than with reference to the ideals which ha come in through the increased use of that instrument of mechanic precision; Goethe's attitude was a "naive-sensory" one, while that of t modern man of science is the "abstract-mathematical,"—all of whi means, however, no disparagement of Goethe's exceptional scientific gi nor any lessening of the value of his discoveries and prophetic conjectur but simply that science has advanced since his time.

Goethe also was by nature little interested in epistemological criticis with its subjective orientation; his own mind faced the other way—towa what is objective, active, ethical. "How can one learn to know himsel Through reflection never; but rather through action. Try to do thy du and thou knowest at once what concerns thee." With this is connecte on one side, his reliance upon direct observation. We are equipped wi all that is necessary for learning all of nature that we need to know. Natu "hides no secret that she does not somewhere lay open naked before t eyes of the attentive observer;" and what she does not want to reveal compulsion can force from her. There are therefore for Goethe limits investigation which it is foolish to try to go beyond, matters "of which he once said, "the deity knows no more than I." The same active-ethic attitude is, on the other side, the key to another fundamental aspect Goethe's scientific thought: its essentially religious character. Before hi shone, as a supreme and not unattainable hope, the belief that throu science God and Nature might reveal themselves to the faithful seek For this end he observed precisely, reverently and with open mind. Wi this also, perhaps, was connected the double feeling for nature so chara teristic of him: the aesthetic delight in all her rich and colorful yet order variety; and his deep reverence for her. "Everything else," he said, more or less plastic, lets us do more or less with it. Nature, however, stan no fooling; she is always true, always serious, always unyielding, alwa right; the errors and mistakes are Man's." What finer incentives f scientific work could there be than these two: delight and reverence!

E. C. S.

(1) *The Depths of the Universe.* By GEORGE ELLERY HALE. Ne York, Charles Scribner's Sons, 1924. pp. xv+98; forty-four illustration Price $1.50.

(2) *The A B C of Atoms.* By BERTRAND RUSSELL. New York, E. Dutton & Co., 1923. pp. 162. Price $2.00.

To the psychologist seeking the stimulus of an excursion to the frontie of sciences remote from his own these two little books may be hearti commended.

(1) The first, by the Director of the great astrophysical observato on Mt. Wilson, California, is a reprint of three articles which have appear during the past two years in *Scribner's Magazine.* They tell engaging and authoritatively of the methods by which astronomers have recent been plumbing the depths of space and of the results obtained, of Ba nard's dark nebulæ, and of the magnetic peculiarities of the sun-spot

(2) The second presents, with the ease and clarity of a master, sketch of the recent advances in the border-land between physics ar chemistry—electrons, the hydrogen atom, the theory of quanta, radi activity and the like—so far as they can be made comprehensible to tl layman, which under Russell's hand means to a very interesting degre not omitting a suggestion of the philosophical questions which these thin raise. The work is a model of what popularization at its best may b

E. C. S.

NOTES

Dr. Spencer's Experiment on Retroactive Inhibition[1]

Dr. Spencer has discussed the results of his own very interesting experiment largely in terms of the critical light which he assumes they throw upon an experiment of mine.[2] Before commenting upon Dr. Spencer's investigation I should like to state that I am in thorough agreement with him upon at least one important point: further work must be done before we can accept as of general significance my finding that the degree of retroactive inhibition is independent of the temporal position of the retroactive interpolation.

In his own experiment, however, Dr. Spencer has failed to isolate the problem upon which I, taking my cue from Experiment 34 of Müller and Pilzecker,[3] made an attack. The plan of the Spencer experiment is justifiable in itself; but it most assuredly is not analogous to the Müller and Pilzecker experiment nor to mine. Dr. Spencer measured the retroactive effects of interpolated learning placed 9 sec. after the study of an original list by comparing recall under this condition with recall after a similar interval not spent in learning a second list. He measured the retroactive effect of interpolated learning placed 20 min. after the study of an original list by comparing recall under that condition with recall after an equal interval of time not spent in learning an interpolated list. He then compared the degrees of inhibition found under the two pairs of conditions, and from this comparison concludes that the more remotely interpolated learning has a less retroactive effect than the more immediate. It is quite as likely that his results are due to the fact that the retroaction of his experiment was measured at two different temporal positions after original learning as to the fact that two temporal positions were employed for the interpolated learning. In my experiment there was a total interpolated period of *constant* length within which the interpolated learning was placed in varying temporal positions.

Edward S. Robinson
The University of Chicago

Summation Tones: A Correction

In a previous study of difference tones obtained from tunable bars (this Journal, 33, 1922, 386) I wrote: "We might add that f^2 combined with $f\sharp^2$, and also $f\sharp^2$ combined with g^2, which are the lowest bars upon our instrument, yield unusually clear summation tones. The tone of the first combination can readily be heard as higher than that of the second."

The tones in question are not summation tones. If they were, the combination of f^2 and $f\sharp^2$ would give a *lower* tone than $f\sharp^2$ and g^2, since f^2 has a lower frequency than g^2. Repeated observation, however, has confirmed the original statement of fact that the tone from the first combination is *higher* than that from the second.

At the present time I cannot explain the observation, but I suggest two possibilities. (1) The so-called summation tones may be difference tones of a higher order than the first, which were misjudged by an octave. (2) The so-called summation tones may be difference tones generated by higher partials in the clang.

Paul Thomas Young
University of Illinois

Errata

In the article, "A Further Study of Revived Emotions," which appeared in this Journal for January, 1924, the following corrections should be made. On page 115, line 15 from top, '74%' should read '7.4%'. On the same page, between lines 18 and 19 from top, insert: "Of the cases where fear was recalled from position 3, 35% had the longest time of recall and 5% the shortest."

M. F. Washburn
Vassar College

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[1] This Journal, 35, 1924, 466.

[2] *Psychol. Monog.*, 28, 1920, 36-40 and 53.

[3] *Zeits. f. Psychol. Ergbd.*, 1, 1900.

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